Agency, Structure and Realism in Language and Linguistics

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Declaration

I hereby declare that this thesis has not been and will not be, submitted in whole or in part to another university for the award of any other degree.

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Summary

This thesis considers the scientific status of linguistics and the historical and contemporary attempts to view linguistics as closely aligned to, or one of, the natural sciences. Such attempts share certain common features that make up what is identified here as the 'Formalist Attitude'. The question 'what is a language?' is central to the discussion of the scientific status of linguistics, so a central task of the thesis is to show how answers to this question display the features of the Formalist Attitude. In particular it is shown that attempts to constrict the theoretical purview of linguistics around a view of language that sustains claims to natural scientific status fail to account for the social ontology of language and the role of speakers within the creation and reproduction of language. A consequence of this failure is an inability to explain important language phenomena such as language change, arbitrariness and knowledge of language, which the alternative conception of language defended here successfully accounts for. 'Language' is best seen as a power of speakers to communicate with one another, a view which emphasises the motivated, social, reproductive and transformative aspects of actual speech. The negative and positive arguments jointly defended, support the view that linguistics, considered with respect to its object of knowledge, methodology and ability to offer explanations and predictions, is not akin to natural science but should be considered a social science.

Besides historical contextualisation of the problem, the thesis looks at current trends, such as cognitive and integrationist linguistics, that are broadly consistent with its criticisms and conclusions. The purpose of the thesis then is twofold; to identify, explain and criticise a problematic and influential tradition within linguistics and then to provide some Lockean underlabouring for contemporary linguistics that will be valuable to linguists and philosophers.

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1.1 Introduction

In 1929, Edward Sapir saw the emerging science of linguistics as providing a model to disciplines such as psychology and philosophy; 'For all of them', he argues, 'linguistics is of basic importance: its data and methods show better than those of any other discipline dealing with socialized behaviour the possibility of a truly scientific study of society' (Sapir 1929, p.207). The emphasis on the 'truly scientific study of society' is indicative of a persistent concern among linguists to establish the scientific status of their work; a concern which marks the history of modern linguistics, from its development in the early nineteenth century and on to contemporary schools of thought. Wasow confirms this in his paper 'The Wizards of Ling', where he claims, 'The concern of modern linguistics with being scientific is nothing short of obsessive' (Wasow 1985, p.486). He goes on to list prominent linguists and their accompanying claims for the scientific status of linguistics: Lyons, Saussure, Bloch, Chomsky and others. This preoccupation is perhaps understandable given that linguistics seems to occupy a liminal place within the pantheon of sciences, between the natural and the social sciences. For example, Koerner claims that, 'linguistics is a social and human (and not exclusively historical) science, by virtue of its object of investigation, and an exact (though not necessarily mathematical) science owning to its methodology' (Koerner 1978, p.25). Sapir appears to concur:

> It is precisely because language is as strictly socialized a type of human behaviour as anything else in culture and yet betrays in its outlines and tendencies such regularities as only the natural scientist is in the habit of formulating, that linguistics is of strategic importance for the methodology of social science (Sapir 1929, p.213).

Lass makes this claim,

> Linguistics up to now has been the descriptive human science *par excellence,* and the steady increase of its descriptive and systematising power has been one of its glories- as well as the envy of other disciplines, and one of the sources of its influence on anthropology, sociology and other fields (Lass 1980, p.126).

Indeed it would be fair to say that linguistics is seen as the most social of the natural sciences and the most natural of the social sciences.

However this consensus hides tensions concerning theoretical and methodological commitments. To begin with the distinction between the natural and social sciences requires developing an account of them and cannot be taken for granted. Further, it is interesting why the scientific status of linguistics is a live topic of debate in a way that is not the case for paradigmatic disciplines within either the social or natural sciences, such as sociology or physics. In addition, it is worth pausing to consider the way linguistics is often seen by linguists as demonstrating to the social sciences how to be more 'scientific' (that is, more like the *natural* sciences). These features of the debate about the scientific status of linguistics opens up in interesting ways the expectations raised when a discipline claims to be a science. To be a science can mean that a discipline has the ability to predict phenomena within its purview, to explain by reference to general laws, to employ a particular methodology or simply to be similar enough in its practices and procedures to a paradigmatic science to warrant the characterisation of 'scientific'.

My aim in this chapter is to unpack assumptions about science that shape current debate within linguistics in order to show that there are a number of pressing problems in associating linguistics with the natural sciences, especially with the desire to strongly delimit language as an object of enquiry. I will examine both the history and the prevalent schools of linguistics in an attempt to throw light on the following questions: what sort of a science is linguistics? What is it about the nature of language which makes the question of the scientificity of linguistics a matter of consistent attention for many linguists and philosophers? Why is it that the study of language is seen as being particularly amenable to a natural-scientific approach? Is this view justified?

1.2 Linguistics as a natural science: An overview

To address these questions, it is helpful to begin by examining the motivation for the association of linguistics with the natural sciences. This will allow me to evaluate claims about the status of linguistics as a natural science. For a considerable number of linguists throughout the discipline's history, linguistics has been seen as one of the natural sciences. As we will see, this has taken on a number of manifestations, all with their particular emphases, but here I give several general factors that plausibly motivate this view. First,

language looks like a natural object as it appears to be universal among human beings. Humans exist with or without systems of currency, but not without language. Second, the desire for exactitude and systematic specification of the discipline can lead to the adoption of standards and procedures from other successful fields of enquiry. Third, the successes of the natural sciences make them an attractive model for conducting enquiry and such a model offers the possibility of the kind of predictions and explanations characteristic of the natural sciences.

The order in which this adoption of standards happens is not necessarily uniform as some linguists take on the procedures of natural science simply because they see linguistics as a natural science. Others look to natural science as a way of attaining scientific status for a discipline whose position qua scientificity is not yet sufficiently firm. As a result of adopting these standards, a discipline can be given a degree of intellectual respectability and it is indeed the case that the natural sciences have provided for linguists a standard upon which to judge something as 'scientific' and thus intellectually respectable.

In the first section, I examine major movements within the history of linguistics and the claims made for the scientificity of linguistics.[1] It is important to note that these views are not uniform and that different authors and schools have different emphases in pressing the claim that linguistics belongs with the natural sciences or is especially associated with the natural sciences. By looking at these views, a sense of the history of the question and the way this history influences current debate is discernable. I consider three major views:

1.2.1 Language-as-organism: Schleicher and Organicism.

1.2.2 The Neogrammarians.

1.2.3 Ferdinand de Saussure and early structuralism.

Before examining these views, it is important that when we talk of the natural sciences, we establish some common ground about the general features of the natural sciences. While no list can be exhaustive or incontestable, a broadly accepted characterisation of science can provide a useful heuristic to help with the comparative analysis of the views that follow.[2] I adapt the following from Alexander Bird's *Philosophy of Science* (1998, p.3)[3]:

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1 This is not an attempt at a history or historiography of linguistics. For such works see Chomsky *Cartesian Linguistics* (1966), Koerner *Toward a Historiography of Linguistics* (1978), Itkonen *Universal History of Linguistics* (1991), Robins *A Short History of Linguistics* (1997) and Seuren *Western Linguistics: An Historical Introduction* (1998).

2 This is also apposite due to the varying conceptions of 'science'. One commentator has it that; 'For

NSa) Nomically governed: governed by universal or natural laws.

NSb) Explanatory: with reference to the laws proposed (NSa), the study has to be able to account for events within its theoretical purview.

NSc) Predictive: a natural science should be able to form hypotheses suggesting the outcome of a future event within its theoretical domain.

NSd) Ability to empirically test hypotheses: For the natural sciences, the paradigm example is that of the laboratory experiment where factors potentially interfering with the testing of a hypothesis can be systematically removed.

NSe) Delimitation: The object of knowledge studied should be well-defined and its borders explicit.

It will become clear that none of the three dominant views of linguistics cited above fits these features of the natural sciences. On the basis of this relatively uncontroversial list, it will be argued, we cannot conclude that linguistics is one of the natural sciences. The negative conclusion is instructive both about the historical situation inherited by contemporary linguistics and about the need to probe more deeply into the relation between linguistics and science.

1.2.1 Language as 'Organism'

The first major example of linguistics being explicitly compared to the natural sciences occurred in the middle of the nineteenth century with the notion of 'language-as-organism'(organicism hereafter) which had its foremost proponent in August Schleicher (1821-68). While not representative of any one particular school, the idea, or metaphor as was sometimes maintained, held significant sway. Organicism emphasised the similarity of

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some (the most extreme), linguistics was a natural science; for others, (a more moderate group), linguistics was comparable with the natural sciences because of the method that it used and the solidity of the results that it achieved; for a third group, scientific, was synonymous with scholarly. In other words, the 'scientificity' of linguistics, though frequently referred to, underwent a continuous process of redefinition all through the [nineteenth] century' Morpurgo-Davies (1998 p.18).

[3] A similar list is provided in Braybrooke (1987) *Philosophy of Social Science.*

linguistics to biology both in the nature of its object and in the rigour of its method. As Schleicher put it in 1863:

> Languages are natural organisms that came about independently of the will of man, grew according to certain laws which also determine their development, ageing and death...the science of language...is therefore a natural science...Its method is identical to that of the other natural sciences. (Cited in Seuren 1998, p.85)

The result of the perceived methodological similarity to natural science was that for linguists, 'language now becomes a real organism with a life of its own independent from that speaker, and linguistics becomes a science with no historical content' (Cited in Seuren 1998, p.88). The phrase 'no historical content' refers to the fact that linguistics had in its recent history been tied to philology and seen within the study of the history of literature, as opposed to an autonomous discipline. However, Schleicher and organicism were in line with current orthodoxy in that they looked to the paradigm of historical linguistics, heavily influenced by comparative philology, for data and approach to its object of study. Though what was different for organicism's diachronic[4] approach was that it saw languages as non-human entities with natural longevity and laws independent of humankind. It is within the scope of this longevity that, as Robins puts it, languages are 'natural objects that grow according to definite laws, go through the phases of development and in the end perish' (Robins 1997, p.205).

In line with this argument, Schleicher saw language as a supra-individual entity, not governed or influenced by speakers but rather, 'Just as human individuals walk in virtue of their legs and feet and the whole biological machinery...in just that way humans possess a biological machinery that regulates acquisition and use of language' (Cited in Seuren 1998, p.84). This may appear similar to generativist notions of the 'language organ', but the non-individualist emphasis on language as an *entity* rather than a *faculty* suggests that similarities are superficial. Language was for organicism an entire *sui generis* organism, not a mere organ within an organism.

Coeval with these ideas, Schleicher supported a *Stammbaum,* or 'family tree' metaphor of linguistic evolution based on Darwin's ideas of evolution in the *Origin of Species* (1998 [1859]).[5] This gave an explanatory framework that accounted for the spread

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[4] Though the terms synchronic and diachronic were coined by Saussure and so postdate both Organicism and the Neogrammarians, they are nevertheless useful in distinguishing methodological or theoretical differences.

[5] As is exemplified in his work, *Die Darwinsche Theorie und die Sprachwissenshaft* (1863).

and diversification of languages, again emphasising that languages had longevity and laws independent of individuals. Languages were similar in existence to mammals, 'represented today by coexistent species in our biological world' (Robins 1997, p.205).

We may list the main ideas of organicism as follows:

O1) Languages are supra-individual entities that have an individual path of development in terms of change, growth and death.

O2) As with genetics for example, languages have their own characteristic set of internal laws that put limitations on their individual development.

O3) Language and languages are amenable to and should be studied independently of other disciplines, for example history, as they are discrete entities. As Morpurgo-Davies puts it, 'the organic metaphor offers a justification for the study of language *per se*' (Morpurgo-Davies 1998, p.87).

In summing up Schleicher's and organicism's contribution to nineteenth century linguistics, Kroener claims that,

> [it] can hardly be over estimated. From his preconception of linguistics as a natural science he drew important conclusions for the theory and methodology of linguistic investigation. The concept of a language as an organism led to an emphasis on its systematic nature and law-governed development (Koerner 1978, p.34).

With organicism we can see evidence of some of the criteria for natural science outlined above. As organicism saw its object of knowledge as being naturally delimited we can align it to (NSe). Second, due to the association of linguistics with biology as well as the assumption of there being law-like statements that were true of language, we may grant (NSa). However, Seuren (1998, p.85) suggests that the status of the 'laws' proposed by organicism were not seen as the explicit and universal laws of physics for example, so some caution is appropriate. Third, the direct link seen by the supporters of organicism with evolution gives it an explanatory framework (NSb) of how language changes and develops. As for prediction (NSc) and empirical testing (NSd) the matter is less clear. Organicism, though proposing that there were laws governing language and linguistic development, was

not explicit in stating what these were and so empirical testing of such putative laws was not possible. Against the background of what were seen as vagaries, a more ambitious view of linguistics emerged with the neogrammarians.

1.2.2 The Neogrammarians

Before the emergence of organicism, work by brothers Jacob (1785-1863) and Wilhelm Grimm (1786-1859) in comparative Indo-European philology gave rise to 'Grimm's Law'[6], the first principle in linguistics with applications for prediction. Grimm's law looked at consonantal shift in Greek, Old Germanic and Modern German and claimed that a circular pattern (*Kreislauf*) existed which showed that phonetic change was law-governed and predictive. Here, the titular connotations of 'law' should be treated with care, for the proposal of Grimm's law was antecedent to strong and explicit claims about the scientific status of linguistics. According to Robins, Grimm, 'did not make technical use of the word *law* to describe what he referred to as sound shift' (Robins 1997, p.199).

The background of Grimm's law is important to understanding the neogrammarian school. Although not conceived of as a law in a natural scientific sense, Grimm's law nevertheless provided grounding for such claims by the neogrammarians, which marked a turning point in the perceived scientific status of linguistics with the publication in 1878 of Brugmann and Osthoff's *Morphologische Untersuchungen* (Morphological Investigations). The central theses of the neogrammarian doctrine centred around two principles that language was governed,

> 1. by sound laws, which transform the old, [historically] transmitted linguistic stock and without changing it in its very foundation bring it into new form; and 2. by the essentially new creations, peculiar to the particular language, produced by the operation of analogy (Janowsky cited and translated in Koerner 1978, p.198)[7].

I will look at these two aspects of neogrammarian thought separately, beginning with sound change. Famously, neogrammarians supported the absolute exceptionlessness of sound laws ('die absolute Ausnahmlosigkeit der Lautgesetze') which were deemed mechanical physical laws. As Robins describes, 'whereas Schleicher had turned to biology, the neogrammarians looked to the exact physical sciences' (Robins 1997, p.207). Sound

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[6] Also known as the Germanic Consonant Shift or 'Rask's Law'. See Seuren (1998, p.83).

[7] Citation from Jankowsky, *The Neogrammarians: A Re-evaluation of their Place in the development of linguistic science* (1972).

laws were motivated by an uncompromising attitude that saw physics as the standard of scientificity. This uncompromising attitude is evident in the claim of neogrammarian Leskien, 'If one admits optional, contingent, and unconnected changes, one is basically stating the object of one's research, language, is not amenable to scientific recognition' (Cited in Robins 1997, p.208).

As well as the claims made of sound laws, the neogrammarians were also keen to refine their object of knowledge and shifted their emphasis of study away from diachronic comparative phonology towards what we would recognise today as a synchronic view. They wanted to distance themselves from what they saw as the empirically unsubstantiated speculations and constructions of early Indo-European, the 'ur-language', which they deemed untestable. Adopting a synchronically-orientated perspective helped them to move away from an otiose linguistics as well as securing an accessible body of empirical information from living languages. As Brugmann and Osthoff put it, they wanted to 'leave the hypothesis-laden atmosphere of the workshop where the Indo-European root forms are wrought and enter the clear air of tangible actual reality so as to gain insight into things that grey theory will never show' (Cited in Seuren 1998, p.91).[8]

This metaphysical and methodological caution was shown in the neogrammarian claim that language was an object of knowledge resting solely within the individual. Partly in reaction to a speculative organicism, they denied the idea of language as a supra-individual object. For the neogrammarians, 'linguistic changes were the changes in individual speech habits' (Robins 1997, p.208). This point is important as it shows that neogrammarians consistently saw sound laws as physical laws, which acted through the individual, requiring no mediating 'entity' as was the case with organicism.

Another important aspect of Neogrammarian thought is analogy. Historically (in Aristotle, Port-Royal Grammar), analogy was cited to emphasise linguistic uniformity,[9] with analogists focussing on the rational and consistent deployment of morphological terminations in grammatically equivalent words throughout a lexicon. This position was also representative of the neogrammarians, though it was put to several explanatory uses. The neogrammarian Paul described analogical formulation in this way:

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[8] Brugmann and Osthoff, *Morphologische Untersuchungen,* Indogermanische Forschungen 11:1900:131-132.

[9] Classically, analogists opposed anomalists, who claimed that language was subject in large degree to unpredictable irregularity. Interestingly, analogy versus anomaly tended to represent positions on the arbitrary versus natural grounding for language, with analogists claiming language was arbitrary and maintained coherence by being governed by strict rules. In contrast, the anomalists claimed language as a natural, monogenetic derivation which was in some sense fallen from purity and so anomaly was unavoidable. For further discussion see 3.1.

> It is an indubitable fact that a large quantity of word forms and syntactic constructions that were never introduced into the soul from the outside can not just be generated with the help of the paradigms but can be generated reliably, in such a way that the speaker never has the feeling of leaving their solid ground (Paul 1891, p110).

For the neogrammarians analogy was central to the question of linguistic creativity as it gave an explanation for how novel production could result from a finite 'ground' or 'paradigm'. Also analogy, being likened here with novelty, was called upon as a prophylactic against apparent exceptions to sound laws. This habit of using analogy to explain away exceptions pointed out by their opponents lessened the persuasive power of the neogrammarians' claim for sound laws and was the main point of attack by dissenters.[10]

To summarise, the central ideas of the neogrammarians are as follows:

N1) Linguistics studies 'sound laws' that are natural laws, no different in principle from those of physics.

N2) Language is an object residing in the brains of speakers and *contra* organicism, is an individual possession, not a supra-individual entity.

N3) New elements are introduced into language by analogy, which is a rational process that could on occasion overrule sound laws.[11]

More than organicism, the neogrammarian approach displays an attachment to what would be recognised today as the tenets of natural science. In their proposal of mechanical sound laws they clearly see them as nomically governed, predictive and testable (NSa, c, d) as sound laws were modified, tested, re-stipulated and anomalies explained away as a feature of analogy. Partly by means of a synchronic perspective, the stipulation of laws also required the strict delimitation of the object of knowledge. As Seuren argues, 'If the notion of an exceptionless sound change is to make sense it must be limited to a well-defined set of rules within the bounds of a well-defined set of speakers' (Seuren 1998, p.98). Here is a clear attempt to strictly delimit the object of knowledge (NSe). What is absent from the literature on this period and apparently from the work of the

[10] For example, Hugo Schuchardt (1842-1927). See (Seuren 1998, pp.96-7).

[11] This is a partial description of how the neogrammarians saw analogy as attitudes varied. For more see (Seuren, 1998, p.92).

neogrammarians themselves is an explanatory model (NSb) giving reasons for language change or an idea of exactly how sound laws worked[12]. This may be explicable in terms of the metaphysical parsimony of the neogrammarians, evident in their refusal to speculate on proto-Indo-European, their wariness of the vagaries of organicism in defining its object of study and their general adherence to grammatical description and building of a body of empirical data.

1.2.3 Saussure

With the work of Ferdinand de Saussure (1857-1913) we move into the twentieth century and to a thinker whose ideas still have influence in linguistics and beyond[13]. Like most linguists of the nineteenth century, Saussure sought to make linguistics an autonomous science. For Saussure as with organicism and the neogrammarians, the natural sciences provided a model for linguistic science. Though Saussure's approach in justifying linguistics as an autonomous science was novel in several respects. Firstly, Saussure made a distinction between the natural sciences and linguistics in a famous passage at the beginning of the *Course in General Linguistics* (2006) [1922],

> Other sciences are provided with objects of study given in advance, which are then examined from different points of view. Nothing like this is the case in linguistics...The object is not given in advance of the viewpoint...it is the viewpoint adopted which creates the object (Saussure 2006 [1922], p.23).[14]

And he contrasted linguistics with chemistry:

> In chemistry, one can study the nature and composition of bichromate of potash without worrying for a moment whether it is a well defined object (Saussure 2006 [1922], p.149).

From this, there are two consequences for Saussure's view of linguistics. On the one hand this claim separates linguistics from the natural sciences, in contrast to the approaches of organicism or the neogrammarians. On the other hand, by demarcating his object of

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[12] One may argue that with the focus on analogy, there was an explanatory model that accounted for exceptions and linguistic change. While this is plausible, there is little consensus about the exact nature of analogy for the neogrammarians. As such it seems not to be overly important.

[13] For evidence of Saussure's influence on modern general linguistics, see Thibault (1997) and Harris (2003) and for structuralist and post-structuralist philosophy of language Barthes (1967) and Derrida (1998, 2002).

[14] Page citations throughout are from the 1922 manuscript pagination.

knowledge in such a way as to be free from external constraint 'the viewpoint adopted creates the object', linguistics is able to meet a level of object-specificity and delimitation common to the natural sciences, albeit one achieved by stipulation as opposed to the nature of the object itself. Saussure considered that 'The theoretically ideal form a science should take is not always the form imposed upon it by practical necessities. In linguistics, practical necessities are more demanding than in any other subject' (Saussure 2006 [1916], p.139). The practical and theoretical convenience of seeing linguistics as not being constrained by a pre-given object allowed Saussure to position linguistics as an autonomous discipline.

Unsurprisingly, given the tenor of nineteenth century linguistics and its concern with the scientificity of linguistics, Saussure specified his object of knowledge so as to be amenable to what he saw as proper scientific study. Partly, this was done by defining two ways in which language could be viewed and studied (the synchronic and diachronic) and then citing one of these as the appropriate scientific method for linguistics. The diachronic perspective on language was the view taken by historical linguistics with its focus on language change over a time. For Saussure though, it was synchronic study that was deemed appropriate to the scientific study of language. The synchronic perspective saw language as an abstract, static system of interdependent elements which was a view that allowed Saussure to claim that, 'each language constitutes a closed system' (Saussure 2006 [1922], p.139) and 'language itself is a system that admits no other order but its own' (Saussure 2006 [1922], p.43). By first allowing that his object of knowledge could be stipulated and then seeing language as a static structure, Saussure was able to delimit the object of study in a way consistent with the natural sciences. It was the neogrammarians who began a shift away from diachronic study; it was Saussure that formalised this shift within a more rigorous framework.

Saussure disagreed with the prevailing neogrammarian orthodoxy that language was not a social or supra-individual object but the sole possession of the individual. He proposed the term *langue* for a synchronic system that was social but closed to the influence of individual speakers through speech or *parole*. As Itkonen has put it, 'the social aspect [of *langue*] is primary vis-à-vis the individual aspect and constitutes the genuine subject matter of linguistics. There will be no scientific study of language until this truth has been grasped' (Itkonen 1991, p.297). Language for Saussure was neither an entity in its own right (as with organicism) nor a series of individual possessions expressed in speech (as with the neogrammarians). Rather it was a supra-individual structure that was the property of

communities, irreducible to individual speakers[15].

Saussure also differed from the neogrammarians on the relevance of laws within linguistic science and denied the existence of phonetic laws as understood by the neogrammarians. Saussure took the view that,

> Synchronic laws are general, but not imperative. It is true that a synchronic law is imposed upon speakers by the constraints of communal usage. But we are not envisaging here an obligation relative to the language users. What we mean is that *in the language* there is nothing which guarantees the maintenance of regularity at any given point. A synchronic law simply expresses an existing order. It registers a state of affairs (2006 [1922], p.131).

What Saussure saw as a law in the natural scientific sense was broadly in line with the positivist tradition, law as a regularity between events or a constant conjunction. However, it is clear that Saussure, despite using the term 'law' within the context of his own theories, was not claiming them as laws in a natural scientific sense. Here, 'general' is akin to 'normative with a limited scope of application' or more plainly 'set of facts Y in area X'. 'Regularities' are not the putatively immutable regularities of natural law but rather salient frequent occurrences that correspond to a synchronic grammatical description (a description of *langue).*

Admittedly Saussure was not consistent in this view. This has led some commentators to justifiably read Saussure as more attached to the notion of law held by the neogrammarians (Morpurgo-Davies in Sanders (ed.) 2004, p.25). For example, at the beginning of the *Course,* Saussure makes it one of aims of linguistics 'to determine the forces operating permanently and universally in all languages, and to formulate general laws which account for all particular linguistic phenomena' (Saussure 2006 [1922], p.20). This position runs counter to the more qualified statements concerning synchronic laws. As such, a definitive account of Saussure's opinion of the character and place of laws within linguistics is opaque, though it is clear he vacillated between a strongly natural-science orientated view of linguistics and something more modest[16].

To summarise, Saussure's ideas (and those of early structuralism) can be stated as follows:

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[15] I discuss the idea of language as a social object irreducible to individuals in more detail in 2.3 and 4.1.

[16] For my purposes this question does not need to be definitively settled. In passing, it is worth noting that this may be one of the vagaries often ascribed to the fact that the *Course* was compiled by Saussure's students from his lecture notes.

S1) To be scientific, language should be studied from a synchronic perspective that allows for a well defined object of knowledge.

S2) Language structure (*langue)* is a supra-individual entity outside of the influence of speakers.

S3) Linguistics should aim to postulate the general and universal laws of language, though this may not in practice be attainable as synchronic linguistic laws have only normative force or, minimally, are a description of a state of linguistic affairs.

Saussure was more circumspect than either organicism or the neogrammarians in his appropriation of the natural sciences as a model for linguistics. Saussure is ambiguous with respect to (NSa), as he wishes to discover general and universal laws in linguistics on the model of laws proposed by natural sciences, but is not convinced of that possibility. As such, it is doubtful that Saussure saw linguistics as predictive in a sense relevant to the natural sciences though it is clear he depended on a rigorous delimitation of his object of knowledge in order to justify linguistics as a science (NSe) and the model for this delimitation clearly has affinity with the natural sciences. Also, like the neogrammarians before him, Saussure's synchronic emphasis allowed for the collection of a stable body of evidence that could be used in the testing of hypotheses (NSd).

While structuralism differed in its approach to the scientific status of linguistics from organicism and the neogrammarians, these two positions helped to inform the outlook and ambition of structuralism. As Sapir claimed,

> Many of the formulations of comparative Indo-European linguistics have a neatness and a regularity which recall the formulae, or the so-called laws, of natural science. Historical and comparative linguistics has been built up chiefly on the basis of the hypothesis that sound changes are regular and that most morphological readjustments in language follow as by-products in the wake of these regular phonetic developments (Sapir 1929, p.209).

Throughout linguistics in the nineteenth and early twentieth century, there was a consistent emphasis on linguistics as a natural science and while the emphases changed, the drive to make linguistics 'scientific' was powerfully present. As Harris has claimed, the approach to linguistics 'was part of a more general movement throughout the nineteenth century to bring serious studies of human behaviour into a new framework of empirical investigation,

for which the natural sciences provided the exemplars' (Harris 1981, p.37) and it is Robins' opinion that throughout the twentieth century 'the emotionally felt obligation shared by all participants [i.e. linguists] has been and remains conceiving and developing linguistics as a science' (Robins 1997, p.241).

1.2.4 The Formalist Attitude and initial problems

As I have indicated, one possible motivation for allying linguistics to the natural sciences was the desire for respectability and recognition of the discipline as a science. Such attitudes among linguists are understandable given the prevailing conception of science and of scientificity we have seen. Like other professionals engaged in a knowledge-generating enterprise, linguists wanted their endeavours to yield results that are interesting and intellectually credible and that would encourage further research. In seeking intellectual credibility linguists were influenced by these prevailing standards. As has been pointed out in terms of the neogrammarians, 'Their proposition...[of sound laws] was in itself only a culmination of successively greater rigour applied to the material of comparative-historical linguistics, but it fitted well with the nineteenth-century natural scientists' belief in the uniformity of nature.' (Robins in Parret (ed.) 1976, p.24).

While one can offer plausible sociological and historical explanations of how 'normal science' functions and how it informs knowledge-generating activity at any particular time, this is a separate matter from a discipline's ability to provide true insights into its object of knowledge. No matter how inevitable or well explained a position may appear, it is still open to philosophical and scientific criticism of its methods and presuppositions.

The scientificity of linguistics continues to be a matter of concern (Wasow 1985, Halle and Higginbotham 1986, Chomsky 1995, 2000, 2002, Seuren 2004) and one way in which a properly scientific linguistics is seen to be achieved is by having a clearly delimited object of knowledge (Nse). As was seen with Saussure however, the justification for the delimitation contains a subjective element that potentially threatens the stability of the delimitation. As Bugarski claims, for linguistics in the mid twentieth century,

> The basis of external limitations [on the boundaries of linguistics] was in principle fairly simple...[however] there grew a feeling among linguists of this period that the authentic object of their science was language in itself. But...it is by no means easy to determine exactly what "language in itself" might be as the subject matter of linguistics (Bugarski in Parret (ed.) 1976, p.2).

The problem that Bugarski identifies for linguistics is twofold. As is noted, it is not obvious how 'language in itself' is to be identified in such a way as to justify the external limitations of linguistics. Another problem is that if one accepts a subjective element in the formulation of the object of knowledge, there is licence to change the object of knowledge arbitrarily. This problem is clear in Saussure's attitude towards his object of study not being given in advance.

Indeed the desire for scientific status manifests itself in the conception of the object of knowledge and one does not have to adopt Saussure's position to see that approaches motivated by the desire to make linguistics scientific are interestingly mutually implicated. As mentioned earlier, it can be the desire to be sufficiently 'scientific' that informs the object of knowledge. For Saussure, the subjective specification of the object allowed for the adoption of a properly scientific method, which made linguistics scientific. Though one need not see the object of knowledge as subjectively specifiable, but may rather identify the natural sciences as models of successful enquiry and so adopt them. Here the result is similar: only an object of knowledge amenable to these standards is admissible. Finally, one may desire a high level of specificity and exactitude in the object of knowledge and turn to the natural sciences as a way in which to ensure this. Again the result is that language becomes defined in light of the standards of objects of knowledge in the natural sciences.

As with organicism, the neogrammarians and Saussure, what language is deemed to be varies. Despite this, there is similarity in attitudes toward the scientificity of linguistics, especially with the desire to strongly delimit language as an object of enquiry; a similarity that I will call the *Formalist Attitude*. I call this an 'attitude' because it denotes underlying assumptions about the nature of linguistics; assumptions that can be shared in spite of other substantial differences and disagreements. Identifying this attitude is important because its assumptions are at the root of several abiding problems in linguistics; assumptions that continue to inform linguistics, as I will go on to show. By pointing out these problems it will be possible firstly to avoid them and secondly, through the lessons learned, to contribute to a positive account of the scientific status of linguistics and the nature of language.

Members of the Formalist Attitude hold that linguistics is a natural science or that it is especially associated with the natural sciences. They also hold the following:

A) If linguistics is to be a science, it should attempt, as far as is possible, to model itself upon the natural sciences.

In addition, the Formalist Attitude can, but need not be, made stronger with the addition of the following:

B) If linguistics cannot meet a similarity of standards with the natural sciences, then linguistics is not a science.

Though B reflects the opinions of some neogrammarians, it is a too negative and unrepresentative position to be of use to the diagnostic and constructive programme that this thesis will engage in. In addition, there is the matter of cashing out similarity of standards to the natural sciences that would be sufficient. A weaker corollary of A is:

C) To the extent that linguistics fails to model itself on the natural sciences, it becomes less scientific.

I will argue the Formalist Attitude consists of A and C and the belief that linguistics is a natural science or is especially related to the natural sciences. With organicism, the neogrammarians and Saussure, there is an explicit desire to associate linguistics with the natural sciences in line with the criteria for science put forward in 1.2. This justifies the proposal of A as an informative generalisation. For C, we should remember claims by the likes of Sapir: '[linguistics'] data and methods show better than those of any other discipline dealing with socialized behaviour the possibility of a truly scientific study of society' (Sapir 1929, p.207) and of Koerner that linguistics can act as a model for the social sciences. If linguistics' similarity to the natural sciences makes it a model for the social sciences, something like C is strongly suggested. As we will presently see with Chomsky, his scepticism about the scientificity of the social sciences presupposes C.

The immediate aim in identifying the Formalist Attitude is to foreground a particular understanding of what constitutes a science and what it means for linguistics to be a science. Central to this understanding is the principle of a strictly delimited object of knowledge (NSe) that allows for or is informed by, an adoption of standards seen as appropriately scientific (see above and 1.2). In such cases the object of knowledge is shaped by perceived standards of scientificity, not vice versa.

While it can be granted that this must to some extent be inevitable as objects of knowledge do not simply appear to the investigator *ex nihilo*, there is a worry that the shaping of the object of knowledge to meet with particular prescriptive standards leads to an unjustified exclusion of facts about that object. The desire to maintain the delimitation of the object (and therefore the scientific status of the discipline) may lead to the unfruitful pursuit of interesting questions or the uninteresting pursuit of bad questions. For example, as is now widely acknowledged (Robins 1997, Morpurgo-Davies 1998, Seuren 1998) the neogrammarian insistence on the exceptionlessness of sound laws was incorrect and led to implausible strategies, such as citing analogy to explain away counter-instances, in an effort to maintain this position. Recall Leskien: 'If one admits optional, contingent, and unconnected changes, one is basically stating the object of one's research, language, is not amenable to scientific recognition' (Cited in Robins, 1997, p.208).

An appropriate way of investigating and analysing the claims of the Formalist Attitude would be to ask the following questions:

1) What is the characterisation of the object of knowledge, O, for a theory T?

2) Is the characterisation O amenable to study in the manner which theory T claims for O?

For both linguistics and the philosophy of linguistics the question can be stated as follows: What is a language? And what is the best available characterisation of the object of knowledge for linguistics? This is a challenge for the Formalist Attitude and to any position that would begin to offer a positive account in its place.

I will argue that the Formalist Attitude fails to meet its own standards of scientificity and that its construal of the object of knowledge is doomed due to an over reliance on the tenets of natural science. The rest of this chapter focuses on two contemporary answers to the question 'What is a language?' that can be seen as manifestations of the Formalist Attitude. First we examine Chomsky and generativism, whose conception of language and linguistics shares much with the examples so far labelled as representative of the Formalist Attitude. Second, I look more closely at Saussure's theory of language, specifically how it is famously treated by Derrida, a philosopher whose approach to language and attitude towards linguistics is ostensibly opposed to the Formalist Attitude. By looking at such apparently dissimilar positions

concerning language and linguistics, it will be shown that there are common problems in the way that language is conceived in both cases; problems which are the result of an over-reliance on the model of the natural sciences (the A and C of the Formalist Attitude).

1.3 Chomsky and the attainment of natural science

My argument in this section develops in four stages. In the first stage I delineate and explain two consistent and related aspects of the Chomskian programme (hereafter 'the programme'): (1) The conception of linguistics as a science and (2) the nature and scope of the faculty of language (FL) and universal grammar (UG). This will provide a platform in the second stage to draw out some of the claims associated with 1 and 2; namely, a) that concepts are innate and b) that linguistics should be modelled on the natural sciences. In the third stage I will argue that claim a) drawn from the background of 1 and 2 is problematic for the programme. In the fourth stage I will argue that claim b) drawn from the same background is likewise problematic.

To begin, an outline of the programme which is a paradigmatic instance of the Formalist Attitude is appropriate. Unlike Pieter Seuren's *Chomsky's Minimalism* (2004), which approaches the minimalist programme as a practising linguist as well as a philosopher, my attention will focus on some important philosophical premises of the programme and avoid talking specifically about the minimalist programme. Essentially, Chomsky's minimalist programme proposes that all languages are unified in being derived from a single grammatical system and that this system is 'perfect' or near perfect; maximally simple for the functions required of it (Chomsky 1995). As the propositions of the minimalist programme are in keeping with the general tenor and claims of the programme, I will not focus on the minimalist programme, though my references will be weighted towards Chomsky's more recent work which will necessarily touch on the minimalist programme. As there is significant overlap between the programme and the minimalist programme, I will consider them as one.

I shall put into question some of the founding assumptions of the programme, in particular its object of knowledge and its conception of linguistics as a science. These, I will show, are paradigmatic expressions of the Formalist Attitude. I will show not only that the Chomskian programme fails to meet the standards of natural science that it sets itself, but also that this failure shows that the core assumption of what I called the Formalist Attitude is the source of the problem.

1.3.1 Galilean Style and the Faculty of Language

That theorists and scientists can and do set boundaries around their enquiries is uncontroversial. Boundary-setting is a methodological necessity in the sciences and evident throughout its history and is attested to in the keenness of linguists to delimit their field of enquiry (1.1). In delimiting an object of enquiry, it can sometimes be that arguments over the nature of objects of knowledge ('language', 'matter', 'organ', 'recession') rest in a difference in terminology and so are not interesting philosophically. Legitimate disagreement needs the respective parties to recognise what each other are (and are not) talking about. As such to *engage* with a theory T about object O, one must first accept the definition of O offered by T. By 'accept' I do not mean agree upon, rather just see that one's arguments address T's account of O, not some other object O*. Failure to do this may highlight only superficial issues that can be defused by rebranding O as O_1, O_2 and so on. For example, this occurs in *Naming and Necessity* (Kripke 1981, p.108), where Kripke invokes 'schmidentity' in place of 'identity' in order to talk of self-relational necessary predicates. The point is that it matters not what Kripke calls self-relational predicates, what matters is that he is clear how one account of identity differs from another.

In order to engage with Chomsky, it will be necessary to show where he sees linguistics within the framework of the sciences, what he thinks of as 'language' and to address problems with his understanding of it. In his more recent writings (Chomsky 1995, 2000, 2002) Chomsky has supported the 'Galilean style' (hereafter GS) of scientific theorising[17] as an appropriate framework on which to base linguistics. GS emphasises a number of things that Chomsky sees as valuable for scientific study in general and so too for the scientific study of language. One emphasis of GS is 'The recognition that it is the abstract systems that you are constructing that are really the truth; the array of phenomena is some distortion of the truth because of too many factors' (Chomsky 2002, p.99). Accepting trans-phenomenality, GS refuses data at the expense of preserving theory, claiming that counter-examples do not entail the abandonment of a theory because even the most successful scientific theories have data which provides counter-examples.

Another emphasis of GS is its lack of concern with intuitive or common sense ways in which we engage with and understand the world. For example in his essay

[17] This term is originally Husserl's.

'Language as a Natural Object', Chomsky argues that for GS, 'the search for theoretical understanding pursues its own paths, leading to a completely different picture of the world, which neither vindicates nor eliminates our ordinary ways of talking and thinking' (Chomsky 2002, p.115). This emphasis on counter-phenomenality has its paradigm case in the Galilean proposition of a heliocentric view of the universe which put into question the common-sense perception of the sun 'rising' and 'setting', which conformed to a geocentric view. Chomsky sees GS as the orthodox position of the natural sciences.

Chomsky also sees GS as applicable to linguistics and this is a principal way in which he casts his study within the mould of the natural sciences. While GS does not necessarily lead to the Formalist Attitude, it is the case that Chomsky holds A and C and sees linguistics as a natural science. Informed by GS, Chomsky views his object of knowledge differently from a common-or-garden understanding, restricting 'language' to mean the 'I-language' of an individual. As Chomsky puts it;

> The language organ is the *faculty of language* (FL); the theory of the initial state of FL, an expression of genes, is *universal grammar* (UG); theories of states attained are particular grammars; the states themselves are *internal languages*, "languages" for short (Chomsky 2002, p.64).

The faculty of language then is a module that contains the universal grammar, a set of highly specified rules and parameters from which the stabilised state is attained. This stabilised state is the I-language, about which Chomsky makes the following claims,

> [The I-language] resembles what is called "a language" in ordinary usage, but only partially: we are no longer surprised when notions of common sense find no place in the effort to understand and explain the phenomena they deal with in their own ways, another achievement of the Galilean revolution, now taken for granted in the hard sciences (Chomsky 2002, p.47).

Though not a concern for linguistics, Chomsky recognises the value of common-sense language terms for what he terms 'ethnoscience' or 'folk psychology'. Ethnoscience and folk psychology are another term for the social sciences. Common sense terms like the putatively supra-individual entities 'English', 'Japanese', which Chomsky refers to as E-languages, implicitly opposing them to I-languages. Pateman has defined E-languages, with Chomsky's approval, as 'an (intentional) object of (mutual) belief, appropriately studied hermeneutically within a sociology of language' (Pateman 1987, p.73). Chomsky claims in terms of naturalistic enquiry, E-languages 'are of little interest for the effort to understand

what language is and how it is used', and that 'alleged community properties can only lead to confusion' (Chomsky 2000, p.100). For Chomsky then the enquiry into language is a solely internalist one, E-languages being an inappropriate subject matter for linguistics.

In addition and also following GS, Chomsky sees the study of linguistics as having little to do with speech as, 'In natural language there is something in the head, which *is* the computational system. The generative system is something real, as real as the liver; the utterances generated are like an epiphenomenon' (Chomsky 2002, p.111). Unsurprisingly, what goes for speech goes too for communication, as possession of language 'need not involve communication or even the attempt to communicate' (Chomsky 1971, p.16). For linguistics the phenomena of speech and communication are only useful insofar as they tell us about the generative mechanism(s) underlying the act itself, the generative mechanism being UG of which the I-language is the realised steady state. As one prominent commentator has put it, for Chomsky 'the central purpose of linguistics is to construct a deductive theory of the structure of human language which is at once sufficiently general to apply to all languages' (Lyons 1991, p.128).

This position motivates another aspect of what Chomsky sees linguistics, informed by GS, doing; marking a formative distinction between descriptive adequacy and explanatory adequacy[18]. Descriptive adequacy is what a speaker knows when they know a language, that is, potentially all the possible sentences that the individual's I-language could produce[19]. Explanatory adequacy is *how* they know this and it is this transcendentally framed question that Chomsky is primarily interested in and answers with the proposal of UG, FL and I-language as putatively fulfilling explanatory adequacy.

It is clear that Chomsky claims that linguistics has several relations to the model of natural science that I outlined above (NSa-NSe) through his association of the programme to GS. Firstly, what Chomsky sees as his ultimate object of study, UG, is universal and as we have seen with the minimalist programme, all linguistic phenomena should optimally reduce to a universal set of rules stipulated by UG. In this, Chomsky allies linguistics with NSa. Second, UG is the sole explanatory framework for I-languages and so NSb is met. Third, the claim for the Chomskian programme that 'much of the variability [of languages] dissolves and we are left with a residue of a few elementary parameters' (Belletti and Rizzi

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[18] The descriptive/explanatory adequacy distinction can be loosely translated from Chomsky's earlier terminology as the Performance/Competence distinction.

[19] Chomsky has defined E-language as 'a set of well-formed sentences' (Chomsky 1986, p.29), though in more recent writings E-language is seen not only as a descriptive set, but also a social object. Here both understandings of E-language would be refused by Chomsky, though E-language-as-set is the target.

in Chomsky 2002, p.20) suggests the ability to empirically test these claims, so there is an association with NSd (see 1.3.4 for more). Fourth, the disavowal of the importance of communication and social accounts of language as an object of knowledge allows for a strict delimitation of the object of knowledge, one that is based solely on the universal structures of the brain (NSe). As such, insofar as it was appropriate to see the neogrammarians and early structuralists subscribing to the Formalist Attitude, so too can Chomsky.

1.3.2 Problems with Chomskian linguistics

The task here is to show that problems confronted by Chomskian linguistics can be traced to claims A and C that characterise the Formalist Attitude. To offer substantive criticism of Chomsky and not merely to tinker with terminological issues we must examine Chomsky's understanding of language and how the scope of his linguistics is motivated by a faulty philosophy of science and language.

Part of the generativist argument for the existence of FL is made by means of a poverty of stimulus argument. This is familiar to anyone versed with Chomsky but will bear repeating as it underpins how Chomsky sees the function of explanatory adequacy in his theory. The poverty of stimulus argument goes as follows; as children develop language with only minimal empirical exposure to speech, there must exist 1) a language acquisition device (LAD) from which the FL develops into a particular steady state (the I-language) and 2) there is a Universal Grammar (UG) that delimits the selection of grammars or 'theories' from the paucity of available empirical data. Both 1 and 2 require that, at the very least, the ability to acquire a language is *in some sense* innate. In some sense, these are uncontroversial claims, as it is undeniable that there is something to be said about the fact that humans, not rocks, clouds or cordyceps fungi, have language.

This is however a far weaker claim than those generally advanced by Chomsky. For example, the claim for UG 2) can be viewed as making two different claims, one harmless and the other possibly problematic:

2a) Due to the constitution of the human brain, there are limits as to what a language could be. UG constitutes the set of these limits and possibilities.

2b) UG is fixed in such a way that I-languages are in an interesting and substantive sense

identical. Mind-external and environmental factors should therefore not play a part in linguistics as language is a 'biological object' (Smith in Chomsky 2000, viii).

If Chomsky holds 2a) then this is uncontroversial. It is clear though that Chomsky holds 2b) when he claims for example that, 'knowledge of these properties [lexical items with complex semantic structure] becomes available on very limited evidence and, accordingly, would be expected to be essentially uniform among languages' (Chomsky 2002, p.86). This can also be seen in Chomsky's thought experiment concerning Martians or Angels coming to earth and witnessing human language. Here, it is Chomsky's contention that 'from an angel's point of view, all languages would appear identical, apart from trivialities' (in Martinich (ed.) 2008 p.687). This is clear evidence of 2b).

For Chomsky, a theory of language aiming at EA should not admit assumptions other than those based on innateness and internal factors. This constitutes the scope of internalist enquiry that 'aim[s] at exploring the mind rather than the environment' (Cook and Newson 2007, p.21). The poverty of stimulus argument is one motivation for this position, but there is another linked argument that supports the Formalist Attitude. This concerns the question of underdetermination of theory by evidence. There are two strands I want to distinguish. Strand one concerns Chomsky's dispute with Quine (1960, in Katz ed.) 1985) concerning extensionally equivalent grammars, grammars that characterise or generate the same set of sentences. Here, the worry for Chomsky is that if one accepts the conclusion that descriptively adequate grammars (DAGs) give us no reason to choose between grammars then 'the question of truth and falsity does not arise' (Chomsky 1986, p.20). According to Quine, it is due to this that the linguist can only 'turn to that last refuge of all scientists, the appeal to internal simplicity of his growing system' (Quine in Katz (ed.) 1985, p.61). If this is the case then Chomsky's claim to be explaining real human faculties and brain structures would be in jeopardy. Against this, Chomsky argues that 'in the absence of such [innate] structure, observed behaviour will lead to no knowledge of language' (Chomsky 2000, p.60). The idea is that if there is no fact of the matter about what humans have in their brains that allows them to acquire language, one cannot hope to explain the difference between humans and rocks, clouds or cordyceps fungi that would account for the one having language and the others not. This transcendental argument commits him to an understanding of language that is not simply the result of practical natural-scientific methodology, but something more metaphysically ambitious. I will return to this in section three. Therefore Chomsky argues that underdetermination of a theory by

evidence is compatible with there being truth and falsity about theories: 'Two distinct I-languages might, in principle, have the same structure, though as a matter of empirical fact, human language may happen not to permit this to happen' (Chomsky 1995, p.15).

The second strand concerning the issue of underdetermination relates to Chomsky's scepticism of induction influenced by Hume (Chomsky 1975, pp.204-206). As we saw of the minimalist program, Chomsky proposes UG as internal, universal and 'perfect' (Chomsky 1995). This sits neatly with scepticism about induction and his derision of the idea that children learn language and concepts 'by "induction" or whatever' (Chomsky, 2000, p.56). This is because there is no requirement of induction in learning a language and so Chomsky is given grounds to propose (or presuppose) entities (FL, UG) that are given, following Hume's phrase, by 'the hand of nature' (Hume 1975 [1777], p.108) Here then, there is strong motivation for holding 2b and as well as this, Chomsky can maintain compatibility between underdetermination with true and false DAGs because there are facts to be explained about the human capacity for language, facts that imply certain DAGs but not others.

Chomsky's scepticism of induction and his holding of 2b) are problematic and showing this to be the case will both jeopardise his claims for the scientific status of linguistics and show these problems are linked to the holding of the A and C of the Formalist Attitude. In order to do this, I will look at Chomsky's argument that concepts and their accompanying semantic representation in FL are innate.

1.3.2 Acquisition and innate concepts

The subject of innate concepts is a notoriously controversial and arguably weak[20] aspect of Chomsky's theory. One may then ask why I am not engaging with the claims about the nature syntactic phenomena, which are both more central to the generativist project and arguably more plausible. One reason is that my aim is to show the extent to which Chomsky wishes to preserve the scope of his enquiry as a strictly internalist one, something that leads him to give support to his philosophically problematic belief in innate concepts. If it can be shown that the theoretical purview of his project is problematic, then this affects the theory globally and thus puts into question Chomsky's claims about the scientificity of linguistics. That is, my focus concerns some presuppositions of Chomsky's theory and their philosophical credibility. As Chomsky's syntactic theory is at least partly an

[20] Though a qualified version of it has been developed and defended by Steven Pinker in *The Stuff of Thought: Language as a Window into Human Nature* (2007).

empirical matter, this is secondary to my concerns.

Now, Chomsky makes a familiar claim about the process of the development of the I-language:

> FL attains state L [an I-language] with little if any effect of instruction, training, or decision, passing through characteristic stages and partially stabilizing at fixed periods. To borrow Hume's phrase, "the operations of the mind, which precede reflection, and which cannot be prevented by it" (Chomsky 2000, p.170. Quote from Hume, 1975 [1777], p.178).

He also sees the development of the conceptual system, containing 'I-concepts', as essentially innate:

> This is the way we learn language. We simply learn the label that goes with the preexisting concept. So in other words, it is as if the child, prior to any experience, has a long list of concepts like "climb," and then the child is looking at the world to figure out which sound goes with which concept (Chomsky 1988, p.191).

> At peak periods of language acquisition, children are acquiring ("learning") many words a day...they are acquiring words on very few exposures, even just one. This would appear to indicate that the concepts are already available, with much or all of their intricacy and structure predetermined, and that the child's task is to assign labels to concepts, as might be done with limited evidence given sufficiently rich innate structure (Chomsky 2000, p.61).

Such is the intuitive implausibility of his belief in innate concepts, which Chomsky accepts has been seen as 'completely unacceptable, even absurd' (Chomsky 2000, p.65), I want to offer a few quotations to show that Chomsky does hold that concepts are innate. For example, when discussing Putnam's argument against innate concepts that 'evolution would have had to be able to anticipate all the contingencies of future physical and cultural environments. Obviously it didn't and can't do this' (Putnam 1988, p.15) Chomsky replies:

> To suppose that, in the course of evolution, humans had come to have an innate stock of notions including *carburettor* and *bureaucrat* does not entail that evolution was able to anticipate *every* future physical and cultural contingency- only these contingencies (Chomsky 2000, p.65).

It is clear that Chomsky does believe that even complicated and historically specific concepts are innate. To dispute that concepts are innate constitutes an attack that falls within the scope of what Chomsky calls 'language' and would meet on Chomsky's chosen ground. While I believe Putnam's objection to innate ideas is a good one, Chomsky does

not and so I will pursue another line of argument against innate ideas that does not rely on Chomsky's rejection of Putnam's objection.

Now to my argument. Let us say that Johnny, a child at the height of his language acquisition powers, lives in a family with an Alsatian dog. Johnny has, by means of few or a single exposure, labelled this dog a 'dog'; that is, matched up his I-concept with the phonetic representation [dɒg]. One day, Johnny is out in the park with his father and he comes across a Yorkshire Terrier with a skin problem. He says to his father 'Look, a dog', to which his father responds 'That's not a dog, it's a rat.' Here then is the dilemma. If Johnny has the I-concept 'dog', he should be able to understand that his father is speaking non-literally because the Yorkshire Terrier is indeed a dog. But of course there is no guarantee that Johnny will understand this as both children and even adults can fail to understand sarcasm. This likelihood should already put us in doubt as to the viability of I-concepts.

But to press home the point let us continue. Let us say that Johnny does not understand the sarcasm but instead re-labels his I-concept 'dog' with the phonetic representation 'rat'. This move keeps his I-concept uniform and only requires a small change in its phonetic representation. As he is walking home from the park however, he spots a large rat and asks his father what it is called. He is told 'That is a rat'. Now again, if Johnny has the I-concept 'dog' phonetically represented as 'rat' he should understand that his father is being non-literal here, but of course his father *is* being literal. Anyway, we might say, Johnny does not understand sarcasm in the first place. However, if he does not take the comment on seeing the rat to be sarcastic, it is hard to know what Johnny should do. He cannot label his I-concept 'rat' with the phonetic representation [ræt], because the phonetic representation 'rat' is now joined with the I-concept 'dog' and his father has told him that a dog is not a rat. To reiterate, the only thing that Johnny can do now is to include the rat he saw as a member of the I-concept 'dog'. But he cannot do this because, on the conceptual level, he knows that a dog and a rat are different things. Given his I-concepts then, Johnny has nowhere to go and for this reason, I-concepts cannot exist. Moreover, even if children did acquire a new word on a single exposure, it would have to be shown that they were in possession of the concept, rather than a chance or very context-specific understanding of when the term might be appropriate. For example, a child may say 'bird' when they see a bird in the sky, but if they then say 'bird' when they see a kite, plane, helicopter or cloud, then they have not understood what a 'bird' is and cannot be ascribed as having the concept 'bird'. Again the fact that such things happen speaks against I-

concepts.

It appears that what Chomsky understands as I-concepts either do not exist or Chomsky has failed to adequately explain what they are. Part of the problem is that we do not know what kind of internalist criteria Johnny's I-language has to meet for him to be said to have the I-concept 'dog'. Hitherto, the use of 'I-concept' although part of a technical vocabulary, presumably seeks to explain what a 'concept' is, rather than to offer a prescriptive technical term that would have an arbitrary bearing on the matter. If 'I-concept' is just such a prescriptive specification, then this is dogma rather than investigation. These arguments against I-concepts suggest that having a concept is not an all or nothing thing. If we allow that concepts can change and indeed can be corrected, then we accept that the attainment of a concept relies on social, normative criteria without necessary or sufficient conditions about what will count as having a concept. For example, I can buy a pot of hummus for lunch and wax lyrical about the virtues of its taste and texture. This would require that I know a fair amount about hummus; if I did this I would show myself to have the concept 'hummus'. However, I could still lack the knowledge that hummus is made from chickpeas and have a concept of hummus that will be fit for use in many contexts. If I were informed that hummus is made from chickpeas this would be informative and might change my concept, though this would require that my concept is open to change and defeasible.

There are more sophisticated and qualified versions of innate ideas from a generativist perspective. For example, Pinker supports a qualified theory of 'basic' innate concepts and holds that 'concepts like "cause" and "motion" really are basic concepts of our cognitive toolbox…saying that some concepts are basic and possibly innate, is not a slippery slope toward saying all concepts are innate' (Pinker 12007, p.107). However, it is worth noting that even if some concepts are 'basic', for example in the sense of their being necessary for the acquisition of other concepts, or in their acquisition being apparent at an early stage of development, this does not imply they are innate, something which Pinker himself notes. This then is a different claim from that of Chomsky and moreover one that would not offer much of a defence for Chomsky's theoretical purview. As such I pursue it no further.

For Chomsky it seems, concepts must be an all or nothing affair. It will be remembered that he claims that 'much or all of their intricacy and structure predetermined' (Chomsky 2000, p.61). Were he to drop the predetermination claim, he would have to give up the study of language from an internalist perspective as impossible, or at least he would

have to abandon some of the theoretical ambitions for generativism, including its association with the natural sciences as he sees it.

1.3.3 Natural science and place of linguistics

We have already observed a problem for Chomskian linguistics in terms of the scope of internalist enquiry and with that, the potential for linguistics to be a natural science in the way Chomsky wishes it. To be in a position to make the kind of claim for innate concepts required by the programme, one would have either to change the scope of enquiry which may make it less in-line with the natural sciences, or simply give up certain claims (I-concepts, modular theory of mind) which would lessen its intellectual appeal as a fruitful area of investigation. Either way, the scientificity of the programme is put in question.

I cited earlier Chomsky's scepticism concerning induction as one of the reasons for his commitment to innate ideas. I now argue such scepticism is problematic[21]. Let us recall Hume's argument against induction. Hume argues that causation is a constant conjunction of events (such that whenever A then B) and that 'The constant conjunction of our resembling perceptions, is a convincing proof, that the one are the causes of the other (Hume 1969 [1739/40], p.53). As such, knowledge of causation relies on knowledge of constant conjunctions (see 2.3). Any enquiry not falling under *a priori* 'relations of ideas', such as geometry and mathematics falls into 'matters of fact' and are open to question by counterfactual possibilities: 'The contrary of every matter of fact is still possible, because it doesn't imply a contradiction and is conceived by the mind with the same facility and distinctness, as if ever so conformable to reality' (Hume 1975 [1777], p.25). In its strongest form the position is, 'no constant conjunctions, no knowledge of causation'. As every act of induction is open to counterfactual possibilities, such acts will find it very difficult to meet the criterion of a constant conjunction and so will fail to provide us with knowledge of cause and effect.

Chomsky's proposal of FL and other generative mechanisms is at odds with this view. There are two reasons. First, it is clear that Hume himself would see the postulation of FL as wrong because 'From causes which appear similar we expect similar effects. This is the sum of all our experimental conclusions' (Hume 1975 [1777], p.25). Also; 'on what

[21] This has been touched on in 1.1. It is worth noting that on the subject of acquisition and innate faculties Chomsky claims his stance 'is much in accord with traditional rationalist conceptions and even, in some respects, the so called "empiricist" thought of James Harris, David Hume, and others' (Chomsky 2000, p.64).

process of argument is this *inference* [between sensible qualities and secret powers] founded? Where is the medium, the interposing ideas, which join propositions so very wide of each other?' (Hume 1975 [1777], p.37). Of course the latter quote is rhetorical, Hume thought there could be no such 'medium' because any such proposal was open to counterfactual possibility. If Chomsky is sceptical about induction then he has to explain how his scepticism permits him to propose such structures as FL and UG.

My second reason for thinking there is a tension is because Chomsky seems to need to endorse induction in some form[22]. If he wants to get his theoretic postulates off the ground as respectable elements within his linguistics, he has to rely on empirical data and these, as 'matters of fact' are open to counterfactual instances. Indeed, this is just what is implied by GS in terms of its acceptance of counter-phenomenality and of the transfactual efficacy of such mechanisms (1.3.1). That Chomsky sees FL as a transfactual mechanism is beyond doubt and he uses the example of a man who, struck dumb, nevertheless maintains his knowledge of language, but loses his ability to speak it to make just this point (Chomsky 2000, p51)[23]. As linguist Peter Jones notes, 'the theoretical abstractions of the scientist, to be sure, are not the result of induction from sense data (although induction does form a part of the process of scientific thinking) but they are, nonetheless, worked up -worked *out*- from available data' (Jones in Cruickshank (ed.) 2003, p.98). Chomsky's reading of Hume appears to be selective and problematic. While Hume accepted that the mind did reach conclusions from experience, the fact that the premises such conclusions are based on do not entail these conclusions required the postulation of an explanatory medium about which Hume was suspicious:

> There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension; and it is incumbent on those to produce it, who assert that it really exists and is the origin of all our conclusions concerning matters of fact (Hume 1975 [1777], p.34).

Scepticism about induction makes it difficult for one to claim the kinds of generative mechanisms that generativism requires. On the one hand, part of what makes UG and FL have plausibility as objects that can be studied from a purely internalist perspective is motivated by scepticism about induction. On the other hand, Chomsky is unsceptical about the rich and innate structures that constitute knowledge of language and his theoretical

[22] Molnar (2003, p.123) makes a similar point with respect to Hume.
[23] I discuss this issue further in 2.3.2.

postulates presuppose an inductive process which is ruled out by Hume. The problem is that if Chomsky were to allow a place for induction in the language acquisition process, then it would be difficult to refuse to include within his linguistics reference to factors such as context, language use and more broadly, communication. If this were the case then the scope of internalist enquiry would be unfeasible and the claim of linguistics to be a natural science would be problematised because the kind of universal and general rules that Chomsky proposes as constituting UG could not be sustained. Chomsky's assertion that the concepts of social science 'will not fall within the explanatory theories of the naturalistic variety; not just now; but ever' (Chomsky 2000, p.209) makes it clear that for him, such a result would be a disaster. Chomsky will not allow a place for social science within the domain of linguistics. Leading on from this, I argue that Chomsky is guilty of dogma in his characterisation of linguistics as a natural science and that as a result, the aspirations of the Formalist Attitude of which he is a member, fail.

In 1.3.2, it was claimed that Chomsky held the following:

> 2b) UG is fixed and narrow in such a way that I-languages are (almost) identical. Mind-external and environmental factors should therefore not play a part in linguistics.

With this in mind, consider the following. Chomsky claims that the minimalist program is based on:

> Innumerable idealizations...We do not expect to find "pure instantiations" of the initial state of the language faculty (hence of UG). Rather, Jones will have a jumble of systems, based on the peculiar pattern of his experience. The explanatory model outlined deals specifically with language acquisition under the idealized conditions of a homogeneous speech community' (Chomsky 1995 p.19).

And in the same vein:

> Linguistic theory is concerned primarily with the ideal speak-listener, in a completely homogenous speech community, who knows its (the speech community's) language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts in attention and interest, and errors (random or characteristic) in applying his knowledge of this language in actual performance (Chomsky 1965, p.3).

Troubling questions arise here that bring out the dogmatism as well as the limitations of

Chomsky's view of linguistics. In these concluding arguments, we will see that the limitations and dogmatism are ultimately a product of Chomsky's adherence to the Formalist Attitude. Considering the first quote above and given the idealisations in operation, it seems inevitable that 'much of the variability [of languages] dissolves and we are left with a residue of a few elementary parameters' (Belletti and Rizzi in Chomsky 2002, p.20). This is because the idealised explanatory model sets a theory-internal series of stipulations which guarantee 2b. Therefore it is not clear that the model is referring to anything real because 2b is derived from a set of stipulations whose bearing on the capacities of real speakers is opaque at best[24]. A question is begged, namely: how does UG constrain and enable the acquisition of linguistic knowledge? The Chomskian programme sets out explicitly to answer this question, though such stipulations mean it has already found its answers before starting to look.

Second, we may wonder why one should not look at just that 'peculiar pattern' of experience that made Jones have the linguistic competence that he has. For Chomsky this would create a too heterogeneous field of enquiry, one that would fail to meet the standards that he supposes are constitutive of the appropriate level of scientificity. Again, such a move would take us beyond the bounds of internalist enquiry. Thirdly and most importantly, it is pertinent to ask if the kinds of idealisations which Chomsky assumes are anything like those of the natural sciences. Above, Chomsky implicitly relies on the paradigm of laboratory conditions, where, to quote one Chomskian, 'a finite set of identifiable and isolable agents or mechanisms are in operation' (Pateman 1987, p.21). But to what extent are Chomsky's idealisations applicable in this context? How is it that Chomsky can claim to be isolating something from the 'jumble of systems' that all speakers have and which would pose a problem for the internalist enquiry of UG?

Of course, one can hold out hope that the cognitive sciences advance in such a way as to provide an experimentally closed system in which the idealisations Chomsky desires become available. But given that even Chomsky claims 'No one knows the extent to which the specific properties of human language are a consequence of general biochemical laws applying to objects with general features of the brain' (Chomsky 2000, p.15), it is an indication that this has not been achieved. It is also indicative of Chomsky's idealisations being dogmatically driven rather than being part of a robust and fruitful theoretical framework.

[24] This also applies to innate concepts. If only the 'ideal speaker-listener' has innate concepts, then this is achieved by stipulation and it is hard to see how useful a claim this is. Though if Chomsky is making a claim that does apply to human beings then my argument against innate concepts holds.

Finally, Chomsky's dogmatism and questionable view of the scientificity of linguistics underpinning his programme are evident in an interview on the minimalist program. Chomsky was asked, 'What kind of empirical discovery could lead to the rejection of the minimalist thesis?' His response:

> All the phenomena of language appear to refute it, just as the phenomena of the world appeared to refute the Copernican thesis. The question is whether it is a real refutation. At every stage of every science most phenomena seem to refute it (Chomsky 2002, p.124).

By 'phenomena' Chomsky is referring to common-or-garden interpretations of events or states which GS is disposed to reject. However, it is clear that Chomsky has avoided the question all together. He has not and does not answer what would constitute a refutation of the minimalist program that he would accept. The question is what a 'real refutation' is and on that point, Chomsky will not be drawn. This is symptomatic of the problems concerned with Chomsky's conception of linguistics as a natural science and is evidence of a refusal to engage despite a tacit acceptance of the pertinence of the question. It is also evidence of a dogmatism that seeks to secure linguistics as a natural science at all costs. Chomsky's attempt to view linguistics as one of the natural sciences fails on its own terms. The postulation of I-concepts does not sit happily with the purely internal scope of his enquiries and scepticism towards induction which provides motivation for UG and FL appears to presuppose induction. We saw in 1.3.1 how Chomsky subscribes to a number of the criteria of natural science outlined in 1.2 and that he holds beliefs constitutive of the Formalist Attitude (A and C). A question remains though concerning the extent to which Chomsky's adherence to the Formalist Attitude is linked to some of the problems we have so far seen.

As I argued regarding linguistics of the nineteenth and early twentieth centuries, there was a desire to ascribe to a set of principles seen as scientific, there is an accompanying danger that the shaping of the object of knowledge to meet with particular prescriptive standards can lead to an unjustified exclusion of facts about that object. Indeed changes in the object of knowledge imply changes in the scope of the enquiry and so a threat to the conception of the object of knowledge can also be a threat to the perceived scientificity of the enquiry. Given Chomsky's and Chomskians' problematic insistence on the internal, closed and highly abstract nature of their object of knowledge, linked to their accompanying insistence on the irrelevance of social accounts of language to linguistics (ontologically and methodologically) and the added dogmatism in the

stipulation of their theoretical domain; it becomes clear that the Chomskian programme's desire for scientificity provides a series of difficulties. So, one can conclude that the adherence to the Formalist Attitude is linked to problems present in the programme.

The Formalist Attitude can be seen as a source of problems for at least one contemporary and influential account of language and linguistics. This may not be surprising, as Chomsky and generativism in general have been criticised for an overly abstract approach (Baker and Hacker 1984, Levinson in Gumperz and Levinson (eds.) 1996, Seuren 2004, Evans and Green 2007). However, as I will now go on to argue, the Formalist Attitude is present and causes problems in less than obvious cases.

1.4 Derrida and linguistic science

Although my criticisms and presentation of generativism as following a historically entrenched attitude in the study of linguistics is new, that generativism has the objective of putting linguistics on a sufficiently scientific standing is widely recognised. The next theorist, however, has not been identified as being committed to the basic claims that make up the Formalist Attitude. Jacques Derrida, to whose views I now turn, is not an obvious proponent of the view that linguistics should be modelled on the natural sciences. To argue this goes against the grain of Derrida's own professed philosophical objectives. First let me outline what some commentators see as these philosophical objectives[25]. Norris understands Derrida as arguing,

> [T]hat deconstruction is a rigorous attempt to *think the limits* of that principle of reason which has shaped the emergence of Western philosophy, science and technology at large…Thus the activity of deconstruction is strictly inconceivable outside the tradition of enlightened rational critique whose classic formulations are still found in Kant (Norris 1987, p.162).

Rorty puts a different emphasis on Derrida's enterprise,

> To understand Derrida, one must see his work as the latest development in this non-Kantian, dialectical tradition-the latest attempt of the dialecticians to shatter the Kantians' ingenuous image of themselves as accurately representing how things really are. Derrida talks a lot about language, and it is tempting to view him as a "philosopher of language"…But it would be less misleading to say that his writing about language is an attempt to show why there should be no philosophy of

[25] I rely on commentators due to Derrida's characteristic prolixity and disinclination to be explicit about the aims and objectives of his philosophy. While not ideal, this has the advantage of additional clarity.

> language (Rorty 1978, p.144).

In an entry on Derrida in *Key Thinkers in Linguistics and the Philosophy of Language* (2005), it is claimed,

> [Derrida thinks] the entire edifice of Western thought is erected on the premise that speech or *logos* is anterior to writing, not only in the temporal sense of order of acquisition but in a deeper, ontological sense…Western philosophy is unrepentantly 'logocentric' (Rajagopalan in Chapman and Routledge (eds.) 2005, p.68).

While there are differences in views on Derrida's philosophical project, at its most ambitious Derrida's objectives question some fundamental assumptions and tenets of western philosophy. The pursuit of these objectives is achieved by close analysis of specific texts; their explicit and implicit commitments, their tensions and their style. As we will see, Derrida's critiques of texts often lead him to suspect and criticise the theoretical ambitions of knowledge generating enterprises, especially in terms of a desire for scientific objectivity and certainty. Therefore to claim that Derrida adheres to the Formalist Attitude goes against his philosophical objectives.

The argument progresses in three stages. I start by giving an account of how Derrida sees writing functioning in language and linguistics and with respect to this, his criticism of Saussure in chapter 2 of *Grammatology,* 'Linguistics and Grammatology'. Having already shown how Saussure is a representative of the Formalist Attitude (1.2.3), a discussion of Saussure and Derrida is a useful way of explaining the origin of Derrida's adherence to the Formalist Attitude due to his inheritance from Saussure of a view of the linguist's object of enquiry. I then examine Derrida's argument against Saussure in the latter's suppression of writing and argue that speech, as well as writing, is problematically suppressed in Saussure's theory. It becomes clear that Derrida also suppresses speech and this can be seen to be motivated by a Saussurean legacy which makes him consistent with the Formalist Attitude. In the third section I strengthen my account by showing Derrida's central thesis concerning the logocentric bias of western philosophy and the accompanying claim concerning the suppression of writing, leads him to deny the importance of speech. It is in this denial that we see the clearest expression of the Formalist Attitude.

1.4.1 Derrida, Saussure and writing

In *Grammatology*, Derrida has the central objective of questioning the relation between

speech and writing in the western tradition of philosophy and science. It is Derrida's thesis that speech has consistently taken precedence over writing in representing language and the concept of language and this is due to what Derrida calls 'logocentricism'. As Spivak explains it, logocentricism is, 'the belief that the first and last things are the Logos, the word...and closer to our time the self-presence of full self consciousness' (Spivak in Derrida 1998, lxviii). Logocentricism is tied to the philosophical search for clarity and certainty.

Derrida claims that despite the historical relationship between writing and speech that privileges the latter, there remains a consistent and often unacknowledged reliance on writing by those that assume its secondary or ancillary status:

> [I]t seems as though the concept of writing- no longer indicating a particular, derivative, auxiliary form of language in general..., no longer designating the exterior surface, the insubstantial double of a major signifier, *the signifier of the signifier*- is beginning to go beyond the extension of language. In all senses of the word, writing thus *comprehends* language (Derrida 1998, pp.6-7).

Therefore Derrida has a clear objective: 'What I would wish to show is that one cannot exclude writing from the general experience of...[the structural relationship of features of language]. Which amounts, of course, to reforming the concept of writing' (Derrida 1998, p.55).

An important part of Derrida's argument that writing is suppressed in favour of speech comes from his critique of Saussurean linguistics in chapter two of *Grammatology*. Here, Derrida attacks Saussure in two ways. Firstly, he argues that Saussure is unable to discount writing as an efficacious and theoretically significant factor in a structuralist theory of language. Derrida accuses Saussure of internal inconsistency in his treatment of the significance of writing within the subject domain of linguistics. Secondly, Derrida argues that the question of the scientificity of linguistics is problematised (in Saussure and beyond) by logocentric dogmas of which Saussure is a representative. These dogmas leave telling discrepancies that can be exploited to show the unstable foundations of structuralist linguistics. In dealing with Saussure's *Course,* Derrida points to passages where he discerns contradiction or inconsistency at work in Saussure's view of writing. Saussure explains his general view of writing within the context of linguistics thus:

> A language and its written form constitute two separate systems of signs. The sole reason for the existence of the latter is to represent the former. The object of study in linguistics is not a combination of the written word and the spoken word. The

> spoken word alone constitutes that object (Saussure 2006 [1922], p.45).

Saussure refuses writing a place within the object of study and the domain of linguistics, claiming the object of study is speech and the synchronic structure that presupposes it. Derrida points to the consistently vituperative tone with which Saussure approaches writing, of the 'tyranny of the written form' (Saussure 2006 [1922], p.54) that 'obscures our view of language. Writing is not a garment, but a disguise (Saussure 2006 [1922], pp.51-52). As Derrida describes it, it is 'within a sort of intralinguistic leper colony that Saussure wants to contain and concentrate the problem of deformations through writing' (Derrida 1998, p.42).

However *contra* Saussure's professed theoretical intentions, Saussure goes on to claim that,[26]

> [I]ts influence on the linguistic community may be strong enough to affect and modify the language itself. That happens only in highly literate communities, where written documents are of considerable importance. In these cases, the written form may give rise to erroneous pronunciations…Darmesteter foresees the day when even the two final letters of *vingt* ('twenty') will be pronounced: a genuine orthographic monstrosity. These phonetic changes do indeed belong to the language but they are not the result of its natural evolution. They are due to an external factor (Saussure 2006 [1922], pp.53-54).

For Derrida a question is begged, namely what constitutes the 'natural evolution' of a language. The begged question impacts on what is seen as the appropriate object of study for linguistics. Saussure assumes that the natural evolution of a language is not influenced by writing so writing is outside the purview of linguistics. Now, even if one were to accept that writing is not part of the natural evolution of a language, it does not follow from this that writing is not appropriate to the study of linguistics. The fact that Saussure admits the influence of writing on language presents a problem for Saussure's rejection of writing. This is because there is motivation for the acceptance of writing as relevant to linguistics. For Derrida, 'It is this logocentricism which, by a bad abstraction, prevents Saussure...from determining fully and explicitly that which is called "the integral and concrete object of linguistics"' (Derrida 1998, p.43).

For Derrida, Saussure's complaint against writing is a *moral*, rather than a *scientific* objection to writing; an objection informed by logocentricism. What Saussure attempts to treat as a *de jure* argument about the nature of language is on Derrida's reading a *de facto*

[26] I quote beyond the scope of Derrida's quotation of Saussure in order to pick out the tension Derrida sees in Saussure.

stipulation about linguistics and an insufficiently justified one. The second way in which Derrida engages with Saussure is to question how linguistics has established its scientific status. Derrida claims that, 'among the "sciences of man," linguistics is the one science whose scientificity is given as an example with a zealous and insistent unanimity' (Derrida 1998, p.28). As I have pointed out earlier, this claim is correct (1.1) and it is one that motivates Derrida's suspicion that the insistence of the scientific status of structuralist linguistics occludes a problem in the way its scientificity is framed. To argue this, Derrida picks out Saussure's stipulation in the *Course* that discussion of writing be kept to the phonetic system (as opposed to, say, the pictographic)[27]. Derrida argues that,

> [such] limitations are all the more reassuring [for structuralism] because they are just what we need at a specific point to fulfil the most legitimate of exigencies; in fact the condition for the scientificity of linguistics is that the field of linguistics have hard and fast frontiers, that it be a system regulated by an internal necessity, and that in a certain way its structure be closed (Derrida 1998, p.33).

Here, due to the imposed limitations, writing becomes automatically outside the field of linguistics. If one limits discussion of writing to phonetic writing, it is a simple move to treat writing as the image and representation of speech. Once writing is established as the representation of speech, its ancillary status is guaranteed as it is defined only in terms of speech. Derrida points here to a paradigm of science where the overriding view of scientificity demands theoretical closure and a strictly delimited object of knowledge (NSe), something which is a salient feature of the Formalist Attitude. It should be noted that in some respects, Derrida's concerns apropos Saussure resembled those I have outlined generally in my proposal of the Formalist Attitude.

Once the status of writing being on the 'outside' of the discipline is assumed due to the closure of the theoretical domain, any influence from writing, or from anything else, comes to be seen as interference with the 'inner' exigencies of the discipline. As Derrida elliptically paraphrases it, 'The outside bears with the inside a relationship that is, as usual, anything but simple exteriority. The meaning of the outside was always present within the inside, imprisoned outside the outside, and vice versa' (Derrida 1998, p.35). If structuralist linguistics, in order to protect its own scientificity, refuses writing admission into the domain of linguistics, it should do so for reasons that are *scientifically* justifiable. However, this is not the manner in which writing gets rejected; 'it is as if, at the moment when the modern science of the logos would come into its autonomy and its scientificity, it became

[27] See Saussure 2006 [1922] p.27.

necessary to attack a heresy' (Derrida 1998, p.34).

Derrida's arguments can be summed up as follows:

1) Saussure is internally inconsistent in his treatment of the place of writing in linguistics. Saussure accepts that writing is a linguistic medium and has significant influence on language, though he refuses to accept writing as part of language.

2) The delimitation of linguistics' object of knowledge is suspect. The move to suppress writing is linked to the demands that linguistics is a well defined object of study which in turn protects its scientificity. However, this is not justified as it is motivated by a dogma.

3) Problems 1 and 2 are driven by a logocentricism within Saussure's thought that wishes to attain a simple, unproblematic and strictly defined object, but is problematised by an ignorance of its own conditions of existence.

1.4.2 The suppression of Actual Speech

Whilst Derrida accuses Saussure of suppressing writing in favour of speech, he supports Saussure in the suppression of speech, or what I call Actual Speech, which functions in a similar way to writing in Saussure's theory. This is a problem because Saussure's reasons for the suppression of Actual Speech are similar to the reason for his suppression of writing. By the lights of Derrida's own argument then, he should not follow Saussure in his support of the suppression of Actual Speech. It is my argument that it is because Derrida suppresses Actual Speech that he can be seen as an adherent of the Formalist Attitude.

Let me first define Actual Speech. Actual Speech is to be contrasted with the Saussurean 'sound image' or 'sound pattern'[28]. For Saussure, it was 'particularly important to note that the sound patterns of the words are not to be confused with actual sounds. The word patterns are psychological, just as the concepts associated with them are' (Saussure 2006 [1922] p.29). So while not reducible to a psychological sound pattern, neither is Actual Speech simply 'actual sound', what Searle has described as 'acoustic blasts produced by...physical and physiological phenomena' (Searle 1999, p.136). Actual Speech

[28] Saussure used a number of roughly synonymous terms including, 'word image', 'acoustic image', 'sound image' and 'verbal sign'. That there may be conceptual differences between these is unimportant here, it being enough to distinguish an act from its psychological necessary conditions.

includes the phonic substance[29] and aural reception of a linguistic act; it is meaningful utterance and its reception, not just a linguistic act. Actual Speech then is quite broad and I define it thus:

> Actual Speech: A spoken linguistic act A which, belonging to a language L reproduces and potentially changes, language L[30].

Actual Speech is not alien to Saussure's and Derrida's theories, and I define it here for purposes of clarity. For Saussurean linguistics, Actual Speech is, like writing, inadmissible to linguistics. The reason for this is because Actual Speech emphasises the individual utterance and its reception and is thus temporal and so essentially diachronic. Saussure shows awareness of Actual Speech and its diachronic nature thus; '*everything which is diachronic in languages is only so through speech*...Speech contains the seeds of every change' (Saussure 2006 [1922] pp.138-139). It is this that makes Actual Speech inadmissible because for structuralist methodology, 'The contrast between the two points of view – the synchronic and the diachronic – is absolute and admits no compromise' (Saussure 2006 [1922], p.119) and '[if the linguist] takes the diachronic point of view, he is no longer examining the language, but a series of events which modify it' (Saussure 2006 [1922], p.128). To study language scientifically is to study it synchronically and to do this Saussure recommended that the linguist 'pay no attention to diachrony' (Saussure 2006 [1922], p.117).

So a link can be discerned between the diachronic and Actual Speech and therefore, the suppression of the diachronic entails the suppression of Actual Speech. Maintaining the suppression of Actual Speech is important to Saussure's view of linguistics, because it preserves its scientificity by delimiting the object of knowledge, as was similarly the case with writing. But, as with writing, Actual Speech exerts an influence on the subject matter of linguistics which Saussure would wish to refuse, but as with writing, tacitly admits. This can be seen in a few ways. Saussure makes this observation about the synchronic:

> The first thing that strikes one on studying linguistic facts is that the language user is unaware of their succession in time: he is dealing with a state. Hence the linguist who wishes to understand this state must rule out of consideration everything

[29] Though this should not be read as excluding other kinds of linguistic substance such as the graphic or haptic.

[30] This does not offer a definition of meaningful utterance or a language. As for the former, I can only appeal here to an intuitive understanding of the term. The latter question will be covered in more detail in 2.3 and 4.1.

> which brought the state about (Saussure 2006 [1992], p117).

For the speaker then, language exists only synchronically and this is one motivation for Saussure's claim that synchronic, not diachronic study is appropriate for linguistics. Baker and Hacker have disputed this counter-claiming that, 'the normal life-span of a human being will allow one to live through significant linguistic change, of which one will typically be aware' (Baker and Hacker 1984, pp.269-270). If this is the case, it puts into question Saussure's motivation for separating the synchronic and diachronic. For example, those born and brought up before or during the digital revolution would have noticed an expansion of the English lexicon to describe the various new technologies and activities ('Googling', 'Ebaying'), brought about by the widespread use of the internet. It does not help if we accept or reject recent changes to the English lexicon as representing a new synchronic state; either way speakers do notice changes in their language. Saussure's presupposition is that linguistics studies the intuitions of speakers, but if speakers are indeed aware of change in their language through time, then the motivation for the synchronic/diachronic split is questionable.

Another tension in Saussure's separation of the synchronic is apparent when Saussure defines a synchronic state by a *lack of change*, though he accepts that 'Since languages are always changing, however minimally, studying a linguistic state amounts in practice to ignoring unimportant changes' (Saussure 2006 [1922], p.142). This raises a number of questions, chiefly; is it possible to distinguish an important change from an unimportant one? There are reasons to think that Saussure has no answer to this. This is because any recognition of a significant linguistic change will only happen after the event[31] and so can only be a matter for historic (diachronic) linguistics that will inform us of the true extent and importance of any change. Due to Saussure's own restrictions, a synchronic perspective has no theoretical right to consider any change as unimportant because it treats languages 'as self-contained systems of communication at any particular time' (Robins 1997, p.224). Two points emerge from this. First, it is only through the study of the history of a language that a synchronic state can in principle be proposed because only diachronic study is able to decide what might constitute an 'unimportant change'. Secondly, if languages are at all points subject to change and speech is a phenomenon which brings about change, then it is theoretically and philosophically difficult to justify the delimitation

[31] A truism of comparative linguistics. Lord argues 'it is useless to look for the *beginning* of a particular sound change, for, by an old tautology, a sound change can only be said to have occurred *after is has occurred*' (Lord 1974, p.137). Sound changes are explained post-hoc, see 3.2.

of linguistics in the particular way that Saussure wishes to because those mechanisms are present within the synchronic state itself. Indeed if they were not, then change would be impossible. Again the absolute distinction imposed between the synchronic and diachronic is questionable.

Insofar as the activity of speech implies the diachronic phenomenon of language change, Saussure's delimitation of linguistics is not compatible with Actual Speech. Indeed, Actual Speech, like writing, threatens Saussure's account of the scientificity of linguistics. As one commentator has noted, it is important to see how 'remote the *signe vocal* [sound-image] is from "sound," and more important, how this remoteness is the very condition of possibility for the structure of the Saussurean sign' (Porter 1986, p.875). Considering the above claims, it is reasonable to expect that Derrida should not oppose Actual Speech as it is suppressed for the same reasons as writing. However, this is not the case. Derrida is aware that, 'Saussure distinguishes between the sound-image and the objective sound' (Derrida 1998, p.63) and supports the idea it has no place within linguistics. He accepts Saussure's stipulation that it is 'impossible for sound, the material element, itself to belong to the language' (Saussure quoted in Derrida 2002, p.18), arguing that,

> [B]y desubstantializing both the signified content and the "expressive substance"- which therefore is no longer in a privileged or exclusive way phonic, Saussure powerfully contributed to turning against the metaphysical tradition of the concept of the sign he borrowed from it (Derrida 2002, p.18).

Derrida makes the odd move of defending Saussure on this point, otherwise 'the sphere of its [linguistics] legitimacy would be confused' (Derrida 1998, p.56). This is incongruous because one of Derrida's philosophical objectives is to show as inadequate the sphere of legitimacy of linguistics, to criticise the fact that, 'structuralism above all insists upon preserving the coherence and completion of each totality at its own level' (Derrida 2006, p.30). Derrida opposes linguistics' 'hard and fast frontiers' when writing is the matter in question, but supports those frontiers when Actual Speech is in question.

Actual Speech is in respects similar to writing and Derrida's refusal to recognise it- to explicitly deny it- puts him in line with the Formalist Attitude as he defends moves to preserve the scientificity of Saussurean linguistics even while attacking it. In this way, Derrida can be seen to be allied with the A and C of the Formalist Attitude.

1.4.3 The conflation of 'Writing' and linguistic history

Why should Derrida support the suppression of Actual Speech? Most obviously, Derrida would wish to avoid privileging speech over writing as such a privileging is a motivation for Derrida's general attack on logocentricism, of which his attack on Saussure is a part. In addition to this it is apparent that if Derrida did not suppress Actual Speech then his account of writing would lose its special position as a suppressed but apparently inextricable part of linguistic science. It is this avenue I will pursue by means of two arguments. The first concerns a conflation of the term 'writing' in *Of Grammatology* that leaves Derrida's treatment of 'writing' in Saussure in difficulty and the second is an historical argument concerning Derrida's use of Saussure as a representative of logocentricism. If writing lacks special significance, then treating it differently from Actual Speech lacks justification and Derrida becomes implicated in a questionable delimitation of the theoretical purview of linguistics, similar to that which he criticises Saussure for.

First, to the issue of the conflation of 'writing'. In order to understand Derrida's position and see the conflation, one first must unpack what is meant by 'writing', as Derrida uses the term in two ways. Staten explains that in Derrida's thought, "'Writing" in the usual sense of the term is language physically detached from the subject who intends meaning, language that is material and intraworldly...Derrida generalizes this ordinary sense of writing, holding on to the pure form of the concept prior to its realization in a signifying substance' (Staten 1986, p.61). He continues,

> Prior to any "inscription" in the ordinary sense- with ink and paper…there must be the pure form of "inscription" – that is "durable institution" of a system of signs upon which any particular realization of the sign must draw. The transcendental concept of writing is thus nothing but the general possibility of the sign, and Derrida calls it "writing" to call attention to certain characteristics which have always been associated with the narrow concept of writing (Staten 1986, p.61).

According to Staten's analysis which Derrida has offered a general encomium of,[32] we have two definitions of writing:

Writing-A (WA): a physical mark having linguistic content. For Derrida this sense of writing is especially associated with the orthographic representation of speech (phonetic writing), though it can include both phonetic and pictographic writing. Derrida has a number of terms for this, calling it 'narrow', 'colloquial', 'quotidian', 'general' and 'primary'

[32] Derrida in Staten (1986, flyleaf).

writing[33].

Writing-B (WB): This is the possibility, or 'transcendental concept' of language. It encapsulates the necessary conditions for language to come into being, and remain in a community. What the general conditions of possibility are is not immediately clear in Derrida or Staten, but the capacity to 'iterate' signs, the power to reuse language and the reproducibility of signs, is important. This is what Derrida comes to call 'archè-writing'[34] which 'cannot, as the condition of all linguistic systems, form a part of the linguistic system itself and be situated as an object in its field' (Derrida 1998, p.60).

In terms of the relation between WA and WB, Derrida argues that the suppression of WA has the coterminous result of the suppression of WB and this link is a motivation for calling both 'writing'. Where he sees the philosophical or theoretical sidelining of writing in the narrow sense (WA), Derrida also sees at work blindness to the conditions of possibility of a particular philosophical or theoretical position, blindness to WB. Derrida claims that 'Oral language already belongs to this writing [i.e. WB]. But that presupposes a modification of the concept of writing [i.e. WA]' (Derrida 1998, p.55).

Some philosophers have stressed that this modification of writing is not an attempt to introduce 'some perverse counter-prejudice on Derrida's part in favour of writing over "speech"' (Norris 1987, p.90). Derrida appears to support this when he claims that deconstruction should 'not consist of reversing it [the relation between speech and writing], of making writing innocent' (Derrida 1998, p.37). I argue that such assertions are false and that Derrida conflates WA and WB in order to achieve a reversal or re-proportioning of the relation between writing and speech. Let us look at two examples where conflation is evident and so dispel any worry I am misreading Derrida against his own and his commentators' understanding,

> Speech thus draws from a stock of writing, noted or not, that language is, and it is here that one must meditate upon the complicity between the two "stabilities" (Derrida 1998, p.53).

> Now one must think that writing is at the *same time* the more exterior to speech, not being its "image" or its "symbol", and more interior to speech, which is already itself a writing (Derrida 1998, p.46, my italics).

[33] See Derrida (1998, p.7).

[34] Norris claims that archè-writing is part of the heritage of '"transcendental" reasoning which Kant first brought to bear upon the central problems of philosophy' (Norris 1987, p.94).

Above, Derrida conflates WA and WB here in the following ways. In the first quotation, WB is referred to as the 'stock of writing' and WA as one of the two 'stabilities', speech and writing. Derrida is suggesting that if language is founded upon writing, then one has motivation to reconsider the relationship between writing and speech. This, however, is not obvious as it is unclear why one should meditate upon the difference between speech and WA as Derrida thinks one should. If, as is meant to be the case, WA and WB are different concepts, then there seems no motivation for such a claim. Derrida appears to be glossing over the difference between WA and WB with the fact that they are, quite simply, both called 'writing'. Unless Derrida can offer better reason that they should be considered relevantly similar, then his recommendation is unfounded and uninteresting.

In the second quotation, the conflation is clearer. What is 'exterior' and 'interior' to speech applies to WA and WB respectively. Derrida claims that writing is at the same time *both* external and internal to speech, suggesting a tension for those that would suppress writing. It is hard to see why this should be the case though, because again two different concepts (WA and WB) are being referred to, so it is not remarkable that they should function differently within a conceptual scheme. Only if they were the same or relevantly similar would this constitute the insight that Derrida claims. They are though, by Derrida's own stipulation, not. Therefore Derrida conflates 'writing'.

This conflation goes unnoticed in some of the literature. For example, in paraphrasing archè-writing, Norris argues that Derrida's proposal, 'can be stated most simply in the following terms. If writing is the very *condition* of knowledge…then how can writing be just one object among others?' (Norris 1987, p.94). Again, the appearance of WA and WB as mutual homophones and homographs disguises the uses that 'writing' is put to. Of course, it could be that Derrida is being consistent and he is implicitly recognising the different kinds of 'writing'. But were this the case, it would leave Derrida's comments as trivial.

Next, I move to my historical criticism of Derrida's use of Saussure as a representative of logocentricism. Roy Harris has argued that,

> [Derrida] seems quite oblivious of the extent to which Saussurean linguistic theory, far from perpetuating the emphasis placed … on sound and sound change, marked a reaction against this trend in the history of linguistics. (But that…would not fit with - let alone be explicable in terms of – Derrida's account of Saussure's role in promoting the pervasive 'logocentricity' of the Western tradition.)' (Harris 2003, p.178).

In quoting Harris, I do not only mean to offer support for my own argument that Saussure and Derrida suppress Actual Speech. Harris is only partially correct here as he is does not recognise that Derrida is aware of and supports Saussure's suppression of Actual Speech; '[Saussure] exclude[s] the very thing which had permitted him to exclude writing: sound' (Derrida 1998, p.53). Harris' criticism seems overly harsh.

This noted, Harris makes a good point about the history of linguistics. It is true that Derrida did not fully acknowledge the context of Saussure's refusal to consider writing as belonging within the purview of linguistic science. The immediate historical background to Saussure's work and thought comes from a century dominated by comparative philology (see 1.2-1.2.2) and the aim to reconstruct proto-indo-European, something which Harris gestures to in the quote above. In the nineteenth century, the body of evidence and methodology of linguistics had an overwhelmingly textual bias. For the majority of linguists, the study of dead languages preserved in textual form, as opposed to the description and typological classification of living (spoken) languages, was the principal source of evidence in linguistic study. As I have pointed out before, linguistics in the nineteenth century was diachronically focused and of this state of affairs it was Saussure's opinion that,

> [A]n exclusively comparative approach...brings with it a whole series of mistaken notions. They have no basis in reality and fail to reflect the conditions which do obtain in language everywhere...not until about 1870 did anyone begin to enquire into the conditions governing the life of languages (Saussure 2006 [1922], p.17).

It was against this textual-comparative bias that Saussure (and some neogrammarians before him) reacted with a synchronic approach. By adopting the synchronic as the appropriate perspective for a scientific linguistics, Saussure signalled a move away from what he saw as the unfruitful and unscientific study of previous generations of linguists.

The point is that the move away from studying sound change and writing was part of a *general methodological shift in linguistics*. That is, it is not just the emphasis on sound change that changed, but also on the perceived appropriate empirical resources of linguistics. With this in mind one can be justifiably suspicious of Derrida's assertion that Saussure's reasons for suppressing writing are logocentric ones, inherited from the inception of western philosophy rather than, as Harris argues, the intellectual climate in which Saussure was raised. Derrida does appear ignorant of this as writing is not the only activity that Saussure suppresses. Rather, this was just one aspect of a package of changes that occurred in

linguistics in the late nineteenth and early twentieth centuries. As Harris notes, part of this shift was with respect to the importance of sound which, as I have argued, manifests itself in suppression of Actual Speech.

Both arguments support my contention that writing does not have the special status that Derrida assigns to it. Actual Speech is in important respects similar to writing in the problems that it causes for Saussure's understanding of the object of linguistics and Derrida should, according to the logic of his own argument concerning the suppression of writing, recognise that Actual Speech as well as writing has an equal claim to be considered within linguistics. This is not the case as Derrida is intent on 'reconsidering the order of dependence' (Derrida 1998, p.51) between speech and writing; a move which leaves him supporting Saussure's account of the scientific status of linguistics, an account he is committed to oppose. In defending the strict delimitation of the borders of linguistics, Derrida unwittingly promotes the Formalist Attitude. That this is not Derrida's professed philosophical position is clear. But what is also evident is that a problematic acceptance of the Formalist Attitude is not only representative of those that univocally support the notion of linguistics as a natural science, but it can also be found in those that ostensibly oppose such pretensions.

1.5 Conclusion

In this chapter I have shown how linguistics is in an unusual position with respect to questions about its own scientific status. Not only are linguists particularly concerned with the scientificity of their endeavours, but this concern has manifested itself in a long and influential trend within linguistic thought which has sought to ally linguistics with the natural sciences, a trend I call the Formalist Attitude. In 1.2, I tracked the Formalist Attitude in its various manifestations among linguists of the nineteenth and early twentieth centuries and in 1.3 and 1.4 I attacked two contemporary examples of the Formalist Attitude. In both of the contemporary cases, there was a failure to consistently delimit language as an object of knowledge in such a way that was consistent with the theoretical ambitions of the position in question. In both of these cases, the motivation for delimitation could be seen to be influenced by the desire to maintain the scientific status of the endeavour. As Putnam has put it, 'the question of how much idealization is legitimate is one that has no general answer. What one has to answer in a specific case is whether the idealisations made…were or were not too severe' (Putnam in Harman (ed.) 1974, p.81). In

the cases so far shown, one is given reason to think them too severe and indeed to consider the Formalist Attitude as requiring overly severe abstractions.

This brings us to another motivation for holding the Formalist Attitude. Despite problems in satisfying the requirement to strictly delimit a theoretical purview, one reason for maintaining it as a goal and holding to the Formalist Attitude stems from an understandable worry. The worry can be put like this: once a study moves beyond a strictly specified domain, investigations become inchoate and open to reformulation so that interesting and verifiable knowledge claims become impossible. Indeed as we have seen, such concerns are expressed by Chomsky in his desire to avoid linguistics becoming an 'ethnoscience' or what he has disparagingly called a 'theory of everything' and also, as we have noted, with the neogrammarian view of Leskien. Of course, such concerns are not aimed at the idea that anything at all might come within the purview of linguistics and the study of language. Rather, and as we have already seen evidence of in 1.3 and 1.4, there is a concern to remove or disallow such things as Actual Speech, communication and what we could term as 'social' or external factors from the study of language, phenomena that seem less amenable to abstract study due either to their temporal nature or the fact that they appear inextricably interlinked with other phenomena. In attempting to go beyond the view of language and linguistics of the Formalist Attitude, there is then reason to consider this worry, if it is justified and what kind of alternatives might be proposed in its place.

Chapter 2
Linguistics and causality in the social sciences

In chapter 1 I argued that in some of its contemporary manifestations, the Formalist Attitude faced problems due to its construal of linguistics as a natural science. In order to accommodate this view of linguistics, the positions I have identified as exemplifying the Formalist Attitude required that 'language' be defined in a way that it is either internally inconsistent or so narrow as to present a tension between the professed theoretical objectives and the ability to meet these objectives. I also argued that an abstract view of language is frequently accompanied by a reluctance to deal with issues arising from actual use of language by speakers (Itkonen 1978, 1983, Pateman 1987). As Itkonen complains,

> [L]inguists, unlike anthropologists or sociologists, do not seem to be overly concerned by the fact that they continuously operate with context-dependent concepts as if these were genuinely universal or context-free (Itkonen 1983, p.215).

And Halliday has offered this opinion:

> We tend nowadays to refer to sociolinguistics as if this was something very different from the study of language as practiced in linguistics *tout court*; but in a way new 'sociolinguistics' is but old 'linguistics' writ large, and the linguist's interests have always extended to social behaviour (Halliday 2007, p.44).

While a study of language that is divested of links to human behaviour has not in fact existed, a tradition of seeing language as abstract, closed and uniform is evident from classical debates between grammarians and anomalists (Robins 1997, Seuren 1998) and up to the present. I have pointed out before that it is plausible such a position is motivated by the worry that to engage with speakers moves study away from what is considered the intellectually respectable tenets of natural science and into the contentious methods and objects of social science. What Chomsky calls 'ethnoscience', or social science, strikes some linguists as possessing dubious claims to scientificity as the social sciences lack the rigorous methods and account of causation that would allow for the proposition of laws or successful and useful predictions. If this is the case, then it is worth considering why some linguists see the social sciences as an inappropriate model for linguistics and why they are suspicious of the social sciences generally. If there are good reasons for this suspicion,

then there may be good reasons to adhere to the Formalist Attitude, flaws not withstanding. The aim of this chapter is to show that the suspicion of the social sciences is badly motivated and so the reluctance to associate them with linguistics as well as considering the actual use of language is unsound. My argument has two parts: I first outline the worries linguists have about social sciences, focussing on causality, and then seek to allay these fears by providing a persuasive account of causality in the social sciences. Tackling the problem of causality removes the attractions of the natural scientific model that sustains the Formalist Attitude and completes the critical diagnostic of this broad approach to language.

In 2.1 I look at a well-known sceptical account about the relevance of the social sciences to linguistics, Roger Lass' *On Explaining Language Change* (1980). This work is interesting in the present context because it rejects a central tenet of the Formalist Attitude: that 'laws' within linguistics are sufficiently similar to those of the natural sciences that linguistics can be seen within a natural scientific mould. I argue that Lass' response to the challenge of explaining language change shows an abiding scepticism about understanding and explaining causality in the socio-cultural realm and as a result, Lass fails to provide an account of linguistics superior to the one he attacks. The upshot of this discussion is that a positive account of the scientificity of linguistics cannot afford to ignore certain problems that arise in the discussion of laws in the philosophy of science.

In 2.2 I pursue this line of enquiry by turning to recent arguments in the philosophy of social science about causality and *ceteris paribus* laws. Invoking *ceteris paribus* clauses and laws is one common and influential way in which scientists and philosophers concerned with the social sciences seek to substantiate causal explanations and show the social sciences to be fruitful and intellectually respectable disciplines. The importance of this debate in discussions about the social sciences is hard to overstate. As noted in 1.1, the question of the scientificity of the social sciences is generally accepted to be related to the question of whether they can be seen to be law governed, offer predictions and explain events which fall within their theoretical purview.

In 2.3 I propose that a powers interpretation of *ceteris paribus* laws is philosophically and scientifically the most satisfactory in the context of the social sciences. This offers a way out of the problems identified by Lass and others (2.1- 2.2), both in rejecting some accounts of *ceteris paribus* laws and in avoiding defaulting to the Deductive Nomological model. I argue a corollary of this is that language can best be seen as a causal power and here I begin my answer to the question 'what is language?' by providing an account that

avoids the problems of the Formalist Attitude while also avoiding pessimism about understanding and explaining events and processes within the socio-cultural realm.

2.1 Lass: *On Explaining Language Change*

The subject of language change has been and continues to be important to linguistics. Winter has made this remark:

> As long as there has been scientific study of language, there has been an interest on the part of linguistics in two major aspects of the field. One may be expressed by the simple question: What is a language like?- the other, slightly more involved, by: What made a language like it is now? (Winter in Polomè (ed.) 1990, p.11).

Language change is seen as relevant to the scientific claims of linguistics and in how linguistics conceives of language as an object of knowledge. This is borne out in Lass' well-known monograph, where he discusses a variety of ways in which language change has been explained and attempts to offer an account of language change that can be given scientific credibility. In doing this Lass explicitly considers issues in the philosophy of science that are pertinent to linguistics and this makes him particularly relevant in the present context as it makes clear the philosophical issues that underpin scepticism about explanation in the social realm.

One way of explaining language change that Lass considers and criticises is with the Deductive Nomological model (hereafter D-N). The D-N model and problems with it as a model for the physical and natural sciences are well known, but I give an outline to contextualise Lass' attack on it and why he thinks it inappropriate for use in linguistics. The D-N model is a model for scientific explanation that characterises laws as universal constant conjunctions between events. It seeks to explain the occurrence of events (explananda) by reference to initial conditions plus at least one law (explanans). Ideally, this process occurs by means of the deduction of the event from the initial conditions plus the laws:

> The deductive paradigm with its emphasis of 'laws'…seems to offer the strongest and most generally satisfying kind of explanation; but it is appropriate only to certain subject matters…this set neither includes linguistics nor any form of history, and thus *a fortiori* excludes historical linguistics (Lass 1980, p.3).

Lass goes on to argue for a positive theory free from the problems he sees besetting the D-

N model. The problems he identifies have been rehearsed in the wider philosophy of the social and natural sciences (Bhaskar 1979, Bird 1998) and so a brief overview will suffice that captures Lass' key criticisms.

Lass argues that both the probabilistic and standard versions of the D-N model fail to offer explanations useful to linguistics. For a probabilistic version of the D-N model, Lass' criticism is twofold. The first problem a logical one and the second is semantic. The logical problem is that the concept of explanation implicit in the D-N model is one that excludes probabilistic explanation as they reduce to uncertain beliefs rather than establishing knowledge of causal relations though deduction, 'it is a necessary property of such explanations that they are (in a strict sense) non-empirical: unlike D-N explanations *they can neither predict particular states of affairs … nor counter predict their instances*' (Lass 1980, p.20). Probabilistic explanation relies on 'only inductive likelihood, not deductive certainty...[this] makes them not only "weaker" than D-N explanations, but makes them non-explanatory' (Lass 1980, p.13). The semantic problem is that probabilistic laws are unable to offer explanations because 'the residue (of whatever size) of instances excluded from the workings of the 'law' [are accounted for] for no apparent reason than the statistical distributions stated by that law' (Lass 1980, p.25). While an event might fit within a distribution of event-types predicted by a probabilistic law, this does not give an explanation of why something did or did not happen.

Against the standard D-N account, Lass raises the familiar point[35] that within historical linguistics, laws of the requisite type are not available because all relevant factors cannot be stipulated within the explanans[36]. This of course is also a frequent criticism of the D-N model from within the philosophy of natural and physical sciences (Cartwright 1983, Bhaskar 2008), but this is not Lass' concern here. The D-N model requires a causally closed system in order to derive *explanandum* from the *explanans* and in linguistics the D-N model would only be applicable 'if in fact one could develop a predictive mathematical model for the development, say, of an open system over time' (Lass 1980, p.88), which would make it possible to treat an open system as a closed one. Lass argues that the causal factors which influence language change are complex, non-deterministic and even non-stochastic so the D-N model is otiose as there are no 'algorithms down the bottom of the [linguistic] garden' (Lass 1980, p.89) that might allow for the fruitful adoption of the D-N model. Lass concludes that the D-N model would ideally be the most desirable model of prediction and explanation for linguistics, though it requires that language be a closed

[35] See Sorokin (1938) and McMahon (1995).
[36] This motivates the adoption of a probabilistic account which as we have just seen, is rejected by Lass.

system or be treatable as a closed system for the purposes of study. This is an idea we have encountered in chapter 1 and it is Lass' opinion that due to problems with the D-N model, 'a pure autonomy metaphysics is likely to raise a number of deep problems, and even if it removes the messiness engendered by speakers, it fails to explain anything' (Lass 1980, p.128). Linguistics is simply not a discipline that can fruitfully employ the D-N model.

Though Lass, quite rightly rejects the D-N model, he also rejects theories attempting to deal with the 'messiness engendered by speakers'. Due to the causal complexity and non-deterministic environment which speakers operate in, attempts to include speakers are either superficial or otiose: 'one might therefore ask whether, considering what looks like a fair consensus [about the lack of need to include speakers within linguistic theory], there is really any need to invoke human beings (messy, complicated things), at all' (Lass 1980, pp.121-122). Whatever the truth of Lass' claim about 'consensus' among linguists[37], there are problems with the idea that linguistics can ignore human beings. One problem is that Lass assumes the failure to usefully include speakers in linguistic theory is indicative of the irrelevance of speakers to linguistics and questions about language. This is hasty, because it is far from clear if it is the nature of the subject matter or the theory itself that is responsible for this state of affairs. What are required are grounds to decide between the two, something that Lass does not provide.

There is good reason to suspect that suppositions of the theory, rather than speakers, is the problem. Lass sees speakers and social phenomena in general as too complex to be included within a theory of language change and is open about a lack of knowledge about them. But it appears that it is *because* of this lack of knowledge that Lass sees speakers as unimportant, rather than on account of their putative irrelevance. Removing or refusing something from theoretical consideration presupposes some understanding about what is being excluded and therefore it is illegitimate to remove speakers on account of the fact that one lacks knowledge about them. Insofar as Lass refuses the inclusion of speakers due to their 'causal messiness', it is plausible he is guilty of the dogma we found with Chomsky (1.3) that in order to maintain a strictly delimited object of knowledge, one could not consider speakers or communication within the purview of linguistics. If Lass holds this view then he too exemplifies the Formalist Attitude, as strict delimitation of the object of knowledge was identified as a salient

[37] The fact that philosophers and linguists (Searle 1969, 1995, Itkonen 1978, 1991, Harris 1980, 1995, 2009, Travis 2008) find a place within their respective theories for the role of speakers suggests Lass' claim about the 'mild embarrassment' (Lass 1980, p.122) speakers cause linguists does not represent a consensus.

feature. This is only a suspicion at present, but I will go on to argue that this is the case.

What is particularly interesting about *On Explaining Language Change* is that there is recognition of problems besetting a strictly autonomous conception of linguistics which suggests that linguistics cannot be autonomous. This is not born out however, as the rejection of autonomy is accompanied by a rejection of speakers in linguistic theory. This suggests a tension which is evident when Lass claims that in linguistics,

> '[T]he notion 'cause' in the deductive sense is wholly inappropriate: our explanandum domain is a set of interactions between non-deterministic open systems...In other words, we are dealing with cultural phenomena, and these are not 'caused'… This does not make [linguistic change], as I will argue, unintelligible (though is does make it inexplicable, which is not the same thing) (Lass 1980, p.132).

In his conclusions, he cites that 'There are (as yet) no D-N explanations for any linguistic change' (Lass 1980, p.143) as a reason to think explaining language change is not possible and asks 'is there any sense in which, in the absence of D-N explanations, and even in the absence of (strictly) falsifiable claims, our accounts of language change can be rational?' (Lass 1980, p.145). The idea that cultural phenomena are not 'caused' and are therefore inexplicable strongly suggests that Lass is presupposing a D-N account of what constitutes cause and explanation. It is not only that he sees the deductive paradigm providing 'the strongest and most generally satisfying kind of explanation', but he doubts anything else can. As linguistics functions within a non-deterministic open-system, events are therefore not 'caused', or at least we cannot say anything about their causes. Lass appears to think the D-N model is the only intellectually respectable option and as the D-N model cannot apply to linguistics, there are no intellectually respectable options for explaining language change[38].

Lass attempts to obviate this pessimism by proposing in what he sees as the spirit of Eddington (1958) and Feyerabend (1982 [1975]), that in replacement of a theory making truth claims about language, we should accept that,

> [O]ur theories do not in any sense map *realia* directly, but are analogues to them, based on structures we discover in (or impose on) certain sensible aspects of the world...Our theories and descriptions then become theories and descriptions of STRUCTURE only...with no attribution of the structure to anything but our

[38] It is worth noting that Lass has offered comment on his earlier work and has been more qualified. However, since he maintains 'This is not to say I think causal explanations are or ever will be available, as I made clear in 1980' (Lass 1997, p.336), his position with respect to my criticisms remains unchanged.

> 'knowledge' (such as it is) of the particular *sensibilia* we happen to be investigating (Lass 1980, p.124).

Lass favourably quotes Eddington to the effect when conducting science 'we emancipate ourselves from a frame of thought when we realize it is only a frame of thought and not an objective truth we are accepting' (Eddington 1958, p.120). With this understanding, linguistics would investigate language structure and linguistic phenomena, though any resulting 'knowledge' would be a product of our linguistic theories rather than of anything real. This allows one to stipulate that language can be viewed as an '"ideal" formalized theory' (Lass 1980, p.124) and in doing this problems of explanation caused by rejection of the D-N model are dissolved, as it is applicable by fiat. What we expect from linguistic theories is not then truth, but what Lass comes later to refer to as 'Insight'.

Insight consists in two things: 'i) virtually undeniable 'facts' discovered about some domain, such as statistical correlations and ii) rather more imaginative projections from facts, such as taxonomic schemata and models as 'metaphorical redescription' (Lass 1980, p.160). Now, i) is a reasonable description of some knowledge claims where reference to causal mechanisms and statistical correlations account for events, such as 'economic depression increases rates of suicide'. This is justified by pointing to studies showing a statistical relationship between an increase in suicides and economic depression, but this is not all. Crucially, what allows the statistical relation to be considered causal is that we have an intelligible explanation about how economic depression could lead to suicide (economic hardship, breakdown in social relations, loss of support services etc).

One problem is that if we accept Lass' account of the inexplicability of sociocultural phenomena and his advocacy of Insight, we would have no rational expectation that, say, funding mental health charities or lobbying government to pursue deprivation-reducing policies would decrease the suicide rate or slow the rate of increase. This is because any Insight we have into open systems concerns our knowledge, not *realia*, and our knowledge is only directed toward our own theories. As such Insight cannot influence rational[39] action because it rejects the idea that we identify causal mechanisms and connections in our explanations of sociocultural phenomena. If causality is about how substances interact then our knowledge of causality is knowledge about how substances interact. This is what Lass rejects and though we might recognise statistical correlations, these cannot influence rational action because they do not permit us to believe any causal claims. Insight can be of little scientific use and by Lass' admission such practices reduce to

[39] I am taking it that Lass sees 'rational' as synonymous with 'deductively derivable' and I use it in this sense.

'myth' and 'poetical' redescription which is 'like an art form' (Lass 1980, pp.158-159).

Lass' proposals for linguistics face a number of problems. His characterisation of linguistics is plausibly circular: a linguistic theory (LT) investigates knowledge that is a product of (LT) and so 'LT' is the object of knowledge of LT. If this were true, then the investigations of the linguist are vacuous and aside from the fact that people called 'linguists' wish to have highly paid and prestigious work, the motivation of linguistics would be unclear[40]. Lass states that 'if the search for nomic necessity in language history is a lost cause, we might as well be content with Insight: so long as we do not lapse into irrationalism or bigotry' (Lass 1980, p.169), though his position is unable to halt such a lapse. If Insight cannot give us rational grounds for action to reduce human misery or believe in Universal Grammar or Zipf's law of least effort, then it is unclear how it can provide a prophylactic against irrationalism. While Insight would not necessarily lead to irrationality, it does not defend against it.

Indeed, Lass's scepticism about non-deductive explanation results in him subverting his own position. He claims, 'there exist no explicit warrants for arguing from distributions [of data] to anything else; any claim that such an argument is meaningful or valid is an assertion of belief, not an argument' (Lass 1980, p.45) but concedes that his sceptical stance concerning inferential warrants 'is also an assertion of belief' (Lass 1980, p.45). Therefore, his position is open to the same criticism he levels at theories holding inference and non-deductive explanations are credible. As Pateman puts it, 'in the end, [Lass] is left not with an alternative to positivist metascience, but only with the discontents of a failed would-be positivist' (Pateman 1987, p.23). To repeat, while I agree with Lass' rejection of the D-N model, his theory faces problems because he is sceptical of any non-deductive model of explanation supplying reasonable grounds for belief in explanations for language change. Lass explicitly rejects the D-N model while implicitly relying on deductive certainty as the only respectable standard of explanation.

Discussion of Lass provides a well-known and clear example of the reluctance of linguists to take sociocultural causation seriously and some of the problems that such reluctance can cause. This reluctance and the problems with it resonates with a broader debate within the philosophy of social science concerning causation and explanation in open systems, systems taken to be characteristic of the study of the social sciences. A sensible way to proceed would be to forego Lass' scepticism about explaining language change and causation of cultural phenomena and try to better understand issues

[40] I deal with this subject in more detail in 4.2.

surrounding the explanation of events in open systems. By doing this and offering an account of causation in the social sciences which can answer some sceptical worries, the aforementioned tendency within linguistics to ignore or disqualify 'messy' things as speakers and social structures might be defused. I now turn to look at one of the main ways in which causality and explanation are accounted for within the social sciences.

2.2 *Ceteris Paribus* laws in the social sciences

It has been widely accepted in discussion of the social sciences (Sorokin 1938, Bhaskar 1979, Mantzavinos (ed.) 2009) that exceptionless laws amenable to deductive models of prediction and explanation are not available in this domain. Whether this is due to the practice of the social sciences or the objects the social sciences study, it is not the case that social sciences seek regularities and laws of the kind associated with the natural sciences. The main practical consequence of this for the social sciences (and arguably 'inexact' sciences such as biology or tidology)[41] is a de-emphasis on prediction and a concern with *post hoc* explanation of events[42]. For the social sciences, having a philosophically robust account of causation and explanation could not be more important, for without one the social sciences cannot hope to explain events and objects within their disciplines. For those seeing linguistics as a natural science or as the most natural scientific of the social sciences, suspicion of speakers and 'enthnoscience' appears reasonable in the absence of such an account. This was seen with Lass, who recognised the D-N model was not suitable for linguistics but in the absence of a clear alternative remained a 'failed would-be positivist' or more generously, remained undecided and sceptical.

In forgoing the expectation of constant conjunctions and deductive derivability of *explanandum* from *explanans*, one popular way social causation is explained is by reference to *ceteris paribus* clauses or laws (cp laws)[43]. A *ceteris paribus* law expresses a causal relationship between objects or events in the form of an antecedent-consequent statement. With a cp law, the consequent being met requires that other potential causal influences, the *cetera*, be non-interfering or 'equal' (*paribus*). An example would be '*ceteris paribus*, high unemployment leads to increased crime'. Here, 'leads to' is a term expressing a causal relation between 'unemployment' and 'crime'. In this case, *cetera* that might prevent the consequent being

[41] For example see Mitchell (2002 in Mantzavinos (ed.) 2009). For a discussion of the example of tidology see Lange (2002).

[42] I discuss post-hoc explanations in the case of language change in 3.2.

[43] A '*ceteris paribus* law' is a law qualified by a *ceteris paribus* clause. Here a 'cp law' and a 'cp clause' are used interchangeably.

met could be increased resource allocation for crime prevention (Police, CCTV) or the government funding of new jobs.

Over the last few decades, the usefulness and metaphysical status of cp laws have been the subject of much debate in the philosophy of science and social science (Schiffer 1991, Pietroski and Rey 1995, Earman and Roberts 1999, Schurz 2001, Spohn 2002, Lange 2002, Cartwright 2002, Earman, Robers and Smith 2002, Kowalenko 2009). Proponents and opponents of cp laws cite a number of factors which make them attractive and unattractive respectively in forming part of causal accounts within the social sciences. Let us begin with what are seen as the advantages of cp laws. Broadly, Schiffer (1991) has described the need for cp laws in this way:

> Some philosophers believe that there are ceteris paribus laws and that without them there would be no special-science explanations, and hence no special sciences. These philosophers think that science is in the business of providing scientific explanations, that such explanations require laws, and that there are no, or only very few, strict special-science laws; whence their appeal to ceteris paribus laws (Schiffer 1991, p.397).

This again emphasises the importance of developing a non-deductive account of causation for the social sciences. As Schiffer states, cp laws are seen by their proponents as providing a way in which the necessity and exceptionlessness of laws can be salvaged in the face of the argument that due to the clearly exceptional nature of proposed generalisations in the social sciences, laws do not function in the social realm (Earman et al 2002). As laws are often seen as being necessary and exceptionless, cp laws offer the prospect of the concept of law being salvageable within a social scientific context and even laws generally. A different approach is made by Lange who argues that cp laws are useful because they do not require laws to be strict regularities at all: '*ceteris-paribus* laws aren't associated with regularities in the straightforward manner demanded by regularity analyses of law *and* analyses of laws as relations among universals' (Lange 2002, p.412). Despite some differences then, cp laws are firstly seen by their proponents as a way of preserving laws for the social sciences.

Second, cp laws provide conceptual closure that makes causal connections explicit. As the social world is causally open, an event could be causally influenced by any number of factors: 'a cp law holds only in a 'closed system', i.e. a system considered in abstraction from other, independently existing factors' (Pietroski and Rey 1995, p.89). Proponents argue that for the social sciences, the conceptual work that cp laws do is an analogue to the

more practical closure the laboratory experiment provides for the natural sciences and in this spirit Kowalenko claims that cp laws consist in a 'type of theorising whose goal is the separation (in the mind) of nature into what are assumed to be its true component parts and processes' (Kowalenko 2009, p.188). A cp law suspends the analog causal matrix of the social world and makes conceptually salient a causal connection or mechanism present in that causal matrix.

Third, cp laws have a philosophically justifiable resilience to direct falsifiability. This is motivated by the aforementioned acceptance that utilising a deductive causal account for explaining social events is doomed to failure as no social hypothesis is exceptionless, given that the social world is causally open and unclosable. There are *no* hypotheses in the social world that are not subject to instances where they might not obtain. Cp laws allow causal hypotheses to remain intact in the face of counter-instances and there is no requirement that a cp law be exceptionless. Though this is similar to the issue of defending the concept of laws in the social sciences, this advantage differs in that it does not directly offer a way of salvaging the concept of 'law' in the social sciences, but rather the idea of having useful and reliable causal hypotheses. That these concepts are different can be seen if one considers that it is consistent to reject the notion that 'laws' of a relevant kind can be applied to social phenomena while maintain that social phenomena are caused and are explicable.

Against the claimed advantages of cp laws, there are some substantive philosophical objections. The main ones and those I will focus on are:

Problem of vacuity: Cp laws are universal and exceptionless but only when they obtain or are met. That they do not always obtain or are not always met is clear, otherwise there would be no need for cp laws in the first place. Therefore, the attempt to save the exceptionlessness and universality of laws in social science by means of cp laws leaves the notion of 'law' vague or not useful as it is *always* possible to make a statement universally true by the creation of a cp law. Cp laws allow for the stipulation of *ad hoc* universals and this makes them vacuous. At bottom, all cp laws contain a vacuous claim of the form 'A causes B only when A causes B'. This is a prevalent criticism of cp laws (Earman, Roberts and Smith 2002, Mitchell 2002).

Problem of confirmation: We cannot know if cp laws are confirmed and explanatory in cases consistent with the cp law being met. If we postulate '(cp) A → B' and we observe that A

is followed by B, due to the unspecified nature of the causal forces surrounding the event, we have no way of ruling out that C, E or F caused B; that is to say, no way of establishing that all other things really were equal. For example, Schurz argues, 'There exists no test criterion which tells us, independently from the truth of the consequent, *when* the *undisturbed* case of a cp law is empirically realized. Therefore, indefinite cp laws cannot be used for the purposes of *predictions*…with their help, all kinds of events which have independently identifiable causes can be *ex post facto* explained by arbitrary and other co-occurring events' (Schurz 2001, p368). Sandra Mitchell (in Mantzavinos (ed.) 2009) points out that due to the problem of confirmation, cp laws violate a pragmatic aspect of the concept of 'law' which preserves the connection between antecedent and consequent. This is because cp laws allow that interactions of different kinds (A→B/C→B) can be treated the same with respect to a cp law.

Problem of counterfactuals: cp clauses do not specify the conditions under which, where the antecedent is present, one can expect the consequent to follow. In a circumstance where '(cp) A -> B' does not obtain, we are left ignorant of the factors which produce counterfactual instances. While it is possible to fill out cp laws to include interfering factors, ('cp, my dog will bark at the postman unless my dog is asleep, dead, being taken for a walk etc.') it is impossible to specify all potentially interfering conditions as such a list is in principle limitless and one cannot exclude the possibility that the environment will change in an unexpected way. For example, the introduction of price caps on commodities would affect Gresham's law of supply and demand, as would an anarcho-syndicalist uprising. If one cannot specify interfering conditions, we have a situation where it is not possible to distinguish between a disconfirming counterfactual (where the cetera were equal but the consequent is not realised) from a non-disconfirming counterfactual (where *cetera* were not equal and the consequent is not realised).

These objections amount to a substantial case for the rejection of cp laws as the solution to issues of causation and explanation in the social sciences. If cp laws are vacuous and cannot be empirically confirmed or disconfirmed then they cannot be useful to the social sciences in supporting explanations and causal claims and we may see that in such a case the suspicions of linguists would be supported[44]. From the problems outlined, it is the

[44] These problems have not prevented cp laws from being taken up by social scientists in their methodology and practice. As suggested by Rupert; 'Admittedly, the assumption that there are c.p. laws has proven useful; perhaps those working in the social and behavioural sciences should continue

problem of vacuity that is most threatening to cp laws and abiding because vacuity is an all-or-nothing thing. If cp laws are vacuous, then they are useless. The same is not the case for the problems of confirmation and counterfactuals, for if cp laws suffered from these problems an optimistic view could be taken, holding that we might improve our understanding of the causal matrix in future (more rigor in observation, larger data sets etc.) which would lead to the problems of confirmation and counterfactuals being less problematic.

I wish to offer a broad, but not uncritical, defence of cp laws in order to show that taking social causation and explanation seriously is warranted and that the suspicion shown by many linguists of sociocultural causation is unwarranted. As cp laws are in some respects ambiguous, covering the lesser problems first will help sharpen what cp laws are and this in turn will inform my discussion of the problem of vacuity and so I will proceed by first defusing the problems of confirmation and counterfactuals.

2.2.1 Defusing problems with cp laws

In discussing the problem of counterfactuals, Mitchell raises this problem for cp laws:

> [T]he ability to fully fill in the conditions that could possibly interfere may well be an impossible task. Indeed, in evolutionary systems new structures accompanied by new rules may appear in the future, and hence we could never fully specify the content of potential interfering factors (Mitchell 2002, p.55).

Though this quote does not concern a social science, it raises an issue for cp laws and is therefore relevant to the present discussion. The first point to make in defence of cp laws is that the problem of counterfactuals is not unique to cp laws and is found in apparently non-cp laws in the natural sciences (Fodor 1991, Pietroski and Rey 1995). Lange for example discusses Boyle's law:

> When Boyle's law was discovered, for example, scientists must have understood its *ceteris-paribus* clause. But they did not know all of the factors that can cause gases to deviate from $PV = k$. They had not yet justified the kinetic-molecular theory of gases. They did not know that the forces exerted by gas molecules upon each other, the molecules' sizes, their adhesion to the container walls, the container's shape, and a host of other petty influences cause departures from $PV = k$. So in discovering that $PV = k$, *ceteris paribus*, scientists couldn't have discovered that $PV = k$ holds when the gas is 'ideal' in the above respects. Rather, the *ceteris-paribus* clause in

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to investigate c.p. laws, regardless of whether anyone has developed a satisfactory semantics for c.p. law-statements' (Rupert 2007, p.2).

> Boyle's law covers the 'disturbing influences' recognized by scientists when discovering the law (Lange 2002, pp.411-412).

Lange is making the epistemic and pragmatic point that even with so-called strict laws, there is a process where scientists can come to understand potential interfering factors, something which requires they work with a law *as a* cp law, where interfering conditions are not fully 'filled in' (because not discovered yet). There is not an obvious upper time limit to fill in interfering factors and if there were such an imposition, we would risk rejecting genuine law candidates.

What the above shows is that the problem of counterfactuals does not only apply to the cp laws of the social sciences and so Mitchell's implication that unless one can specify all present and future interfering factors then cp laws are not feasible seems too strong. If all present and future interfering factors were somehow filled-in, we would not have a cp law at all because we would no longer have any *cetera*, simply a well-defined set of antecedents. As Lange's example suggests, this demand must be too strong, for Mitchell's position appears to rule out that we might rationally maintain a law in the face of a surprise from nature. Surprises from nature are just that -surprising- and impossible to rule out. It is important to remember that scientists have to work with laws that are not 'filled in' as if this were not the case and if we followed Mitchell's suggestion, then many proposed laws would have a very short lifespan indeed. The problem of counterfactuals is an issue for causal hypotheses in general and so is not a *particular* problem for cp laws within the social sciences.

Now to the problem of confirmation. It is clear that (cp) Ax->Bx & Bx, while being consistent with the cp law, does not allow us to discount Cx, Dx, Ex etc. as the cause where (cp) Cx/Dx/Ex->Bx. For example, let us say that a particular change in the phonemic system of English (Bx) can be explained by Zipf's law of ease of articulation (Ax) and mimicry of prestige forms (Cx). It may not seem that one can choose between Ax and Cx as to the cause of Bx. If we had no grounds upon which to decide between competing cp laws as explanations of events, then the problem of confirmation would be damaging. This problem is made clearer if we consider that events are almost always polygenic and if they were not then science would be considerably simpler. In the case of the natural sciences, the causal closure offered by laboratory conditions allows for the possibility that causal influences can be determined in terms of their respective influence and that one can test a causal hypothesis without interference from other factors. As these conditions are not available to the social sciences, neither is that possibility. The problem

of counterfactuals does then look formidable.

One proposed solution is to construe the semantics of cp laws as stating what *normally* happens (Schurz 1995, 2001, Silverberg 1996). Schurz argues that a law such as (cp) Ax->Bx should be reconstructed as Ax->Bx where '->' is a normic operator. He explains,

> [T]he indefinite CP-clause is understood as a normality clause that does not make a separate antecedent-conjunct but is implicitly contained in the normic conditional operator…[normic laws] imply a statistical normality condition and hence have empirical content and can be used for probabilistic predictions (Schurz 2001, p.369).

This would defuse the problem of confirmation as probabilistic laws do not commit themselves to specifying cause in an event where the antecedent condition (Ax) is present. They simply hold that where Ax, there is probability $p>0.5$ of Bx occurring. In the case of normic-cp laws, a requirement that they be distinguished when an event occurs which is consistent with more than one of them is not applicable. This move defuses the problem of counterfactuals as a probabilistic law allows for a specified degree of counterfactual instances within a data set without being disconfirmed. The problem with this move is that the construal of cp laws as normic collapses any distinction between cp laws and probabilistic laws. Therefore it is hard to see how this provides a defence of cp laws rather than an abandonment of them, as there is little to distinguish the two. Though Schurz might justifiably claim to be clarifying the semantics of cp laws rather than dropping them, in doing so he removes an important aspect of them: their claim to explain why something happens. As such, normic-cp laws cannot effectively answer the problem of confirmation or counterfactuals as they do not look like cp laws at all, they look like probabilistic laws.

As well as this, the idea that cp laws should tell us about what normally holds raises a doubt about their value to scientific practice and theorising. For example, take society S. All the members of S exercise regularly and eat a balanced diet but also smoke. It is a true statement that 'cancer is a rare disease in S' with respect to the lifetime probability of a member of S developing cancer (<0.5). For S then, it is untrue that 'smoking normally causes cancer' and so the implicit normic rider in, (cp) Px->Qx where Px is 'smoking' and Qx is 'cancer' is falsified. The interpretation of S that we want to give is that eating a balanced diet and regular exercise prevents cancer and so while 'smoking causes cancer' is true, its influence is masked by the influence of other factors. In S, smoking is not a very *effective* cause of cancer, though it still has *efficacy* in causing cancer. If we want to hold on to accounts of this kind (and I strongly suspect that we do), a normic interpretation of cp

laws will not suffice as it cannot account for anything that does not normally happen and so we would be forced to reject the claim 'smoking causes cancer' for society S.

While I disagree with the normic view of cp laws, Schurz's view raises an important point. This is that there do appear to be cp laws that concern what normally happens. The rise in popularity of far-right parties normally leads to an increase in race-related attacks, printing money normally leads to inflation, murdering a spouse normally leads to imprisonment. Without these and many other normal happenings which justify the expectations of agents, social life would be impossible and this gives some plausibility to normic-cp laws. Though in spite of this plausibility, there are many things that normally happen, and many things that don't normally happen. If we want the things that don't normally happen to stand any chance of being treatable by a *ceteris paribus* clause or explainable at all, then a normic interpretation of them is inadequate.

I return now to the problem of confirmation for cp laws that are *not* normative. I admitted before that due to the polygenic nature of events and the open system which constitutes the social science's explanatory domain, there is no clear way of recognising an undisturbed instance of a cp law. One may take this to mean that the problem of confirmation is fatal for cp laws, but while this characterisation of cp laws is accurate, it does not warrant their rejection. One thing to consider is that if cp laws are viewed through a deducivistic prism where rational belief in them consists in every other possible causal factor being ruled out, then we are simply requiring that cp laws not be cp laws and so beg the question (cf. discussion of the problem of counterfactuals above). The issue then is about the availability of good reasons for citing an antecedent as the cause of the consequent when there are other competing antecedents available.

Let me elaborate with the following example. A recent *Guardian* article on the 2011 London riots cites a government report which claims that the riots were the result of a number of factors; distrust of police, unemployment and disaffection of some rioters from wider society[45]. If such cp laws were suggested as, '(cp) unemployment causes civil unrest' and '(cp) distrust of police leads to civil unrest', then one would not on the face of it be able to decide if these cp laws were confirmed in the instance of the London riots because there appears no way of attributing causal influence and there is no way to develop a test which tells us when an undisturbed case of a cp law is realised. In so far as the explanation relies upon cp laws, should one reject the findings of the report as unscientific and incredible?

[45] Source: http://www.guardian.co.uk/uk/2011/dec/05/anger-police-fuelled-riots-study. Accessed 23/3/2012.

Such a conclusion would miss the point about explanations in the social sciences and what it misses, I argue, motivates the rejection of the problem of confirmation. Explanations of the above kind look vulnerable to the problem of confirmation because they are framed outside the context of any canon of research (say into the Brixton riots or LA riots). Though reasons for discounting one cause over another, or seeing a cause as effective in a particular context are many, congruence with other data, explanatory adequacy, rational reconstruction of the event, induction concerning the (non)presence of other factors in similar situations and the ability to motivate interesting research programmes are (still disputable) criteria used in assessing laws[46] in both the natural and social sciences. Despite what is seen as the problematic quality of cp laws in their not being easily confirmable or refutable, they are not arbitrary. We can see this if we consider that, '(cp) oaks cause an increase in neologisms' or '(cp) watching cricket causes psychotic episodes' are never proposed. This is because such proposals lack intelligibility; we cannot imagine how they might be true. These points support the idea that the problem of confirmation is not fatal to cp laws.

Recall my example of language change. While only an outline, it is not far-fetched as mimicry and ease of articulation are often cited as reasons for sound change (Saussure 2006 [1922], Aitchison 2001, cf. 3.2). In this case, it appeared that there was no rational way of choosing between Ax and Cx as the cause of Bx. Though without what might be relevant context (the speech community may have no prestige form, brevity in speech may be government policy), such an example is bound to be vulnerable to the problem of confirmation and this appears to be the case with many cp laws because, *as statements*, they are given without a specified context. But as statements given without specified contexts, we might be doing one of two things. As Earman et al suggest, we might have a '*pragmatic* reason for producing a CP law-statement, rather than [an] *epistemic* reason for believing in the existence of a CP law' (Earman et al 2002, p.296). That is, a cp clause might be used in the same way we might say 'stuff' rather than list the full contents of our shopping basket in a conversation about our activities. This is innocuous shorthand. Another thing we might be doing though is producing a cp law that, pragmatics aside, is contextually impoverished. In this case we might well reject the cp law, but this does not mean that within a specified context and canon of research there can be good reason to believe in the explanations from open-systems of polygenic events that are presented with a *ceteris paribus* rider.

[46] For example see Bird (1998), pp.263-264.

As a last note here, it is important that we not resort to a default scepticism because of lack of deductive certainty or surety obtainable from laboratory experiments where an event occurs which is a confirming instance of a cp law. Again this begs the question against cp laws and could also represent a double standard, for as we saw in the case of Boyle's Law, some natural scientific hypotheses survive in the face of the same issues.

I now move to the problem of vacuity. The problem of vacuity is the most discussed and the most difficult of the problems facing cp laws (Pietrosky and Rey 1995, Mumford 1998, Mitchell 2002) and to make the issue clearer, I wish to separate two concerns involved in the problem of vacuity. These are:

(i) cp laws allow for *ad hoc* universal generalisations.
(ii) cp laws are non-explanatory tautologies.

For (i), the worry is that acceptance of cp laws gives one free reign to stipulate universal generalisations. This makes cp laws useless to science as they lose the ability to distinguish a putatively true causal claim from a false one or an accident from a causal connection. Any cp law I invent '(cp) oaks cause an increase in neologisms' is universally true by dint of the cp clause and while this is clearly not the intention of anyone proposing a cp law, it nevertheless fails to stop such universals from being posited and cp laws are therefore vacuous.

While cp laws do not block the proposal of *ad hoc* universals, they do not licence the belief in any universal I might invent in a way that would make them all vacuous. If I believe in at least one cp law, I may still rationally deny the truth of other cp laws and if I can do this, then the complaint that cp laws licence *ad hoc* universals is not damaging to cp laws *simpliciter*. Let us use an analogy. I may hold that no word in and of itself is racist and therefore deny the idea, say, that no non-black person can ever use the word 'nigger' in a way which is not racially offensive. Rather, I suggest, it is the use that people put the word to that matters. Now let us say that the (white) leader of a political party uses the N-word in the context of suggesting that black people are to blame for the moral degeneracy of the nation. When called a racist because of his use of a racist word, he responds by pointing out that he did not use a racist word for there are no racist words, just words used in a racist way. Here, the political leader is using the licence given him by my position which is roughly, 'no word is racist, only the use words are put to'. Some may draw the inference that because they believe the political leader to be a racist and to be using words that are

racist, then I must be a racist, because my position licences the use of such words and a defence of their use. My position 'no word is racist, only the use words are put to' might then be accused of being a racist position because it licences *ad hoc* defences of racism, for one can *always* say that one is not being racist. To this I would respond that my position does nothing of the sort. My position might give racists an excuse to indulge their racism (which is regrettable), but the real issue is with our analysis of the speech of those that use such terms and in what context. If we can beg a question here and say that the leader of the political party *was* in fact being racist because of the way in which he was using terms which are often used in racist ways, then we can see that his defence is a sham and it is not my position, but his illegitimate use of my position, that is the problem.

I argue that this is the situation with cp laws licensing *ad hoc* universal generalisations. Cp clauses *do* licence '(cp) oaks cause an increase in neologisms' or '(cp) ionic bonding causes nuclear fission' but cp laws do not stop nonsense from being false and from those that support cp laws rejecting such nonsense. Such a claim is not supported by any research, it does not bear up to any rational reconstruction of how oaks might cause what the statement claims and so on. As with any bad (unsupported, unintelligible) hypothesis, it should be barred from being a working hypothesis. As with discussion of the problem of confirmation, the issue again concerns the availability of good reasons for citing an antecedent as the cause of the consequent in a particular instance. Cp laws are open to abuse in a way that exceptionless and universal laws derived from the D-N model are not but that we can in principle distinguish abusive and non-abusive cases means that licensing *ad hoc* universal generalisations is not a problem.

Now to the idea (ii) that cp laws are non-explanatory tautologies. Pietrosky and Rey characterise the problem like this:

> There is a legitimate worry that appeals to cp-clauses render the nomic statements they modify somehow vacuous or unacceptably circular. In particular, if 'cp, F => G' means merely that 'F => G' is true in those circumstances in which there are no instances of F and not G, then 'cp-laws' look to be strictly tautologous. True, but presumably not explanatory laws in an empirical science (Pietrosky and Rey 1995, p.87).

As with other responses to problems with cp laws, defenders sometimes stress that all or many laws are cp and so shift the pressure off the social sciences specifically and onto the sciences in general: '[it] allows them to proceed without worry that they are focusing on some peculiar and, perhaps, undesirable feature of the special sciences' (Earman et al. 2002,

p.283). The laws of physics have exceptions and our predictions using them fallible (Cartwright 1983, Bhaskar 2008) and there are laws in the natural sciences which rarely or never apply in events in the world as freefall does not occur in frictionless environments, receptacles containing gases are not uniform etc. Such observations lead some defenders of cp laws (Morreau 1999, Lange 2002) to claim that they 'go all the way down' and that all laws contain *ceteris paribus* clauses. The brothers-in-guild argument goes like this:

P1: Any law with an unspecified antecedent is vacuous.
P2: All laws have unspecified antecedents.
C: All laws are vacuous.

Defenders point to the unpalatable conclusion as a reason to accept that laws can have unspecified antecedents without being vacuous and so cp laws are not irredeemably vacuous. It looks like if we do not accept this we are forced to accept that laws are not useful in explaining and/or predicting events in the world because they are vacuous.

As a response to the problem of vacuity, this move is not effective. For the sake of argument, let us assume that cp laws do 'go all the way down' and we reject C. The defender of cp laws in this case has got their way in showing that cp laws cannot be *strictly* vacuous because there are cp laws in the natural sciences and they are very useful. This however does not prevent cp laws from being near vacuous (Schurz 2002). It does not matter if all laws contain a cp clause, what matters is how effective and informative laws are as tools for prediction and explanation and it is not controversial that the laws of the natural sciences are useful. We can gain knowledge of causal connections through laboratory experiments by creating (relatively) closed environments and can apply this knowledge to contexts where we know enough about the presence of other causal influences to make successful predictions. It is acknowledged that this kind of knowledge and predictive success is lacking in the social sciences[47] and that causation within the social realm is more complex than in the areas studied by the natural sciences. If the aim is to defuse the worry there is something about cp laws in the social sciences that makes them especially vulnerable, then the brothers-in-guild argument does not achieve this.

An explanation of why cp laws in the social sciences may be vacuous or near vacuous even if all laws are cp 'at bottom', is that the subject matter of the social sciences

[47] With the notable exception of economics. Though as MacIntyre (1981) has shown, the accuracy of predictions by economists are notoriously poor, despite an institutionalised assumption that economics is predictive.

is so 'messy' or causally complicated that one cannot disentangle the causal matrix. Mumford puts the problem this way:

> The possible background conditions cannot be excluded in a finite list that is appended to the conditional. This is because there is no finite list that could name all such possible conditions in which the manifestation is prevented. To state that the excluded background conditions are any conditions which interfere with the disposition manifestation is to render the conditional trivial (Mumford 1998, p88).

While we have already seen concerns about the 'filling in' (here 'finite list') of antecedents, Mumford emphasises the fact that this leads to cp laws being trivial, or non-explanatory tautologies. This makes the connection between the problems of vacuity, counterfactuals and confirmation clear because all the problems are related to our failure to identify background conditions: either conditions interfering with '(cp) Ax->Bx', or those conditions we cannot discount as causally responsible for Bx, or those regarding which we do not know their influence on Bx. Failure here leads to the semantics of cp laws being construed as vacuous or near-vacuous, though it is an open question whether 'filled in' cp laws are cp laws at all.

As a final point here, the link between idealisation and the vacuity problem is also important. As I pointed out before, something that makes cp laws attractive is that they offer conceptual closure that makes causal connections salient (Kowalenko 2009). Though this appears problematic when they are interpreted as being, 'a vehicle of such abstractions [those of the natural sciences]. Metaphorically: cp-clauses are cheques written on the bank of independent theories' (Pietrosky and Rey 1995, p.89). The danger is that such cheques become *blank cheques* which produce idealisations so abstract as to be unhelpful in investigating the nature of objects, leaving them open to the change of gerrymandering conditions to suit theoretical purposes[48]. Such blank cheques are indeed vacuous or near vacuous.

So where are we with cp laws in the social sciences? I have argued that the problems of confirmation and counterfactuals are not fatal for cp laws in the social sciences and that cp laws cannot be considered normic as these are no different from probabilistic laws and are also non-explanatory. In consideration of cp laws as idealisations, they run the risk of being blank cheques leading to vacuity and as there is no prospect of having 'filled in' antecedents or in creating causally closed environments, cp laws in the social sciences look considerably shakier than their natural-scientific counterparts, even if

[48] See discussion (1.2) of Chomsky's use of the idealised speaker-hearer.

we agree that some or even many laws of the natural sciences contain a cp rider.

2.2.2 Cp laws and causal powers

Are cp laws in the social sciences to be discarded as vacuous? Is the caution toward the social sciences and social causation by some linguists justified in light of the problems besetting an influential way of construing causal claims in the social sciences? I want to argue that cp laws are not vacuous and the general pessimism shown by Lass, Chomsky and others is unjustified. This requires that we think differently about cp laws and the way in which I suggest we do so may for some constitute a rejection of them. I suggest that we consider causal statements in the social sciences as statements about power. Powers are sometimes known as 'dispositions' (Mumford 1998) or 'tendencies' (Bhaskar 1979, 2008), but in the present context these distinctions are of no import. Seeing cp laws either as power ascriptions (Kusch 2005) or inadequate representations of power ascriptions (Cartwright 2002, Molnar 2003) is not new but what I hope to show is that the interpretation of cp laws as power ascriptions retains the advantages that make cp laws attractive but does not suffer from their problems. Ascription of causal power consists in a number of theses. Basically, the notion of a 'power' captures the idea that an object possesses a causal potential (which forms the antecedent in the formulation of a law) that can lead to a particular event (the consequent). Powers are transfactually efficacious and can be exercised despite not empirically realised (the attractive force of one magnet may be disguised by that of another). Also, powers can remain unexercised. For example, a car has the power to go 60mph, but when stationary it is not exercising this power and a store manager may have the power to fire an employee, but may not do so. Like cp laws, conditionals implied by power ascriptions are defeasible propositions and acceptance of them implies rejection of the idea that causal statements can only be maintained by the postulation of nomically necessary exceptionless laws.

Power ascriptions make different claims from cp laws as cp laws quantify over events, whereas powers quantify over properties. If cp laws are taken to be universally quantified propositions describing a strict nomic regularity between events then they are vacuous because their predictions about events are exception-riddled and it is not possible to fully specify the background conditions under which they would be realised. While I have argued that the requirement for fully 'filled in' background conditions begs the question against cp laws, such a mistake is understandable if one views a law as a strict

regularity of event-types. If a law is just a description of a regularity of events then the only way to judge that law is at the level of events; a reason one finds very few strict laws in nature (Cartwright 1983). As the history of logical positivism has attested, this often leads to scepticism about induction and causation in the social sciences (cf. 1.3.2).

As this view of laws motivates scepticism, it is plausible that a different view of laws will affect how sceptical one is about conditional causal statements. And as laws relating to dispositions do not quantify over events, we have reason not to be as sceptical as if we consider laws as quantifying over events. One reason for this expectation is because the fact that dispositions are not always realised at the level of events is fundamental to our everyday existence; we cover the fragile wine glasses in bubble wrap when moving, at times of high demand for rented housing governments can impose rent caps and soldiers wear armor. If this is true and we have less reason to be sceptical, then we also have a reason to reject the problem of vacuity as being lethal because there cannot be a requirement to specify all possible conditions. Where this complaint was understandable against cp laws seen as conjunctions of events, it is not if cp laws are seen as power ascriptions. Kowalenko explains the association between powers and cp laws as follows:

> Given that the laws of nature cannot lie, any successful metaphysical account must allow that laws relate not to particulars, but dispositions. *Non-strict* laws fit comfortably with this world picture: dispositions are underlying states that remain constant across varied changes that are visible manifestations, and they are therefore ideal candidates for the true subject matter of *ceteris-paribus* laws (Kowalenko 2009, p.191).

Happy fit between cp laws and power ascriptions however is not an argument that one should accept powers. At this point I present a promissory note concerning belief in powers and I will cover this subject in detail in the next section. Though seeing cp laws as power ascriptions does not motivate the same level of scepticism when considering conventional cp laws, it also avoids the problem of vacuity in other ways. Unlike conventional cp laws, power ascriptions are not conditionals (though they entail conditionals). Cartwright claims that the power ascription 'smoking has the capacity to cause cancer',

> [I]s a precise claim. It states a matter of fact that is either true or not; it is not vague; and it has no *ceteris paribus* clause that needs filling in…More central to objections, it is testable, it makes predictions, and it entails regularities in the course of events, in this case statistical regularities' (Cartwright 2002, p.430).

Insofar as power ascriptions are not conditionals they are not vacuous. They do not idealise or abstract from other causal factors, they state that the antecedent has the power to cause the consequent. As such they do not have or require 'filled in' antecedents and are not directly vulnerable to the charge of vacuity. Still, it will be argued, power ascriptions are ultimately vulnerable to the problem of vacuity because they imply conditional statements: a power ascription that X can cause P implies that X will cause P in given circumstances. Mumford recognises this issue and responds by arguing that power ascriptions represent context-relative ideal conditions where the consequent would be met. I quote at length as Mumford raises a number of issues present throughout my discussion of causation and cp laws.

> Disposition ascriptions are made for a reason. The ideal conditions that facilitate the manifestation of the disposition are thus expected to be ones which are not realised only in exceptional circumstances. If such ideal conditions were exceptional, relative to the context of the disposition ascription, then there would be little utility in making the ascription…In making an appropriate and useful disposition ascription I am saying that, in ordinary conditions for the present context, if a particular antecedent is realised, a particular manifestation usually follows (Mumford 1998, p.89).

This might appear to fall back on the idea that conditional casual claims should be seen as statements of what normally happens (Silverberg 1996, Schurz 2002). I have argued against a probabilistic construal of cp laws and while Mumford's position has some ostensible similarity, there are differences that make Mumford's position tenable. A problem with probabilistic cp laws was that they appeared not to account for unusual events, but this is not the case with Mumford's position. If power ascriptions are context relative then it is not problematic to discuss what might happen in an unusual context, so long as the context is specified. Bear markets are not common, but we do talk about typical events and expectations within the context of a Bear market (wealthy long-term investors buy up vast amounts of what is deemed 'cheap' stock). Also, I argued probabilistic cp laws were non-explanatory as they only stated a probability and it is not clear how a probability is explanatory. Mumford's position is explanatory as power ascriptions require that we can tell an intelligible and cogent story about how a power can cause that which it is directed towards (see 2.3).

Mumford's position avoids the problem of vacuity by arguing that conditionals are met in normal circumstances relevant to a particular context. He argues that 'For any set S stating a list of such conditions, there is always the possibility of some interfering

condition C which renders the entailed conditional false…the set of ideal conditions can thus only be assumed to be unremarkable for the context of the disposition ascription' (Mumford 1998, p.92). This means that filled-in conditionals are beside the point, as each power ascription must be looked at on its own merits and within context. One might reject the notion of 'normal' or 'ideal' conditions as too vague, but the context relativity of such claims means that it is not possible, in a general sense, to specify 'normal'. Indeed to do such a thing is just what Mumford would reject as leading to vacuity.

It could be claimed that I have not offered a defence but a rejection of cp laws in favour of something else. To this I would respond that I am not concerned if we keep cp laws whilst understanding that they are power ascriptions or we drop *ceteris paribus* references to avoid confusion. Power ascriptions share enough in common with cp laws (they are transfactually efficacious, tolerate non-confirming instances, make a causal connection salient) that they occupy a similar place in the philosophical spectrum and so, I am not concerned what name is used. Rather, what is important is that we defuse problems and offer an understanding of causation in the social realm.

Turning back to linguistics, what might we provisionally conclude? In chapter 1 I argued that the Formalist Attitude offers an unsatisfactory account of language as an object of knowledge and fails to make it a sufficiently closed object. This inability to close off the theoretical domain of linguistics problematised the idea that linguistics be considered a natural science or a social science especially associated with the natural sciences. In 2.1 I argued that one reason for considering linguistics a natural science in spite of problems with the Formalist Attitude was due to pessimism about understanding social causality. In considering Lass's position with reference to language change, it was seen that Lass shared this pessimism, seeing social phenomena as inexplicable and not 'caused': something which led to him supporting the deductivist account of causation indicative of the Formalist Attitude.

To show that the pessimism of linguists is not justified, I have argued that one scientifically and philosophically popular way of understanding and expressing causality in the social realm, by means of *ceteris paribus* clauses and laws, is more robust than opponents of cp laws suggest. I have suggested that cp laws are best seen as power ascriptions, which is a plausible way of avoiding pessimism about causality in the social realm and in the next section I will do more to defend powers. I consider there to be little reason to think the Formalist Attitude a viable position in linguistics. As the social realm is not as a forbidding and unscientific a place as some may fear, it is reasonable to consider that such things as

speakers, speech, time and social structures have a place within linguistics: in explaining the nature of language, language change, knowledge of language and what kind of a science linguistics is. By the end of this thesis I will have answered these questions, but in light of my recent discussion of powers, I wish to consider the idea that language is itself a power or disposition of speakers.

2.3 Language as a power: An answer to 'What is a language?'

The question of what language is and what is the appropriate object of knowledge of linguistics has been central to this thesis. As has been stated before, the object of knowledge of a discipline determines to a considerable degree how a discipline sees itself in terms of its theoretical purview, what kind of science it is considered and the questions it can be expected to ask and answer. So far, I have argued for a way of seeing language where as Halliday once put it, 'instead of rejecting what is messy, we accept the mess and build it into a theory' (Halliday 1984, p.38). Such a view is not suspicious of social causation and recognises the importance of speakers and communication to language. This section continues this by asking the question 'What is a language?'

My argument moves in three stages. In the first part I argue that language is a power of speakers to understand and be understood by other speakers of that language and use criteria developed in George Molnar (2003) to support my position. I then show some advantages of this approach and argue that this power can be used as a basis for language typology which makes sense of the intuitive responses of speakers about the language that they speak. In essence I will argue when speakers talk about the language they speak, they are picking out those powers that they and others possess. In the second section I consider some problems raised by my position and in the third I consider some more advantages that make my position attractive. I will argue that as my position avoids problems and possesses several attractive features, it is a good answer to 'what is a language?' and in light of the failures of the Formalist Attitude, offers a reasonable alternative that is in keeping with taking speech and speakers seriously.

2.3.1 Language as a power

I claim that language is the power of a speaker to communicate[49] with another speaker of a

[49] I accept that there is more to communication than verbal communication, but use 'communication' here

language. A monoglot speaker of language La has the power to communicate with another speaker of La but not with a monoglot speaker of Lb, who has the power to communicate with other speakers of Lb. This is why we need subtitles in some films, why we might only be able to read *Don Quixote* in translation and why I once disappointed a Japanese woman, whose need for some kind of assistance fell on, if not deaf, then at least uncomprehending ears. Seeing language as a something akin to a power has been suggested before, albeit not in detail. For example, in *De Anima*, Aristotle discusses the knowledge of (Greek) grammar in terms of a potential to speak Greek (Aristotle 1987), Kenny (1984) discusses language as an ability to produce and understand certain kinds of behaviour and Morriss (2002) sees human "power' as a sort of ability' (Morriss 2002, p.48) and language among those abilities[50].

A preliminary point before I elaborate further. It may be objected that as I am providing a characterisation of what language is, I am begging the question because I am stipulating that two speakers have the same language when the question is how to characterise 'language'. Indeed, I am assuming that some speakers can communicate with one another and some cannot, but this seems such a fundamental observation that I am happy to assume it. What I am claiming in addition to this is that language is the power to communicate and I will be arguing that it is a power. There is an awkwardness which accompanies some power ascriptions to human beings which is not present in power ascriptions to inanimate objects. For example:

1) Ben has the power to run a marathon.*
2) Water has the power to dissolve sodium.
3) Clara has the power to hire and fire.
4) Mr Universe has the power to lift a family saloon car.

With 1 we feel more comfortable with 'ability', 'is fit enough' or 'can' in place of 'power' or 'disposition', though we are unlikely to have any scruple with 2, 3, or 4. I do not think the reason for this is of philosophical importance as we can see from 3 and 4 that power ascriptions can apply to agents without appearing odd. The issue then is one of convention and I see no problem in substituting 'power' in 1 for any of the other options as they all pick out a property within an object that has the potential to affect a change in the world.

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as 'verbal communication' because it is a prominent example of communication and clearly bears on language.

[50] See Morriss (2002, pp.24-25, pp.100-101).

What matters is how we characterise 'power', 'ability' or 'disposition'.

In what sense or senses can language be considered a power? In *Powers*, Molnar identifies five features of powers that will form the basis of the discussion of language as a power. These are:

- *Directedness*. A power must be directed towards some outcome or outcomes. A power is a power *to do* something.

- *Independence*. A power is not reducible to its manifestations and exists apart from those manifestations. A power exists even when not being manifested.

- *Actuality*. A power is not the mere possibility of a manifestation-event, but is actual in the same way in which common-or-garden objects are actual.

- *Intrinsicality*. Powers are intrinsically properties of their bearers. Molnar defines intrinsicality in this way:

> F is an intrinsic property of a iff a's having property F is ontologically independent of the existence, and non-existence, of any contingent b such that a is wholly distinct from b; and a's not having property F is ontologically independent of the existence, and of the non-existence, of any contingent b such that a is wholly distinct from b (Molnar 2003, pp.39-40).

- *Objectivity*. This has two parts. First, powers exist irrespective of our knowledge of them and second, powers are not reducible to our beliefs about them. This is especially interesting, and problematic, in discussion of psychological and social powers, such as bravery or being Prime Minister of the United Kingdom. In such cases there is a tension between the first requirement of objectivity and the need for a psychological or social power to be recognised by agents as a condition of its existence. I will return to this as it is crucial for my account of language as a power of speakers.

These features provide a useful set of criteria by which language can be judged a power. In this discussion I make good on my promissory note that I would provide an account of why one should believe in powers. The scope of this thesis does not allow for a

comprehensive account[51] though it will be made clear the broad advantages and plausibility of powers. Where I have specific issues with Molnar's treatment of powers I will make this clear and argue why I think that despite some difficulties, language can be considered a power and further why it is a suitable way to understand causal claims in the social sciences.

2.3.2 Aspects of powers

- *Directedness*

Powers are directed towards events: they make things happen. The consumption of alcohol is directed towards intoxication, honesty is directed towards truth-telling and candour, the brittleness of an object is directed toward that object breaking when suitably struck. The conceptual criteria of a power's identity consists in the manifestation of that power, be that through direct observation or inference from explanatory gaps in our observations (e.g. the postulation of the gravitational force of dark matter to explain the rotation of galaxies). It is a familiar tenet that without the manifestation of powers we would have no objects of knowledge at all (Mumford 1998). For example Searle, like other philosophers sees directedness as an important characteristic of cognition: 'intentional states and events are those mental states that are *directed at* or *about* objects and states of affairs in the world' (Searle 2009, p.10) and like Molnar, Searle understands directedness as a condition of conceptual identity. As language is a possession of individuals and plays an undeniable role in cognition it is then appropriate to see language within the framework of the intentional. But in what sense can language be considered directed?

There is a clear sense in which our pre-theoretical notions about language are consistent with the idea that its identity is constituted by its directedness (to what behaviours/events its manifestation pertains). If I can read the *Sunday Mirror* or give a lecture at the University of Surrey (outside of the department of languages), I do this partly in virtue of my speaking English and my speaking English is identified in my performing those kinds of behaviours. Language is used to communicate and we identify animals and some humans as not having language or a particular language by something they *cannot* do, inferred from behaviours that they do not exhibit. We can speak of language-possessing individuals as, 'not having Greek/German/Xhosa', implying that the individual cannot communicate with monoglot Greek/German/Xhosa speakers. This

[51] See Mumford (1998) and Molnar (2003). For debates around the more contentious issues within the theory of dispositions and powers, see Crane (ed.) (1996).

suggests that language has directedness in the same way as objects predicated with powers have directedness.

We only have language as an object of knowledge because it is available to us through acts of speech and writing, though one might worry that not all such acts are directed, because we sometimes speak without any communicative purpose. One possible way of defusing this worry would be to state that our understanding of language as an object of knowledge rests on the recording of very many acts of communication or would-be communication (imagine the English-speaking linguist in their office saying grammatically correct English sentences for research purposes). If powers are to be identified by their manifestations, then as all examples of language are communicative or would-be communicative it is reasonable to see language as directed. Things are not this straightforward though. Morriss notes a distinction between what he calls 'active' and 'passive' powers in discussing the difference between intentional acts of speaking one's language (an active power) and unintentionally understanding one's language (a passive power). According to Morriss, seeing language as ability (a non-natural power) has,

> the slightly unfortunate consequence that your 'ability' to understand your native language when it is spoken in your presence is not an ability at all, since you cannot *not* understand it…But nevertheless, you *do* understand your native language. By contrast, a monoglot Eskimo will not understand your native language. (Morriss 2002, p.100).

There is a distinction because the power to speak concerns the intentional *choice* of the agent where in understanding there is no choice. Therefore it seems that language is not directed towards a single event-kind, for to be identified in terms of its directionality, it needs to be seen as the power to speak *and* to understand. This suggests that language is an *object with powers* (like a sugar lump that can dissolve in water, be used as chemical energy etc.) rather than *a power of an object* and this is because the passive power of understanding and the active power of speaking cannot be reduced to one another. However, I do not think the idea that we understand unintentionally is as obvious and clear-cut as Morriss claims.

There are at least two ways in which it might be said I choose not to understand. I might stick my fingers in my ears and shout 'Not listening!' or as at various points in my school days, 'switch off' in class so that though I can *hear* the teacher speaking, I am not *listening*. The former example is not convincing as it is 'interference': analogously I might gag myself to stop myself speaking. This does not offer a genuine example of intentional

non-understanding, as I have prevented rather than chosen not to use my ability. In the latter example though it seems fair to say that I am choosing not to understand which is implied by the semantic difference between 'hearing' and 'listening'. Still, it is difficult to deny that we do understand our native language unintentionally or even when we do not want to. I have 'undergone' the opinions of racists when I would rather not listen and if I catch Meatloaf's 'Two out of three ain't bad' on the radio I understand its overwrought and sentimental depiction of unrequited love without a particular desire to do so. Also suggesting Morriss' distinction is correct is the existence of receptive and expressive aphasia, where individuals can either produce speech but not understand it or vice versa. What is clear is that the power to speak one's native language and understand it are connected in a more intimate way than the power of the sugar lump to dissolve and be metabolised to produce chemical energy. We can see this is the case because all aphasics speak and understand their native language *before* the onset of aphasia. This is how aphasia happens and part of the definition of receptive and expressive aphasia is that one loses part of one's language ability or 'system' (Evans and Green 2007, p.745). We cannot learn to speak without learning to understand or vice-versa and my example of the hearing-not-listening student supports the idea that the active/passive distinction is not absolute. All this suggests that language is a power rather than an object with powers.

Another possible objection is that I am allowing language to be assimilated into intentional states but that it is intentional states, not language *per se,* that possess directionality, even if language possession is important or essential for some intentional states. If language does possess directionality, then it is directed toward a very large set of events and objects, perhaps every object or event that an intentional state is about. This would make it directed in a vague sense because it is directed to no manifestations in particular. While I would agree that we never speak for the sake of language (see 3.2) and so manifestations are not directed towards what we could term 'English/Xhosa situations', that does not stop manifestations being in some sense about English or Xhosa. But in what sense? In the sense that an English sentence is not about Xhosa and a sentence in Xhosa is not about English. That is, a language is directed to acts of communication with other speakers of the same language and when it is successful this is partly because speakers share a language. While the directedness of languages still appears broad, it is not trivial for we can identify manifestations of English from manifestations of Xhosa, even when manifestations of different languages are about the same subject.

It is usual that opponents of powers do not focus their attacks on the directedness

of powers because directedness itself presupposes much of the metaphysics of powers. To give a fuller account and defence of the directedness of language requires discussion of questions of independence and objectivity.

- *Independence and actualism*

Independence is the thesis that powers exist apart from the manifestations they are directed toward or disposed to produce. It follows from the thesis of independence that a power may exist which is never manifested. Actualism rejects the thesis of independence and there is a long tradition of realists since Aristotle that have defended independence against actualism. The independence of powers is rejected by actualists because there is a worry that as everything that exists is actual, independence permits unmanifested powers to exist while being non-actual. Such unmanifested powers present, it is argued, something both mysterious and ontologically superfluous[52]. To effectively show why actualism is false, I want to separate and define a 'strong' and a 'weak' actualism. The arguments I present in discussion of independence refute weak actualism and the arguments I present later in discussion of objectivity refute strong actualism.

> *Weak Actualism* (WeA): Powers exist only when being manifested. If the power P of an object O is manifested at time t then O has power P at time t. Ascriptions of power and statements of manifestations of those powers are isomorphic.

> *Strong Actualism* (StA)*:* Powers do not exist, only happenings.

Hume espoused WeA:

> [W]hy, in other instances where those qualities have appeared, do you presume that the same power is also there? Your appeal to past experience gives you no help with this. The most *it* can prove is that *that very object* which produced a certain other object was *at that very instant* endowed with a power to do this; but it can't prove that the same power must continue in the same object (Hume 1739-40 [2004] p.55).

Weak Actualism is consistent with the reality, but not the independence of powers. In addition, WeA implies the impossibility of countervailing powers preventing the

[52] A contemporary presentation of these concerns can be found in Karen Bennett's article 'The Two Axes of Actualism' (2005).

manifestation of other powers as powers would have to exist at a time before their manifestation and this is rejected by WeA. On the other hand, strong actualism is consistent with neither the reality nor the independence of powers. This being the case, the term 'manifestation' is not appropriate so I have opted for the neutral 'happenings'. It is not clear if StA implies the impossibility of causal interference, though if StA is to have a coherent account of causality it would need to countenance counterfactuals. StA relies on what Molnar calls the 'thesis of deductivism', which I will deal with when discussing objectivity. Here though it is enough to state that if deductivism is false, strong actualism is not a viable position.

It is quite natural to explain the fact that I smell the roses at time t_1 and then at time t_2 by virtue of my olfactory senses, which are present between t_1 and t_2, though may not be active during that interval. This presents a challenge for WeA but not for StA, for the strong actualist can claim that rose-smelling at t_1 and t_2 is the result of a psychological imposition of order on unconnected events. StA then becomes a question for the objectivity of powers, not of their independence and so I will deal with StA later. For WeA however, the example demands an explanation. WeA holds that powers are intermittent, existing only when manifested. This is an odd position because it is unclear why the advocate of WeA needs to talk of powers at all, as the distinction between powers and manifestations seems to have collapsed. Part of what makes powers philosophically attractive is that they explain the occurrence of iterable events and as WeA denies that powers are independent, powers lose this attractive feature. So essential is independence to the understanding of powers that it is unclear if powers are being discussed at all in WeA. Whatever the case, WeA appears incapable of explaining iterable events and also offends against the intuition that causation is always linear. If powers are the causal base of their manifestations then it follows that they exist before the manifestations that they are the causal base of. WeA claims that power and manifestation are coterminous and so violates this requirement.

Another issue concerns how WeA deals with our intuitions about causal interference. Consider a patient undergoing chemotherapy. We know through a large body of double-blind studies that one of the side effects of chemotherapy treatment is sickness and nausea. To prevent sickness and nausea, it is normal for a doctor to prescribe a course of anti-emetics. Now let us say that the patient undergoing chemotherapy suffers from sickness before they take anti-emetics and the sickness ceases or significantly reduces after beginning the prescription. We are likely to explain this by reference to the anti-emetics and

this is consistent with the thesis of independence. This explanation though is unavailable to WeA because according to it, once sickness is no longer manifest, there is nothing for the anti-emetics to suppress. At the point where sickness stops there is also no reason to believe that chemotherapy has the power to cause sickness and nausea, as that power is not actual (though it might be in the future- who can tell?). It follows that the patient can now stop the course of anti-emetics without particular fear of the onset of sickness and nausea. Presumably, the anti-emetics themselves have also now lost their power to suppress sickness and nausea because although the patient is not nauseous, there is nothing with the power to cause sickness and nausea currently being manifested. If this is the case, there is another reason to stop taking anti-emetics as its power to prevent sickness is unmanifested and hence for WeA, not a power.

The issues raised here are mirrored in linguistics and the philosophy of language and have a direct bearing upon my claim that language is a power. This can be seen in the argument concerning the reducibility of knowledge of language to the ability to speak and write (and presumably understand). Among those who believe that knowledge reduces to ability are Dummet (1993) and Kenny: 'to know a language is just to have the ability to do these and similar things' (Kenny 1984, p.138). Here it is useful to distinguish Kenny's position on 'ability' from my own on power, as they bear some resemblance. Kenny gives language directionality, but refuses it independence from its manifestations in speech. For Kenny, it is important that language is used to communicate, but this does not permit us to make claims about non-sensible properties (knowledge of language) putatively possessed by the speaker[53]. He may therefore justifiably be seen to hold WeA. Against this view Chomsky argues,

> Suppose Jones, a speaker of some variety of what we call 'English' in informal usage…loses this ability because of an injury or disease (then recovers that ability, say, with a drug). Note that a speaker of "Japanese", under the same circumstances, would recover *Japanese*, not English, with the same drug (Chomsky 2000, p.51).

Chomsky's point against Kenny is analogous to arguments I have made against WeA above. For Kenny, it appears difficult to explain the situation Chomsky has described without reference to a distinction between knowledge and ability, analogous to power and manifestation respectively. It is not clear what Kenny means by 'ability': '[it is] plainly not ability in the quite useful normal sense of the word' (Chomsky 2000, p.51) as Kenny does

[53] This is discussed in more detail in 3.3.

not countenance a distinction between ability and the manifestation of that ability. Here too the issue of interference is present, for while Chomsky sees language as independent in a power-relevant sense (as it can be masked by injury or disease), Kenny is committed to refusing that Jones 'knows' English, which fails to explain what has happened when he recovers. Though Chomsky is anti-actualist, it would be wrong to see him as an advocate of language as a power because he does not see language as a system for communication. Chomsky does not argue that language is a power, only that a distinction exists between knowledge of language and the ability to use language and he rejects talk of 'knowledge-ability' as a needless addition to technical vocabulary (Chomsky 2000, p.52). In stark contrast to Kenny, Chomsky sees what he calls language as having independence, but not directionality.

While I think both positions are incorrect, Chomsky's 'knowledge' and Kenny's 'ability' are not mutually exclusive, but from a powers point of view, quite compatible. The irreducible cognitive element of linguistic knowledge that Chomsky wishes to preserve can be seen as part of the power of a speaker to communicate and the denial of it by Kenny raises the same problems for him as for WeA. On the other hand, the view that 'language is not properly regarded as a system of communication' (Chomsky 2002, p.76) requires that knowledge is 'completely divorced from ability' (Chomsky 2000, p.51) and unless we recognise, as Kenny does, the importance of communication, we are left with the problem of how one identifies language at all if not by its manifestations.

- Actuality

There is a clear sense in which powers are actual and not the mere possibility of particular manifestations. Molnar argues, '[w]hat is not actual cannot be a cause or any part of a cause. Merely possible events are not actual, and that makes them causally impotent' (Molnar 2003, p.101). Therefore if powers are causally potent, they have to be actual. The actuality of powers is reflected in the way we talk about directedness. Powers are *for* things and *to do* things, i.e. to causally contribute to events through their manifestations. The actuality of powers makes it possible that such-and-such events happen, but this does not warrant the conclusion that powers reduce to mere possibilities because if this were the case, powers would be impotent.

What goes for powers generally goes for my thesis that language is a power and so I need not spend much time on this here. By defending powers and the claim that language

is a power, the actuality of language is implied. Actuality is implied by directedness, independence and objectivity (discussion forthcoming) and as such, does not require special attention if my stance on independence and/or directedness is correct.

- Intrinsicality and relations

A power P is intrinsic to an object O only if it does not rely on being in relation to another object or power in order to exist. This is the intrinsicality condition. Something is extrinsic if it relies on such a relation, be that between a single object or multiple. Molnar's position is that all powers are intrinsic and that what may initially look like extrinsic powers are reducible to intrinsic ones, so a power can never be intrinsic and relational. A well known candidate for an extrinsic power and one that Molnar discusses is accredited to Boyle (1666). This is the example of the relation between the key and lock. It is in virtue of the existence both of the key and the lock that they have a power, namely for the lock to be opened by the key and for the key to open the lock. The power of these objects appears to rely on a relation with another object in order to exist and so do not meet the intrinsicality condition. To avoid this conclusion, Molnar argues that in order for the key and lock to bear the relation they do, a relation that bestows a disposition upon both objects, there must be congruence between the objects. He explains,

> Congruence is comparative. Comparatives are founded relations that supervene on properties of the relata. The properties of the lock and the key that found their congruence are the very ones that explain why they can interact in the functionally appropriate way (Molnar 2003 p.105).

Therefore putatively extrinsic powers have their ontological base in intrinsic powers:

> All dispositional and extrinsic predicates that apply to an object do so by virtue of intrinsic powers borne by the object. All truths about the powers of objects have only intrinsic properties as truthmakers (Molnar 2003, p.109).

In explaining and defending the intrinsicality of powers, Molnar adopts a deflationary account of extrinsic powers that is open to criticism. Molnar conjectures, but does not argue, that 'the class of actual powers does not include any the having of which by one object depends on that object standing in some relation to another object' (Molnar 2003. p.108). There are no truly extrinsic powers and talk of them is due to the pragmatic benefit they have in our everyday talk. This position is in keeping with the intrinsicality condition

as stated by Molnar but it reveals a tension between the intrinsicality condition and Molnar's deflationary account of extrinsic powers. The tension concerns how truths about powers can have only intrinsic truthmakers when at least some powers appear not to fulfil the intrinsicality condition.

To return to the lock-and-key example. Whatever the intrinsic powers of key K, its power P to unlock lock L is reliant upon the existence of L and a relation of congruence between L and K. This Molnar accepts, but stresses that such congruence is founded on the intrinsic powers of K and L. But the power of K to open L requires K standing in a particular relation to L in order for it to possess P which the relation describes. The statement 'if L had not existed, K would not have P' is true and if this is the case then K (and L) fail the intrinsicality condition and so cannot be truthmakers for truths about powers, because those powers are not intrinsic. We have an example of something that looks like a power but we are not permitted to conclude this because of Molnar's intrinsicality condition. There seems to be no obvious alternative account of the true statement 'K can open L' without reference to a power that is possessed by another object, and this breaks the intrinsicality condition. Now, this is not just a recapitulation of Boyle's argument for the existence of extrinsic powers, for I accept Molnar's argument that extrinsic powers are founded on intrinsic powers. The problem is that we appear to have a case where the intrinsicality condition fails to characterise all powers.

I think this problem stems from an assumption held by Molnar that a power cannot be both intrinsic *and* relational. This subject is taken up in Westphal's article on Hegel's 'Force and Understanding'. Westphal points to two senses of 'intrinsic' that he argues are systematically conflated in talk about powers:

> One use of the term 'intrinsic'…is to designate a characteristic which is essential to a substance, so that the substance would not be what it is without that characteristic. Another use of the term 'intrinsic' in this connection contrasts with 'extrinsic' in the sense of 'relational'. In view of this contrast, an 'intrinsic' characteristic is contained solely within the individual substance; it is non-relational…conflating them [the two uses of 'intrinsic'] generates the standard assumption that relational properties cannot be essential to individual substances (Westphal 2008, p.2).

We can characterise two kinds of intrinsicality as follows:

IntA: F is an intrinsic property of *a* iff being F is constitutive of the identity of *a*.

IntB: F is an intrinsic property of *a* iff F's being F does not require that it stands in relation to any other object or property distinct from *a*.

As Westphal makes clear, one can hold IntA without also holding IntB and this is not obviously philosophically contentious. Molnar distinguishes between IntA and IntB and so is not guilty of the equivocation or conflation suggested by Westphal. He recognises IntA in his definition of an *essential* property (Molnar 2003, p.39) and his recognition of IntB is implied both by his support for IntA and his position that there are no truly extrinsic (relational) powers. Again: 'the class of actual powers does not include any, the having of which by one object depends on that object standing in some relation to another object' (Molnar 2003, p.108). Molnar then is committed to denying that a power can be intrinsic *and* relational and so supports both IntA and IntB, though he does not conflate them.

My disagreement with Molnar is twofold. First, there is a good argument against it being the case that an intrinsic power cannot be relational and second, accepting that an intrinsic power can be relational (accepting IntA but rejecting IntB) has philosophically attractive qualities. Westphal argues that if intrinsic properties are *not* relational then it becomes 'impossible to understand causal necessity' (Westphal 2008, p.3), a serious charge. His argument goes like this. Powers are essential to objects, but in being manifested no power acts alone and therefore *all* causation is interaction (being reliant upon and acting with the powers of other objects). If one holds that intrinsic or essential characteristics cannot be relational (IntB) then one cannot explain causal necessity as all causation appears contingent because all causation is interaction. This idea is familiar in discussion of powers and is sometimes referred to as 'reciprocity'[54]. The thesis of reciprocity states that a power's directedness to a manifestation is not solely a result of facts about the object that the power can be truly predicated of. As Martin puts it, 'the manifestation of a given dispositional state will require the cooperation of some other dispositional states amongst its reciprocating partners' (Martin in Bacon (ed.) 1993, p.183). It is certainly hard to describe a power without explicitly or tacitly assuming the power of something else. The power of aspirin to relieve headaches is not simply a power of aspirin; it requires that such-and-such be the case about human physiology, in this case the power of the enzyme cylooxygenase-2 to bind with acetylsalicylic acid, the active ingredient in aspirin. The power of governments to stimulate consumer spending by cutting taxes relies (among other things) on people's ability to spend. Needless to say examples of reciprocity abound.

[54] For example in Mumford (1998, p.127n) and Place (in Crane (ed.) 1996, p.117).

Accepting that powers can be intrinsic and relational is attractive in a number of ways. Following Westphal, it stops our concept of causal necessity being obscure. Also, acceptance dissolves any tension between the intrinsicality condition and extrinsic powers because we have dropped the intrinsicality condition (IntB). To repeat, the intrinsicality condition is the idea that a power P is intrinsic to an object O only if it does not rely on being in relation to another object or power in order to exist. We can accept, *contra* Boyle, that truthmakers for propositions about powers are intrinsic but that things true about the powers of object O are true only in virtue of the intrinsic properties of some set of objects ($O…O_n$) which includes objects that are *not* O. If we accept, *contra* Molnar, that the intrinsicality condition is false because powers can be intrinsic and relational (which the intrinsicality condition denies), then we avoid the problem of falsifying the statement that all truths about powers have intrinsic truthmakers.

My position is this: All truthmakers about powers have intrinsic truthmakers (here I agree with Molnar) but all the true propositions about the powers of object O are not exhausted by reference to the intrinsic powers of O. The intrinsic powers of other objects can be truthmakers for truths about the powers of O and vice versa. We can maintain Molnar's position that relational powers are founded in the essential properties of objects, but deny that relational powers are reducible to the essential properties of *single* objects. This improves on Molnar's position and is consistent with its general thrust, giving us the best of both worlds: powers are linked to the nature of objects and we can also accept the intuitive appeal of the idea that there are powers that exist in virtue of a relation or relations to another object or objects.

Part of my motivation for arguing that powers can be intrinsic and relational is because my definition of language implies that it is a relational power and if one accepts the intrinsicality condition, then my understanding of language would not qualify as a power. The idea that speaker A with property P only has the power to communicate with speaker B if speaker B also has that P makes this clear. Language is a power where a relation holds between some speakers and not between others and this relation holds due to facts that are true for sets of speakers, not for a single speaker. If my criticism of Molnar is correct then language possession can also be seen as an intrinsic property of speakers. The criticism defuses a tension in Molnar's theory that does not come at a high price for the theory globally, so my using of Molnar's criteria for powers is not jeopardised.

-Objectivity and the ontology of social entities

X is objective if it does not rely for its existence on any actual or potential knower of X. Defences of the objectivity of powers in the face of empiricist attack are common and I do not intend to offer a defence of what has been said better elsewhere (Collier 1994, Crane (ed.) 1996). I want to offer something more modest and pertinent to my argument that language is a power that involves saying something about powers that are non-natural or social. This is important because like Morriss (2002), I see language as a social power.

In order that I can go on to argue for a distinction between natural and social powers, I first want to establish that there are objective natural powers. I will only focus on rebutting anti-objectivist arguments against powers that make the criticism that natural powers are anthropocentric concepts, not objective properties. I do this because social powers putatively differ from natural powers in that social powers are 'human-made' and so without a defence of the non-anthropocentric status of natural powers an account of social powers which did not immediately submit to an anti-realist or sceptical position would be difficult to sustain. The position I wish to attack is accredited to Hume but has a number of modern adherents (Van Fraassen 1980, Rorty 1998). The claim is that our causal language and assumptions about causal connections between objects and events make the world intelligible *to us*, but causation cannot be validly inferred from the succession of events given to us by experience[55]. This is also the case for our belief that we as agents are possessors of powers. Powers are not objective but rather are anthropocentric projections; necessary for intelligibility but illusory. Hume argues,

> An act of volition produces motion in our limbs, or raises a new idea in our imagination. This influence of the will we know by consciousness. Hence we acquire the idea of power or energy; and are certain, that we ourselves and all other intelligent beings are possessed of power […] But the means, by which this is effected; the energy, by which the will performs so extraordinary an operation; of this we are so far from being immediately conscious, that this must for ever escape our most diligent enquiry (Hume 1975 [1777], pp.64-65).

For Hume, it is not only in our experience of the external world, but also in our experience of our own consciousness that we are unwarrantedly led to be 'certain' that there are such things as powers because the way we reason about both is the same. In light of this, for Molnar, the challenge for the supporter of powers is this:

> To deduce power statements from observation statements one would need

[55] I noted in 1.3 that this was a motivation for Chomsky's postulation of UG and I-concepts.

> additional premises, but these are not available from observation alone. Consequently, we have no (purely) experiential grounds for asserting singular causal statements, or for prescribing powers to particulars on a single occasion (Molnar 2003, p.118).

Molnar offers an attack on Hume by pointing out that Hume accepts the Thesis of Deductivism (TD), exposited by Stove. Stove explains TD like this:

> A being who knew only the premises of an invalid argument…would never, if he were completely rational, invest the conclusion with any positive degree of belief at all. All invalid arguments, that is, are completely irrational: or there are no different degrees of conclusiveness or reasonableness among invalid arguments (Stove 1970, p.77).

TD is the thesis that a judgment is only rationally supported by that which entails the truth of the judgment. Molnar's approach is not to attack TD, but rather to show the destructive consequences of its application to various questions and argue that TD blocks the employment of some important forms of argument. To use one of Molnar's examples,

> P1) If the past is knowable, then it is knowable a priori or it is knowable a posteriori.
> P2) The past is not knowable a priori.
> P3) Nothing in our observations entails the reality of the past.
> P4) (TD) A judgment is only rationally supported by that which entails the truth of the judgment.
> C) The past is not knowable. (Molnar 2003, p.124).

Accepting TD means that we are compelled to throw the baby out with the bathwater as TD is 'such an unrestrained generator of sceptical conclusions that it violates the principle of selective anthropocentricism' (Molnar 2003, p.124). If we consider this alongside my argument that Hume assumes something like powers (iterable acts of volition) in supporting his sceptical position, then we have good grounds to reject TD. We can reject the argument that powers are not objective and that they are anthropocentric projections. We might accept the objectivity of natural powers, but there are problems for allying my argument that language is a power with the claim that it is objective as there is a serious question as to whether social powers are objective. When considering objects of natural science such as gravity and the elements as paradigmatically objective entities, there are a

number of differences between these and putative social entities that pose questions about the objectivity of social entities. Here again is a standard definition of 'objective':

> Ob: x is objective if it does not rely for its existence on any potential or actual knower of x.

In light of this definition, social entities and social powers are not objective because a social power is something which is maintained by mutual belief and entails the existence of people, so social entities and powers are strongly anthropocentric[56]. Social entities falsify the conditions for objectivity as their existence relies upon epistemic facts about human beings. To illustrate, let us say that an alien race puts the world to sleep and then wipes everyone's memories of the concept 'marriage'. The aliens then remove any reference to marriage in literature, painting, the legal system, etc. When the world awakes, what reason would an independent observer have to think that marriage exists in the world? It is hard to resist the conclusion that marriage would have ceased to exist because humans no longer have the concept 'marriage' and its accompanying social relations (though it may have been invented again by lunchtime). Not accepting this and maintaining that marriage does still exist would lead to an unattractive ontological profligacy as it implies that concepts exist which have never been known. This just does not seem to be what we mean by 'concept' and it collapses the distinction between natural and social powers and leaves one without a principled way of defining which social entities do and do not exist.

What follows for social entities and powers from not meeting conditions for objectivity is that they are ontologically dependent upon the continued existence of human beings as well as our knowledge of them (see 4.2). Social objects, as well as differing from natural objects in not existing independently of our conceptions of them, do not exist independently of humans *simpliciter*. This though is not damaging to my argument that language is a power. To be damaging it would have to be the case that social powers were *merely subjective* and possess little or none of the qualities we associate with objectivity. If social powers were merely subjective then they would be open to limitless re-definition and it would be impossible to distinguish one power from another. Indeed, failure to distinguish one power from another would mean that we have no definitions available to us in the first place. The fact that people communicate, get paid wages, pass GCSEs, get married and vote in elections all refute this.

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[56] See Molnar (2003, p.112).

If social powers are neither objective in the way that gravity is nor merely subjective, what are they? As well as the definition of Ob above, there is another sense in which objectivity can plausibly be considered:

Ob_1: x is objective if x is not reducible to beliefs about x.

What this requires for a social power (in this case language) to be objective is that it display a degree of autonomy from the agents whose existence and beliefs are required for it to exist. It implies that agents' beliefs about a social power or entity are fallible and that social entities provide criteria for success and failure for particular behaviours. I share the opinion of a number of philosophers (Bhaskar 1979, Itkonen 1983, Searle 1995) that social entities or systems of rules maintain a degree of autonomy and as such social/institutional facts can be considered objective in the sense of Ob_1, if not in the sense of Ob. The case for the autonomy of social entities and powers can be argued, in Durkheimian fashion, by pointing to the constraints the entity puts upon agents. Itkonen claims that 'the intersubjective existence of goals and beliefs is shown precisely by the existence of controlling and controlled behaviour' (Itkonen 1983, p.188) and this can be seen in the following example. If in 2012 I try to pay my greengrocer with one pound notes, I will be frustrated. This is explained by the fact that one-pound notes are not part of the currency system of the UK. My intended action of purchasing celeriac is constrained by facts about the currency system and no matter what my beliefs about the purchasing power of one-pound notes, it is not the case that I can buy celeriac with them. This is relevant to the explanation of why I would have been successful in such an action before the 12th November 1984, the date when pound notes were removed from circulation. At different places and times, agents are enabled and constrained by the rules that govern the social entities which they sustain and this suggests they have the degree of autonomy described by Ob_1.

Within linguistics, the autonomy of language as a social entity has been most emphasised by the Saussurean or structuralist tradition[57]. As Saussure put it, 'language constitutes a system…precisely because of this, the community is unable to change it at will' (Saussure 2006 [1922], p.107). As with other social entities, language is not reducible to beliefs about it. For example, if I believe I can express in English specific or particular nouns by using the pronoun 'a/an', I misunderstand English and attempts to act on this

[57] See 4.2 for discussion of the relationship between Durkheim and structuralism.

belief are likely to fail. Language is not reducible to beliefs about it because it is linguistic *behaviour* that constitutes the conceptual criteria for someone being a speaker of a language. The English speaker cannot have the rational belief that they do not speak 'English'. It is on pain of performative contradiction that I say 'I don't speak English', and if this is my belief, it is *ipso facto* an irrational belief. In deciding who is a speaker of a language, we take what someone can do (speak fluent English) as the criteria, not what they profess to or in fact believe. Again this does not mean that agents can hold any belief whatever about a social entity, for social entities are not merely subjective for the reasons given above. As Searle puts it, in identifying social entities and powers, 'it seems almost to be a logical truth that you cannot fool all the people all of the time' (Searle 1995, p.32).

I have now shown how language is a power by considering it with respect to some recognised features of powers. Language is directed, actual, independent, intrinsic to speakers (*and* relational) and in a qualified sense objective (Ob_1). Language is a power to speak a specific language, so when one speaks one does not speak *language*, but English, Greek, Xhosa etc. By seeing language as a power of individuals in virtue of their ability to communicate with another speaker of that language, the relational and social aspects of language are recognised without losing sight of the fact that language is used by individuals. I now want to turn to some of the problems and advantages for my understanding of language.

2.3.3 Some problems with powers

I wish to head off criticism of my position that language is a power by looking at three possible problems. These are: a problem in defining languages if intelligibility can be asymmetric, a potential conflation of manifestation and event in consideration of polygenic events and a question about the necessity of manifestations of social powers leading back to actualism.

- Mutual and asymmetric intelligibility

For my thesis, mutual intelligibility (MI) is important in defining what a particular language is, as language is not an undifferentiated power to communicate, but a power to speak and understand a particular language[58]. MI is the relation that holds between speakers of the

[58] See Harris (1981, pp.3-13). Here Harris discusses confusions associated with the failure to distinguish

same language. Seeing languages as separated by the mutual intelligibility of speakers is an intuitive way in which non-specialists distinguish languages, as well as linguists. MI is a neat way of defining languages and language powers, though it meets a problem when we consider asymmetric intelligibility. Take Jensen's (1989) work on the intelligibility between Spanish and Portuguese, an important background factor of which is that Portuguese speakers are usually deemed to have significantly higher comprehension rates of Spanish than Spanish speakers have for Portuguese (Jebsen and Biel 1986). This is known as asymmetric intelligibility. Jensen finds that 'Spanish and Portuguese are mutually intelligible, but at a level of only about 50% to 60%' though this is qualified by his 'support [for] the common belief that Portuguese speakers understand Spanish better than vice-versa' (Jensen 1989, p.851).

This asymmetricality of intelligibility raises a problem for my argument as it seems that 'intelligibility' is not as clear cut a concept as I have assumed. I require that a language have at least two speakers who share rules and/or abilities necessary for communication and that the power to communicate is relational. In the Portuguese/Spanish example it appears there is no consistent way to employ MI to distinguish languages as the Portuguese/Spanish example does not give us grounds to decide if Spanish and Portuguese are the same or different languages. If this is the case then we cannot decide if we are dealing with a single power or two powers and so the directionality of the power appears ambiguous and confused. This is not a problem however. It would powerfully speak against my thesis if two languages (P, Q) were found where a monoglot speaker of P could understand philosophy lectures and poetry in Q, but the monoglot speaker of Q would be unable to understand simple requests, statements of affirmation and other interlocutory staples in P. No such situation exists however and as Jensen makes clear, Spanish and Portuguese speakers *do* have the power to communicate, albeit the situation is slightly different for the respective speakers. Such examples (see also Lind 1972) support the notion of a dialect or language continuum, where intelligibility is scalar and that there is such a continuum does not threaten my view, because I am not supposing that powers be entirely distinct and so asymmetrical intelligibility is just a limit case of MI. Grey areas are found in many places, not just in language.

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between language-in-general and specific languages. This is because 'language' can be used as a plural or singular term, which 'sponsors the confused notion that language is knowable' (Harris 1981, p6). However, in contrast to my position, Harris also rejects the concept of 'a language' as a starting point for linguistic enquiry. For Harris, such a move supports the 'fixed code' fallacy, which is characteristic of the modern linguistic orthodoxy he rejects.

- Language and polygenic events.

Manifestations of powers are typically distinct from events. If Ben is riding his motorcycle down the road, there are many powers being manifested: the combustive properties of petrol, Ben's balance and co-ordination, the gravitational pull that keeps the motorcycle firmly on the road and so on. This is important to recognise lest we confuse manifestations with events. Molnar explains an aspect of polygeny that leads to a worry for me,

> [T]he causally operative sufficient condition for almost any manifestation is complex, and its parts are bound to include circumstances that are extrinsic to the bearer of the disposition. The first consequence of equating the causal base of the disposition with the complete cause of the manifestation is that we lose intrinsicality (Molnar 1999, p.3).

Molnar is here making two points, one of which is repeating his position that powers cannot be intrinsic and relational, which I have already discussed. The other point he is making is that causal complexity at the level of events (Bob rides his motorbike, Carol buys an Italian bond, a meteor passes earth) is such that a variety of powers be manifested in any event and this can make conceptual or empirical separation of powers difficult. One may then worry that in taking language to be a power there is confusion between event and manifestation, that I am conflating a communicative event with the manifestation of linguistic power.

The worry is misplaced though. I have argued that language is necessary for a communicative event to take place, not that it is sufficient. A communicative event, like any event, will consist of many manifested powers and by allowing that a communicative event is a complex of powers of which language will be part, I am not confusing event and manifestation. Whether two speakers communicate about cricket, the hustings or metaphysics is reliant upon a host of factors, including the rational objectives of the speaker in a given situation (see 3.2/3.3). Strongly anthropocentric powers such as language are to be viewed in broad terms as the social realm is causally open, experimentally unclosable and the directionality of a social power makes it conceptually necessary that it be connected to a social event which will always be polygenic. That power ascriptions in natural objects are more specific and determinable than those of social objects is hardly surprising due to the potential for causal closure in laboratory experiments. To ask then for comparable specificity of social powers with those of the natural is not reasonable.

- Necessity of manifestations

Realism about powers implies that there can in principle be powers that are never manifested as well as powers that are manifested but never known. It is a corollary of my discussion of social powers that this cannot be the case as social powers rely on the existence of people and on people having some concept of that power. Language then requires reproduction and mutual belief in order to exist, something I will say more about in 4.1 when considering the relationship between agent and language structure.

What this suggests about social powers is that it is necessary that they be manifested in order to exist, for without manifestation they cannot be known. This leads to the worry that the standard realist distinction between ontology and epistemology does not hold for social powers. It also leads to a question about whether social entities cease to exist when not being manifested. Here the question of actualism appears again, for if social powers rely on manifestations to exist, one can argue they do not exist when not being manifested and this suggests WeA. I have already attacked WeA and to avoid inadvertently adopting it, I need to show how social powers are compatible with realism. Let us compare the window tax and the higher rate of income tax in the UK, which is currently 45% for income over £150,000. We have the intuition that the window tax does not exist but the higher rate of income tax does and that it does, does not depend upon the higher rate of income tax being manifested. If no one in the UK earned over £150,000 then the Inland Revenue would not be taxing any income at 45%. One might argue that if this was the case, then the window tax and the higher rate of income tax would be on an ontological par as neither is manifested and so we have no reason to believe there is a difference. This is unconvincing because while both taxes have conditions of satisfaction, only the higher rate of income tax has the *potential* to be manifested because it is recognised within the UK tax code.

If the higher rate of income tax did not exist when not being applied to income, for example on bank holidays, then it would be impossible to explain why it factors in agent's judgments and why Conservatives desire to reduce it or why people try to avoid it by exploiting tax loopholes. Likewise, what makes the difference between ignorance and sound judgment in those who make and design buildings without reference to the window tax comes down to whether the window tax has the potential to be manifested. That it does not have the potential to be manifested is reflected in the lack of concern for it or reference to it when designing or building. One might argue that my example does not

show that the window tax lacks the potential to be manifested and that it would only take the election of an 'anti-window' government to show that the window tax does have such a potential. To this I would respond that no-one acts with consideration of such a potential window tax in mind, which suggests that the 'potential' of higher income tax to be manifested and the 'potential' of the window tax to be manifested are different. Social powers then have the potential to manifest themselves, but it is not necessary that they manifest themselves to be considered a power. If they did not exist apart from their manifestations, then we could not include them in our decision making. There is therefore no danger of a slip into actualism and social powers are broadly consistent with realism. Having dealt with several potential problems, I want to look at some of the benefits of seeing language as a power and of a powers approach generally.

2.3.4 The perks of powers

Let me conclude by briefly discussing the advantages of my position for linguistics and for thinking about language generally. Some of these have been touched on before, but I bring them together here for clarity.

-Vindication of Social Causation

In 2.1 I claimed that linguists are often suspicious of taking social causation and social factors seriously in their endeavours to understand language and in 2.2 I tried to defeat this suspicion by considering one way in which social causation is explained, namely by the use of *ceteris paribus* laws. While cp laws were more robust than opponents deemed them to be, they were still vulnerable to the charge of vacuity. This was avoided by interpreting (some may claim discarding) cp laws as power ascriptions. As my powers account of causation avoids the problems of a deductivist account and the problematic aspects of cp laws, it can therefore justifiably obviate the suspicion of linguists about social causation.

It follows from my stance on powers in the social sciences that language is a power. It should be noted how different my view of language is from rival views expressed by those ascribed to the Formalist Attitude. My view of language does not require that language be the kind of strictly delimited object required by the Formalist Attitude to maintain sufficiently 'scientific' study; it is neither a closed synchronic system or an internal

'dictionary of which each individual has an identical copy' (Saussure 2006 [1922] p.38) (structuralism) nor a universal, innate and exceptionless series of rules to which all 'languages' reduce (generativism). It avoids seeing language as what Peter Jones has described as 'an abstract, self-contained system of forms, meanings, and rules whose existence is the precondition for successful acts of linguistic communication, any such act being the mere realization or expression of elements or rules in the system' (Jones 2007, p.338). In seeing language as a power, the linguistic behaviour of speakers is foregrounded rather than occluded at the expense of the 'self-contained system' and by understanding language as a strongly anthropocentric object, its operation within an open causal system is explicitly admitted.

-Actual Speech and Social structures

In 1.3, I criticised Derrida and Saussure for their unjustified exclusion of what I termed 'Actual Speech' from the purview of linguistics. To recap, I defined actual speech in this way:

> Actual Speech: A spoken linguistic act A which, belonging to a language L reproduces and potentially changes, language L.

By foregrounding communication in my understanding, it is clear that my view of language as a power does not exclude Actual Speech and so is not susceptible to the criticisms levelled at Derrida and Saussure. Seeing intelligibility as a continuum and particular languages as objects that exist and function within an open causal system does not entail Actual Speech, but it is certainly consistent with it. Indeed as I will go on to argue in 4.1, it is not just consistent, but readily compatible with Actual Speech. On my view, speakers are not relegated as 'messy' things that are justifiably excluded from the theoretical purview of linguistics. Speakers and what they speak is, to use a metaphor, the engine of language. To possess a language is to possess a power and this power is ineluctably social in nature. To deny this, as we have seen, is beset with difficulties: explaining how language changes, maintaining a consistent and justifiable theoretical purview and understanding how we have concepts are a few such problems. There is more to say about these subjects and I deal more with Actual Speech in 3.1, language change in 3.2 and language as a social structure and Actual Speech in 4.2.

-Language variation and typology

My view chimes with an intuitive understanding of language typology in giving a criterion for distinguishing between languages. As I mentioned before, the idea that possession of a language allows us to communicate with some speakers and not others seems fundamental to our intuitions about what it is to speak a particular language. As is shown in Jensen (1989) and Lind (1972), it is accepted that intelligibility is a continuum and my position is both able to recognise this, while allowing for a principled view of language typology. While my position offers a criterion that is both intuitive and useful in distinguishing languages, it is not the job of philosophy, but of linguistics, to provide workable and practical criteria for defining language boundaries. That courgettes become marrows at no particular point does not mean that there cannot be a rational and consistent framework upon which to judge a courgette from a marrow. Nor does it mean that what counts as a courgette or a marrow has nothing to do with the objective properties of the courgette or the marrow. At best, my work provides some small amount of underlabouring for language typology, but I do not intend to engage in this further.

My position has a number of advantages and does not encounter the kind of problems I have outlined in chapters 1 and in 2.1. My answer though requires expansion and adumbration. For example, what might my stance on language and social causation mean for the explanation of language change? If language is a power, what does knowledge of it consist in? What does my position suggest about the scientific status of linguistics? It is to these questions that the next chapters will be devoted.

Chapter 3
Language change, social action and linguistic knowledge

In the last chapter I argued language should be viewed as a power of speakers to communicate and that knowledge of language consists, at least in part, in the defeasible and causally maskable ability to communicate with other speakers sharing the same language. Seeing language as a power helps us to understand what we mean when we say 'Bob speaks English' as well as the idea that one can be in possession of a language, though not in fact be able to use it (one may be dumb). But to argue that language is a power leaves open specific questions about the nature of language, which I now plan to address. In this chapter I provide a more comprehensive view of language in how it functions in society and what it is for a speaker to understand a language. In order to do this I pick up on some debates in the philosophy of language and linguistics, again with a view to considering their implications for linguistics *qua* science and for what they can tell us about the nature of language.

I start by considering something often taken as a given in discussions about language; that the signs making it up are arbitrary. I argue there is no philosophically or scientifically interesting sense in which language is arbitrary and that discussions about the arbitrary nature of language conflate two distinct notions of arbitrariness. This conflation is both unjustified philosophically and scientifically damaging, obfuscating the idea that language is used by people *for* purposes and is inextricably embedded into social activity. Again I point to the desire for the delimitation of theory as one plausible motivation for seeing signs as arbitrary.

Next, following my discussion of language change, the suspicion of 'messy' speakers and the non-deductive methods and causal claims of the social sciences, I consider a contemporary account of language change and argue that knowledge of language, language use and rationality are intimately intertwined. I argue that if language change is to be explained at all, it has to be explained in terms of understanding what speakers are using language *for*. In consideration of this, I reject accounts of language change that see language as changing 'for its own sake' or having a 'life of its own', an idea we have encountered with organicism, but one with a contemporary pedigree aligned with the Formalist Attitude. In addition, I argue that a powers conception of language is fully compatible with this approach to language change and I bring together arguments from

previous chapters to this effect.

In the final section of this chapter I look at the issue of knowledge of language and consider to what extent, if at all, there is 'knowledge of language' as something separable and distinct from knowledge of the world. I argue that there is little difference and that to maintain a rigid distinction between the two obfuscates our understanding of language. I compare an internalist stance on knowledge of language to an externalist one and defend the latter as the more plausible by showing how interrelated knowledge of the world and 'knowledge of language' are.

3.1 The Myth of Language Arbitrariness

It is widely accepted that natural languages are systems of arbitrary signs. This I intend to question. The idea has been held widely in the history of thought on language and is sometimes taken as an obvious and unproblematic truth. The claim that language is arbitrary is this:

Arbitrariness thesis (AT): Any word in a linguistic system *could* and could just as well have been, represented by another, any other. Words are associated to objects, ideas or uses, *de facto*, not *de jure*.

To contextualise AT one should note that historically, AT has stood in opposition to natural nomenclaturism (NN), which is defined as follows:

Natural nomenclaturism (NN): There exist or could exist words that naturally or ideally belong to the object they refer to or the quality or concept they pick out[59].

The debate between these two positions occurs first in Plato's *Cratylus,* where Cratylus asserts in support of NN: 'Representing by likeness the thing represented is absolutely and entirely superior to representation by chance signs' (Plato 1926, p.169). Here, we can read 'chance signs' as representative of AT. AT is an interesting thesis, with some important claims relying upon it. I have provided below a series of chronological quotations to show the historical breadth, consistency of support as well as the claims made for AT in terms of what it explains.

[59] Sometimes known as the *Adamic* thesis in reference to the Biblical account of naming found in *Genesis*, see Harris (1988, p.47).

Plato:

1. [Hermogenes:] For I think no name belongs to any particular thing by nature, but only by the habit and custom of those that employ it and who established the usage (Plato 1926, p.11).

2. For it seems to me that whatever name you give to a thing is the right name; if you give up that name and change it for another, the later name is not less correct than the earlier (Plato 1926, p.9).

Saussure:

3. There is no internal connexion, for example, between the idea 'sister' and the French sequence of sounds *s-ö-r* which acts as its signal. The same idea might as well be represented by any other sequence of sounds. This is demonstrated by the differences between languages, and even by the existence of different languages (Saussure 2006 [1922], p. 100).

4. The arbitrary nature of the linguistic sign was adduced above as a reason for conceding the theoretical possibility of linguistic change. But more detailed consideration reveals that this very same factor tends to protect a language against any attempt to change it. It means that there is no issue for the community of language users to discuss, even if they were sufficiently aware to do so (Saussure 2006 [1922], p.106).

From a modern linguistics textbook:

5. [T]he word *dog* is arbitrary in the sense that there is nothing predictable about the...sounds that are used to express the lexical concept DOG in English (Evans and Green 2007, p.123).

Chomsky:

6. Variation of I-languages may reduce to Saussurean arbitrariness (an association of concepts with abstract representations of sound) and parts of the sound system, relativity accessible, and, hence, "learnable" (Chomsky 2000, p.27).

7. [There are] arbitrary links between concepts and sounds: the genetic program does not determine whether *tree,* the concept, is associated with the sounds "tree" (in English) or "Baum" (in German). The linkage of concept and sound can be acquired on minimal evidence, so variation here is not surprising (Chomsky 2000, p.120)

These quotes argue for or show support for AT. What some also show and suggest is that AT is used to help explain a number of things about language. These are:

a) The existence of different linguistic systems. This is supported or argued for by quotes 2, 3 and 7.

b) The necessity of learning languages. Supported by 6.

c) The creation of new words and languages (linguistic change). Supported by quotes 2, 3 and 4.

d) The stability of linguistic systems. Supported by 4.

The explanations offered by supporters of AT motivate the rejection of NN. This is because given the claim of NN that there is a 'correct' language and a natural connection between signified and signifier, NN looks unable to account for the uncontroversial fact that there are and have been a variety of languages. Also, NN cannot explain how language change would be possible if there is such a correct or 'natural' language. It could be that modern languages are in some way defective, though it would be upon a supporter of NN to argue where, despite the fact that all languages allow for successful communication, such defection lies. Needless to say, the prospects of NN looking like a reasonable thesis are bleak and this is why Harris notes that, 'few thinkers since antiquity have ever championed the Cratyline [i.e. NN] thesis' (Harris 1988, p.47).

My argument moves in three steps. First, I consider what arbitrariness is and argue that language is not arbitrary, but is in fact pervasively motivated. Second, I argue that AT does not have the explanatory power it claims to with respect to a-d. Third I argue that some of the things AT claims to explain can be explained by motivation. It will be my assumption that a successful attack on AT does not entail that NN be made any more plausible. As such, the focus for the rest of this section will be on the arbitrariness thesis, not NN.

3.1.1 Two versions of AT: Lack of motivation and logical arbitrariness

The most influential contribution to and re-capitulation of AT comes from Saussure's

Course. Saussure is particularly relevant as he argues that AT can explain a, c and d above (see (6) in respect of (a) and (7) in respect of (c) and (d)). One commentator has paraphrased Saussure's position on AT in this way:

> Saussure's idea of arbitrariness has two dimensions: (1) Signs are arbitrary insofar as they are social conventions. (2) They are arbitrary insofar as they are composed of elements (a concept and a sound image) that have no necessary or natural connection to each other (Lemert 1979, p.935).

Saussure qualified his stance on arbitrariness, stating '[t]he term implies simply that the signal is unmotivated' (Saussure 2006 [1922], p.101) and claimed that one can distinguish 'in any language between what is intrinsically arbitrary- that is, unmotivated- and what is only relatively arbitrary. Not all signs are absolutely arbitrary…*The sign may be motivated to some extent*' (Saussure 2006 [1922], p.181) and he separated 'absolute' from a 'relative' arbitrariness. Put simply, linguistic motivation is the idea that there are explicable reasons for words being the words they are[60]. In Saussure's distinction, he used the example of 'vingt' (twenty) as an unmotivated or 'absolutely arbitrary' sign and 'dix-neuf' (nineteen) as a 'relatively arbitrary' sign, being composed of two arbitrary elements combined presumably according to some analogical process. It is clear that relatively arbitrary signs are still social conventions, so being a social convention is not sufficient for being arbitrary[61]. A non-numerical example would be 'screwdriver'. Though 'screw', 'drive' and the affix 'er' are absolutely arbitrary, 'screwdriver' (something used to drive screws into a substance) is not. All this implies arbitrariness is connected with the absence of motivation of the sign and so the issue of motivation is therefore relevant to AT. We can give a modified definition of AT which recognises this:

> ATm: Any word in a linguistic system *could* and could just as well have been, represented by another, any other. Therefore arbitrary words are words that lack motivation. To the extent a word or sign is motivated, it is non-arbitrary.

Many linguists share the view that arbitrariness is related to a lack of motivation (ATm)[62].

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[60] This in principle includes such things as syntax and grammar though as Saussure discusses arbitrariness principally in terms of the sign, I shall keep my focus here.

[61] Indeed Harris (1988, pp48-49.) and Waugh (1993) warn against the conflation of arbitrariness and convention.

[62] It is not clear this is the case with philosophers, for a few reasons. Firstly, some philosophers who discuss arbitrariness (Locke) are still arguing against NN and where arbitrariness is discussed, it is often only in passing, so one should be cautious about reading into such statements.

In discussing arbitrariness, Barthes states 'in linguistics the signification is *unmotivated*.' (Barthes 1968, p.50) and elsewhere: 'arbitrariness is the rule, not the exception' (Lord 1974, p.20), 'arbitrariness is often contrasted with motivation' (Radden and Panther 2004, p.4), 'the notion of motivation is usually contrasted with that of arbitrariness' (Konieczna and Kleparski 2006, p.103)[63]. Indeed as Waugh notes with respect to arguments about arbitrariness and motivation, 'any aspect of language that goes against this assumption [of arbitrariness] is considered to be only a minor exception to the general rule' (Waugh 1993, p.71).

ATm though is in tension with the definition of AT because AT was defined in terms of the possibility of one signifier replacing another. Therefore, it needs to be shown that AT and ATm are equivalent or roughly equivalent. It is appropriate to ask what AT would entail if it is seen apart from lack of motivation. It is quite possible to hold that signs may be motivated (in the way that 'dix-neuf' is motivated), but are still arbitrary. If such a position were a viable representative of AT, then there would be an alternative to ATm. Such a position could hold that arbitrariness is the *logical possibility* of one signifier being replaced with another and that the issue of lack of motivation is not pertinent to AT. It can be defined as follows:

> ATl: Any word in a linguistic system *could* and could just as well have been, represented by another, any other. Arbitrariness is the logical possibility of words being different.

Though linguistic motivation may exist, it has nothing to do with the logical possibility of one signifier being replaced by any other. One holding ATl might accept that 'dix-neuf' is motivated without ceding that the sign is arbitrary. ATl is distinguished from ATm as ATm holds that lack of motivation is connected to arbitrariness, whereas ATl denies this. If arbitrariness only concerns the logical possibility of signifier substitution then ATl makes no commitment whatsoever to linguistic motivation. What is critical to note is that for ATl, discussion of motivation in any *particular* language or any kinds of motivation that function on and influence language is categorically different from the abstract and atemporal conception of arbitrariness represented in ATl. Harris recognises something like ATl in his comment that '[t]he key feature of the linguistic sign, for Saussure, is that the relationship between *signifiant* and *signifiè* is arbitrary. But in the non-linguistic domain he recognised the

[63] See also Holdcroft (1991, pp.53-55) and Thibault (1997, pp.249-151).

existence of signs in which a non-arbitrary relation obtains' (Harris 2009, p.65). As we have just seen though, Saussure as well as many other linguists sees arbitrariness related to motivation and his example of 'dix-neuf' is certainly a linguistic example. As Holdcroft points out, 'the principle of the Arbitrariness of the Sign has universal scope but only those signs which are not modelled on productive patterns internal to the language are completely arbitrary, the remainder being relatively arbitrary' (Holdcroft 1991, p56). However, what Harris recognises is that ATl considers language in an atemporal and abstract sense of a form/content association, a way in which the Formalist Attitude has characteristically seen it. We now have two competing versions of AT.

To become clearer about AT we should consider which version best fits the initial quotes and more importantly, which version is more congruent with the claims of what AT explains (a-d). I argue that ATm is the appropriate representative of AT because though ATl is correct, it is not an interesting position and is not relevant for discussion of the explanations (a-d). On the other hand, ATm can help to explain at least some of (a-d). The first thing to say against ATl being representative of AT is that I have shown that there is a relation between AT and ATm for Saussure and many other linguists and as Saussure's thought here has been so influential, it is plausible that many of the quotes *do* express a relation between AT and ATm. It is also evident in the first discussion of arbitrariness in *Cratylus*. There, Hermogenes' claims about arbitrariness (1, 2) in naming are brought into question by Socrates, who does this by pointing to putatively *motivated* words in Greek: 'The name Orestes (mountain man) is undoubtedly correct, Hermogenes, whether it was given to him by chance or by some poet who indicated by the name the fierceness, rudeness, and mountain-wildness of his nature' (Plato 1926, p.45). Though the seriousness of Socrates in establishing some of the etymologies found in *Cratylus* is questionable, Socrates' frequent opposition to Hermogenes along these lines suggest that ATm is relevant to AT in this case.

Another reason to think that ATl is not representative of AT is because it is not clear it can explain (a-d) and most of the quotes purport to explain at least one of (a-d). By definition ATl has nothing to say about how language functions in a society and for a speaker, so it can only claim that certain events are possible and this is different from explaining them. The Big Bang made it possible for me to subscribe to *Private Eye*, but it would be odd to suggest that this explains my subscription in any interesting way. One who supports AT and with it wishes to explain (a-d) must choose between ATm and ATl. AT cannot be *both* ATm and ATl because they are categorically distinct with respect to their

relation to lack of motivation. If one sees AT in terms of ATl then they should give up claims to explain (a-d) by AT. ATl is true but aside from tackling NN, is a fairly trivial thesis.

I think there has been confusion about AT that has made discussion of it difficult and it may be that Saussure and others hold an unclear mixture of ATl and ATm. Where this is the case there is need for qualification as ATl and ATm are distinct. It is important to note that insofar as supporters of AT hold ATl, it is an uninteresting but true thesis, though insofar as they hold ATm this is an interesting but not obviously true thesis that needs to be considered further. Having now done something to clear up the issue, I will now consider AT synonymous with ATm.

3.1.2 Problems for AT: Motivation and Delimitation

In this section I raise a problem for AT in order to further my argument that language is in no interesting sense arbitrary and also to show that discussion of arbitrariness is difficult to keep within the realms of the linguistic. In the last section I discussed the distinction between absolute and relative arbitrariness and outlined how signs could be considered motivated. However, there is reason to doubt the category of the absolutely arbitrary is useful and if there is reason to doubt the non-motivated nature of putatively absolutely arbitrary signs, then AT should be considered in peril.

Saussure's list of 'relatively arbitrary' (motivated) signs is not limited to the numerical: trees, people, places and crafts are all picked out as examples where linguistic motivation is present (Saussure 2006 [1922], p.181). The list is long and varied enough to give one suspicion about the extent to which signs are absolutely arbitrary and the extent to which Saussure's 'first principle' of the arbitrariness of the sign is representative of linguistic systems. In defending the idea of absolute arbitrariness, one might point to signs already defined as absolutely arbitrary (vingt) and claim that the basis of motivated signs is always reliant upon absolutely arbitrary ones. However, there is a clear sense in which 'absolutely arbitrary' numbers can be seen to be motivated and if they can be seen to be motivated, then it is plausible that this is the case with other candidates for absolutely arbitrary words. An uncontroversial example: French *Un*, German *Ein,* Italian/Spanish *Uno* and English *One* are all connected from a common heritage of Latinisation (*unus/una/unam*). Saussure, a polyglot linguist specialising in Sanskrit, was well aware of these etymological facts and doubtless considered them facts, so it is worth questioning

why he does not see 'Un' as motivated.

In light of Saussure's support for the Formalist Attitude, one reason suggests itself. As I have shown, Saussure is committed to the view that languages were, or should be treated as if they functioned, as 'closed systems' and that there is a synchronic/diachronic separation that 'is absolute and admits no compromise' (Saussure 2006, [1922], p.119). One plausible answer therefore is that trans-linguistic motivation is not motivation of the relevant sort as it falls outside the synchronic. The first point against this is that if this is the case, arbitrariness is maintained by mere stipulation, so one cannot claim that signs are arbitrary because their arbitrariness is only a result of a theoretical imposition, rather than anything about language *per se*. Still, even if we are generous and delimit motivation so that it does not include trans-linguistic motivation, there are intra-linguistic cases that cause similar problems. For example, the English word 'Werewolf' derives from Anglo-Saxon for 'man', 'Wer'[64] (lit. Werewolf = Man-Wolf). This example fits Saussure's definition of a motivated unit as it is a compound of two other apparently arbitrary elements. However, 'Wer' is not part of demotic English and so one is faced with a dilemma. One might deny that 'Werewolf' is motivated because 'Wer' is not an English word so its formation is not a synchronic but a diachronic matter. This though is not a satisfactory response for two reasons. First, it refuses motivation in the face of a case that fits Saussure's own account of motivated signs and second, such a refusal simply stipulates arbitrariness. Alternatively, one might accept 'Werewolf' as motivated, but this would allow a word not in the current English lexicon to provide a basis for the motivation of an English word. This would mean that *tout court* status of trans-linguistic motivation would need to be dropped because one can accept motivation from outside the current English lexicon. As Norris notes, the presence of relative arbitrariness requires that 'the most basic precept of structural-synchronic linguistics is one that has to be given up…as soon as the focus of attention switches from language conceived *in abstractio*…to language as a means of communication between rationally motivated subjects' (Norris in Sanders (ed.) 2004, p.227).

Another option remains though. One could respond by accepting 'Werewolf' as a motivated English word in virtue of a definition of 'English' that includes Anglo-Saxon. Here, it will be argued, the disallowing of intra-linguistic motivation will still hold. But this is the thin end of the wedge. One would presumably refuse the motivation of 'Floccinaucinihilipilification' because Latin is not English but it is hard to see how the motivation of 'Werewolf' differs from that of 'Floccinaucinihilipilification' and it is not

[64] *Se wer* is the nominative case, *were* the accusative case.

clear that Latin has had less of an influence on English than Anglo-Saxon. Again this move refuses motivation in the face of a clear case. There seems no good reason why one should stop linguistic motivation at the much disputed[65] boundaries of natural languages. If this is the case and plausibly many examples of the 'absolutely arbitrary' are motivated, AT becomes less plausible.

As has been noted before (Jospeh 2000, Norris in Sanders (ed.) 2004), despite holding AT, some of Saussure's claims appear to accept the importance of motivation: 'the entire linguistic system is founded upon the irrational principle that the sign is arbitrary. Applied without restriction, this principle would lead to utter chaos' (Saussure 2006 [1922], p.182). This claim supports the importance of motivation because if the entire system were founded upon arbitrariness then it would be chaotic, which is not the case if 'chaos' means anything like 'unorganised'. Holdcroft notes the oddity of Saussure's claim, commenting it 'is something of an understatement if it suggests that we can conceive of a language that is not systematic' (Holdcroft 1991, pp.93-94). Motivation then halts arbitrariness and prevents chaos in language systems as arbitrariness is not 'applied without restriction'. This implies that languages require motivation in the construction and maintenance of language and again suggests that language use and social activity are more connected than Saussure often recognises, something I will discuss more in 3.2 and 3.3.

So far I have put AT into doubt by showing how arbitrariness is connected to lack of motivation and have then pointed out how some strong candidates for absolute arbitrariness are motivated and how motivation cannot be limited to the 'closed system' of language. AT then has been wounded, but it is not finished. This is because AT purports to support some interesting theses. To effectively undermine AT, I now show how it does not have the explanatory power it claims to.

3.1.3 The varieties of motivation

As AT is linked to lack of motivation, an explanatory account of (a-d) which cited linguistic motivation within its explanatory framework would be in conflict with the explanations offered by AT. This is how I will proceed in order to discredit AT. However, if motivation is going to provide convincing explanations for (a-d), it will be necessary to provide an overview of some kinds of linguistic motivation, as the definition of motivation has been broad so far. Literature on linguistic motivation does not just discuss

[65] For an outline of the problems of language boundaries see Lord (1974 p.30).

motivation of the sign, but also grammatical, syntactical and 'non-linguistic' motivation. For example, Haiman posits a kind of grammatical motivation called 'iconicity of motivation' which he defines as, 'that in which a grammatical structure…reflects its meaning directly. The clearest example of such iconicity is that of sequence. Other things being equal, the order of statements in a narrative description corresponds to the order of the events they describe' (Haiman 1980, p.516). However, both for the sake of simplicity and because it is the sign the initial quotes concern, I will concentrate on types of motivation affecting the sign. The collection *Studies in Linguistic Motivation* (Radden and Panther (eds.) 2004) identifies kinds of motivation that affect the sign and language broadly. The kinds of motivation I will look at briefly are: ecological, genetic, experiential, perceptual and cognitive motivation.

Ecological Motivation: This refers to a unit's place or 'ecological niche' within the language system and how it can motivate and be motivated by other linguistic units. As Taylor claims 'it is not the case that each structure occupies a self contained pigeon hole. Each unit has pointers to other units' (Taylor in Radden and Panther (eds.) 2004, p.58). Radden and Panther give the example of '*hamburger* as a compound is motivated in being related to other compounds within the system of English' (Radden and Panther 2004, p.24). Ecological motivation is similar to Saussure's 'relative arbitrariness' in his example of 'dix-neuf' (and equally of 'Werewolf') as such a sign is 'decomposable into its constituent grammatical units. It draws attention to its own principles of grammatical construction' (Thibault 1997, p.283).

Genetic/Structural Motivation: Genetic motivation is synonymous with 'Structural Motivation' (Heine in Radden and Panther (eds.) 2004) and concerns historical factors, with an emphasis on grammatical phenomena though is relevant to signs also. It rests on the idea that what people speak in linguistic state X at time t is partly due to what people spoke in state Y at $t\text{-}_1$. Wittgenstein recognises something like structural motivation in the *Tractatus* when he claims, '[a]lthough there is something arbitrary in our notations, *this* much is not arbitrary- that *when* we have determined one thing arbitrarily, something else is necessarily the case' (Wittgenstein 2001 [1922], 3.342). For example, in English the dominance of the subject-verb-object (SVO) sequence is partly explained because English in past centuries has utilised the SVO sequence. This is not the same for Japanese, where the SOV sequence dominates and has dominated historically. What is interesting about genetic motivation is

that in contrast to the opinion that 'arbitrariness is the rule, not the exception' (Lord 1974, p.20), it supposes that arbitrariness is the exception rather than rule.

Experiential Motivation: This concerns how embodiment affects linguistic structure and signs. For example, the words 'lift/elevator', 'pacifier' ('dummy') and 'screwdriver' are all motivated by the way in which we characteristically experience and interact with them. The existence of personal and impersonal pronouns reflect facts about embodiment and 'basic verbs have a fundamental role in language, as they are subject to figurative extensions' (Benczes 2007, p.96) as for the verb 'take': 'take out', 'take in', 'take on' etc.

Perceptual Motivation: Radden and Panther claim that '[m]any of the organising principles that are pertinent in the structuring of perception also motivate language structure' (Radden and Panther 2004, p.28). They use figure/ground and gestalt examples as evidence for this, contrasting the unremarkable sentence, 'The book on the table' with the odd 'The table under the book'. While the latter sentence may be appropriate in a given context, the point is that perceptual salience can motivate the construction of sentences where the perceptually salient object is grammatically salient (takes the subject).

This is far short of a comprehensive list or account of motivation, which would be beyond the scope of this thesis. What I hope to do with reference to these four kinds of motivation is to show how AT does not explain (a-d) and that motivation can provide better explanations for some of (a-d).

3.1.4 Motivation as explanation

Where arbitrariness putatively explains something about language, the explanatory framework will be simple as there are no kinds of arbitrariness that are not categorically distinct. This is not the case with motivation. As Konieczna and Kleparski state 'there is no denying that motivational factors usually do not function in isolation but they tend either to apply jointly or to compete with one another' (Konieczna and Kleparski 2006, p.105). This is consistent with my arguments in chapter 2 emphasising the open system that language functions in but it presents an explanatory challenge as explanations of linguistic phenomena will be unavoidably post-hoc and non-predictable. This is something I will deal with in 3.2.

Before dealing with (a-d), I wish to make a point about genetic motivation that supports the idea that language is pervasively motivated. The idea that language is *genetically motivated* is presupposed by any notion of linguistic change. Consider the following. For any change to occur, it has to operate upon a linguistic object or state[66] O to form a new linguistic object or state, O_1. Without some connection between O and O_1 the term 'change' would be misapplied. As linguistic change does occur then change from O to O_1 has to be delimited as if it were not then we would not be able to recognise any change and would have no basis for thinking that O_1 was related to O. The intelligibility of etymology, no matter how erroneous or fanciful postulations might be, assumes the genetic motivation of language. This strongly suggests that individuals and communities create signs out of the existing stock of signs and grammar from their (and other) languages. This stock motivates and constrains signifier selection and therefore signs can never be absolutely arbitrary. Language is basically motivated and even where we might not clearly see the reasons for motivation, we have reason for not being pessimistic about the presence of motivation. I now address how I think AT does not explain (a-d) and how motivation offers a plausible explanation for at least some of them.

Linguistic stability (d). Motivation can help us understand how languages remain relatively stable if we consider the idea that the words we use reflect how we typically experience an object, action or idea. For example, it is plausible that 'Carrion Crow' has its name not only because English contains the words 'Crow' and 'Carrion' (ecological motivation), but also because certain ornithological specimens of a sub-genus corvid feed upon cadavers (experiential motivation). 'Tennis-Elbow' is another example, being ecologically motivated by the meaning of its constituents in being a compound and also by the location and manner in which tennis is played, with strenuous use of the elbow (experiential motivation). How does this relate to linguistic stability? That communities call things what they do because of the character of the world and the way they experience the world, it suggests that words that are motivated by such experience will tend to be stable insofar as the way in which that community experiences them remains stable. In *Cratylus*, Socrates argues that in naming 'we cannot follow our own will, but the way the instrument which the nature of things prescribes must be employed...if we pursue this course we shall be better in our naming' (Plato 1926, p.21)[67]. This is not simply the point that one cannot say

[66] We need not read 'state' as a synchronic state with its accompanying commitments. Minimally, a 'new state' simply notes *some* change within a system.

[67] When discussing arbitrariness, Pinker is dismissive of this notion: 'But think about the "sane"

'bubububu' to mean 'pass the salt', it suggests that there are 'extra-linguistic' reasons for a sign being that sign and that language use and meaning are interconnected. This not only suggests that stable practices secure a degree of language stability, but also that the relative stability of the world also affects the stability of language.

Saussure's claim that linguistic stability is due to the arbitrary nature of signs because 'there is no issue for the community of language users to discuss…For to discuss an issue, there must be some reasonable basis for discussion' (Saussure 2006, p.106) is wrong in two respects. First, people discuss and debate words continually, such as words considered sexist or racist. One might object that this is a non-linguistic point and Saussure is only concerned with the linguistic, not facts about language use. I disagree here, for Saussure accepts that one can discuss 'the pros and cons of a system of symbols' (Saussure 2006 [1922], p.106) and that 'our symbol of justice, the scales, could hardly be replaced by a chariot' (Saussure 2006 [1922] p.101). Of course a chariot *could* represent justice, so it does not seem that questions about sexist or racist words can be consistently excluded. Still, even if we do exclude them, there is a plausible basis for linguistic discussion, for one could argue that 'incredible' being used to mean 'very good' and 'disinterested' to mean 'uninterested' violate morphological rules of English.

Linguistic change (c). I have outlined how language change presupposes genetic motivation, but more can be said with respect to individual instances when considering experiential motivation. To return to previous examples; if carrion crows' diets changed to consist only of fungi or occurrences of Tennis-Elbow disappeared from tennis, there would be motivation for change in the use of that expression, namely it becoming obsolete. As well as language change in the form of becoming obsolete, changes can of course be additive to reflect change in practices. The frequency of electronic communication has led to a change in how we refer to messages, with e-mail often being called 'mail' and paper mail being referred to as 'snail [i.e. slow] mail'[68].

Language Learning (b). I do not think either motivation or arbitrariness explain the need to learn languages. For AT, the explanation is unconvincing because as is argued by generativists, the question of language learning concerns the extent to which language is

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alternative of depicting a concept so that receivers can apprehend the meaning in the form. The process is so challenging to the ingenuity, so comically unreliable, that we have made it into party games' (Pinker 1995, p.84). Examples given in this section as well as others found in the literature suggest otherwise.

[68] Such changes are attested to in the recent emergence of 'internet linguistics', see Crystal (2006).

innate and it is hard to see how AT fits into this. If we were to say that language was innate, it may be non-arbitrary in the sense of ATl, but might or might not be motivated in the sense of ATm. It is difficult to see how this might explain the need to learn languages as motivated or not, speakers presumably would have to learn language, even on very 'minimal evidence'. On the other hand, to suggest that signs are arbitrary (in the ATl sense) because they are not innate would imply that everything from carpentry styles to gastric surgery was arbitrary, but this would make arbitrariness utterly trivial. I do not claim that motivation explains the need to learn languages and both positions do not appear relevant to the issue.

Different languages (a). The most plausible claim made for AT is that it explains that fact that there are different languages. This claim is based on the assumption that if there were a natural connection between signifier and signified then there would be a single language. As there is more than one language, there must be no such connection and signs are therefore arbitrary. The problem with this explanation is that it assumes that *either* there is a natural connection between signifier and signified (NN) or one adopts AT. Though NN is implausible, rejecting it does not require the adoption of AT. It is not necessary that there be any particular sign or system of signs, but the points I made about genetic motivation, language stability and change support the idea that the languages of the world and their signs could not have just been anything any more than human history could have been anything. Therefore, one can reject AT as an explanation for there being different languages without adopting NN. One might claim what is meant here is ATl because rejection of NN does imply ATl. This is true, but ATl cannot explain why there are different languages in any interesting sense.

In terms of motivation explaining (a), I wish to be modest. I do not think that motivation can explain the fact that there are different languages at all, but it can help us appreciate why there are different languages now and why languages may remain distinct and different. If we consider genetic motivation, we can understand that there *are* different languages because there *were* different languages and the vital role language plays in the existence of communities motivates their being distinct. We can consider ecological motivation in this respect too, as when a linguistic community creates a neologism in response to a change in their state of affairs, they will tend to look to their own language for the necessary resources, as is made clear with portmanteau words such as 'brunch' and 'smog'.

As a last note, one may be temped to resurrect ATl as representative of AT because there is a sense in which ATl accounts for some of the above as the form/content relation could (in the logical sense) have been different and ATm seems wrong as language does appear motivated. Against this I would stress how trivial ATl is: the monetary system, The French Revolution and popular basket designs in China are all in the same way arbitrary. Sociologists do not explain societal change or the fact that there are different societies by virtue of the fact that a society *could* have been and be different and by the same token, we should not try to explain facts about language so.

It is worth asking why AT has had such longevity. I think there are three reasons. Insofar as AT has been contrasted with NN, the implausibility of NN has contributed to the plausibility of AT. As I noted, this contributes to why AT can be thought to explain (a). Second, the undeniable and uninteresting truth of ATl and confusion between it and ATm has protected the subject of arbitrariness from rigorous analysis. Third and most important, arbitrariness has been insisted upon by thinkers influenced by the Formalist Attitude, whose working assumptions are resistant to consideration of social factors and language use. As motivation is usually diachronic and social in nature, it is no surprise that those who see language as an abstract object should ignore motivation or see it as non-pertinent. I have argued that both of these are mistakes. Claims of arbitrariness obscure the fact that language is used by speakers *for* purposes and that language use is intertwined with the life of a community. AT is false because language is pervasively motivated, the extent of which has been underplayed by linguists. AT does not explain (a-d) in a meaningful way and motivation can in various ways plausibly explain (a), (c) and (d). The idea that motivation explains language stability and change brings us back to the issue of causality and explanation of linguistic phenomena. Explanations from motivation are *post-hoc* explanations and it is to this issue, with specific focus on language change, that I turn.

3.2 Language change, causality and post-hoc explanations

'The only true object of study in linguistics is the language, considered in itself and for its own sake.'

(Saussure 2006 [1922] p.317)

In 2.1 I looked at Lass' well known *On Explaining Language Change*. There I both agreed and disagreed with Lass. I agreed with Lass that the D-N model was not adequate in principle

to explain language change and any attempt to employ it was bound to fail. I disagreed with Lass' pessimism about explaining language change, particularly in his disavowal of the possibility of understanding social causality. There were two main reasons for disagreeing with Lass. While Lass rejected (with good reason) the D-N model, he implicitly relied upon the truth of deductivism in criticising non-D-N accounts of language change. There was therefore a tension in his theory, as his pessimism relied on the truth of a causal model he opposes. The other reason for disagreement with Lass was that his idea of Insight made investigation of language change otiose as it dropped all truth-claims. I now turn back to the issue of language change as I want to give consideration of an appropriate account of language change and to some salient factors in language change.

This is useful to do because linguists' conception of their own science is often problematic (1.1-1.4) and language change is an area where these problems are clear because the issue of causality is perhaps at its sharpest. Why is it at its sharpest? Firstly, because predictive theories and 'laws' of language change have failed. That is, theories that support nomic laws expressing a causal connection between the presence of an explananda and a particular language event (explanandum), one that would both explain and predict future events ('Grimm's Law' of consonantal shift or Zipf's 'law of least effort') have failed on their own terms to offer accurate predictions. For example, Labov admits to 'the apparent impossibility of saying which words have a better chance of surviving and which do not' implying that the operation of language changes are 'far too episodic and unpredictable to be compared to the systematic operation of natural selection' (Labov 2001, pp.13-14). The question of causality is also pertinent to language change because literature on language change cites the social ontology of language and social factors as being important to explaining change (Labov 1973, 2001, Croft 2000, Aitchison 2001, Radden and Panther 2004). This is important because the relevance of the social nature of language and linguistic activity has worried linguists, who fear that social considerations would make linguistics too complex and unconstrained to be fruitful. Language change then is an area within linguistics where the issues and problems of social causality are obvious because of the failure of predictive theories and the perceived importance of social causation.

Indeed, change poses a problem for any account of language which views it as an abstract closed system of rules. Itkonen puts the problem like this, 'the constant possibility of change in social behaviour points to the pervasive historicity of social data […] a decision to adopt a fixed, ahistorical system of descriptive concepts, leads immediately into

conflict with social data' (Itkonen 1978, pp.28-29). If language change is caused by social factors, it follows that the syntactic, grammatical and semantic makeup of a language can change as a result of social factors and this threatens both the idea that we can describe and understand language in an abstract way and predict language change. By returning to this issue I will make clearer the deficiency of viewing language in a way indicative of the Formalist Attitude and advance my account of the importance of social factors in the study of language.

My argument moves in three stages. I start from the assumption that language is not arbitrary (3.1) and agree that given the failure of the kind of predictive theories of language change, social factors are important. I argue that a corollary of this is that language never changes *for its own sake*, but always for some social end. By 'for its own sake' I mean the idea that language changes for reasons that have little or nothing to do with communicative function, but rather according to a set of rules and/or laws internal to language(s). This is consistent with the thesis of organicism (1.2.1) and also with neogrammarian ideas of 'fixed laws' of language change (1.2.2). I call these changes internalist changes. As McMahon observes, generativist engagement with language change fits this mould, being 'descriptive rather than explanatory. Early generative theory, with its emphasis on simplification, provided no adequate general account of actuation or transmission' (McMahon 1995, p.45). I finish this part of the argument by showing that a social end is necessarily present and so there can be no non-social linguistic acts, so the idea of language changing due to internalist changes is false.

I then contextualise the first section by showing how a contemporary account of language change holds that language does change 'for its own sake'. I look at Jean Aitchison's influential work, *Language Change: Progress or Decay?* (2001) and consider how her problematic assumptions about language leave her unable to explain change. Lastly, I look at an alternative to explaining language change without reference to internalist changes by *post-hoc* explanations and give a brief account of why we should believe in any *post-hoc* explanations of linguistic change given that they are potentially unfalisifable and unconfirmable. This will bring us back to the issue of causal powers and cp laws (2.2, 2.3). Here I use Labov's (1962) account of language change in Martha's Vineyard and argue that we should believe in *post-hoc* explanations because, a) one can distinguish between more and less coherent and *post-hoc* explanations, b) some *post-hoc* explanations are more consistent with empirical research and as such can be considered as confirmed and confirmable (though not in the D-N sense) and c) *post-hoc* explanations are the best explanation on offer

in the social sciences and we should defer to best explanations. The purpose here is not to convince the sceptic, but rather to provide good grounds and reasons for believing in *post-hoc* explanations of linguistic change that can offer us an alternative to internalist changes.

3.2.1 Speech and social ends

I have argued that language can be seen as a power with a social ontology (2.3) and that language is not arbitrary in any interesting or useful way (3.1). In particular, I argued that language can only be thought of as arbitrary from a logical point of view and that logical arbitrariness is of little importance with respect to the issue that concerns us here, language change. The negative conclusion in that part leaves us with the open question of the explanation of language change and this has two consequences that help define the next step in our investigation. First and uncontroversially, it tells us that linguistic change is not random; there is always some reason for linguistic change. This I take to be implied by the notion of linguistic *motivation*. If language is not arbitrary then motivated change is not just ruled in, it is the only option available. An explanation of any kind or particular instance of motivation is another matter and one can consistently hold that change is motivated, but these changes can be purely internal. This is the internalist position which I discuss presently.

Secondly, the pervasiveness of motivation and its social basis suggests that the reasons why language changes are at least partly social: reasons agents have for speaking (and understanding) differently in order to achieve some social end (Searle 1969, Austin 1975, Johnson 1990, Clark and Brennan in Resnick and Levine (eds.) 1991). This externalist position is what I argue for. If language change is motivated and language is used for communication, it is plausible that the language change will be motivated by the communicative need of speakers, by what speakers want to do socially. I define 'communicative need' as any act of speech aimed at achieving a social end. By 'social end' I mean a desired change in the relationship between the speaker and one or more individuals or social structure. These kinds of changes happen continuously and in countless banal ways: in acts of purchase, marriage, employment, requests to put the kettle on, attempts to sue and so on. Being motivated by communicative need however is not sufficient for the success of the desired social end. I can try to tell someone my name in order to begin a dialogue, but they might not hear me, ignore me or get hit by a bus. As I have defined it, 'communicative need' is a social concept and explanations involving communicative need

will be social explanations. Not only is motivation the only option on offer to explain language change, but social explanation is the prime candidate for explanations of language change.

It is therefore plausible that all change is at least partly socially motivated and that in looking for explanations we consider social factors. More needs to be done to show that linguistic change is socially caused and in order to do this, I want to rule out the competing idea that change can be accounted for by language-internal motivations. A language-internal motivation is a motivation whose explananda make no use of the concept of communicative need or any social phenomena in the explanation for that change. Good candidates would be 'Grimm's Law'[69] (see 1.2.2) or the assumption underpinning the Minimalist Program that 'the apparent richness and diversity of linguistic phenomena is illusory and epiphenomenal, the result of interaction of fixed principles under slightly varying conditions' (Chomsky 1995, p.8). In such cases, language would change 'for its own sake', with little or no interference from external conditions. The influence of the Formalist Attitude is obvious in the divorcing language (a closed system of rules) from manifestations of language in social context. As we will see, such opinions are evident in current approaches to language change (2.1.2).

To argue that internalist changes are incredible will only do part of the job of showing that social factors are important, as one cannot argue for something being X by showing that it is not Y. However, if X and Y are the only apparent options, showing something to be not Y gives us reason to believe it is X. This is my aim in the next section and in 3.2.3, where I will offer positive reasons for believing X –i.e. to give an account of why we should believe social factors cause linguistic change.

3.2.2 Internalist and externalist changes

For any change to happen within a speech community, there has to be a process by which saying S (where S is any arbitrary utterance) changes from being marked (unintelligible, censured, or simply not used) to being unmarked (intelligible, not censured and widely used) or vice versa. There are two scenarios about a language change motivated by internal factors, both of which are problematic enough to show that internalist changes are

[69] Nerlich (1990 pp.56-58) gives an account of nineteenth century views of language change where 'laws' of change are seen as independent of speakers and usage, whether these be 'external' laws of language-as-organism (Schleicher, Müller, Chaveè) or the 'internal' laws of language in the mind (Brèal, Darmesteter).

incredible. The first, in line with generativism, would see change happening within the I-language of a speaker which would alter their competence in some way. Even if this change occurred without reference to social factors, it would not be accepted by the *language community* automatically as it would have to transmit. The change in performance implied by the change in competence could take any number of values, being seen as evidence of the authority or nobility of the speaker (which might make it widely adopted) or being deemed pompous or elitist (which might discourage its being adopted). As Croft points out 'variants in a linguistic variable have social values associated with them. Speakers select variants to use…on the basis of their social values' (Croft 2000, p.32). It is hard to see therefore how any one change in an individual's I-language could avoid going though a social process of transmission which would determine its influence on the language community and thus the language. The explanation of such a change would need to include social factors.

Labov has questioned this though as he claims there is a 'narrow' interface between language and society, which allows for a separation of the internal and external and implies support for internalist changes as I have described them:

> The evidence for the isolation of abstract linguistic structures from social evaluation and differentiation comes from many sources. In the quantitative analyses of variation, it is found that changes made by the addition or subtraction of internal, linguistic factors are reflected in the values of other internal, linguistic factors, while values of the external, social factors remain identical (Labov 2001, p.28).

This is questionable for two reasons. First, it is not possible to rule out linguistic features gaining or losing social values. In the UK in the 1920's, 'squiffy' was a common informal term for being inebriated, but if one uses it in 2012 they would likely be thought to be being ironic or pompous and one does not have to be a trained linguist to think there are many other such cases. Secondly, even if certain constructions, pronunciations or words have no statistically significant correlation to a social value, this does not mean they lack social value in certain contexts. English speakers in general may have no intuitions about the pronunciation of 'fungi' [fənjī]/[fəŋgī] or the formation of the plural of 'fungus' (fungi/funguses) but this may not be the case among mycologists, who may discriminate. Language use functions in context and the ability of speakers to create 'passing theories' (Davidson 2006, see 3.3) suggests that Labov's distinctions which allow for internalist changes are problematic.

The second scenario of an internalist change would be that a change more-or-less instantaneously occurs within a language community and so the speech of the community is altered in some uniform way, which would avoid the problem with the first scenario. It implies there are changes that do not pass through the speech community at all, but are instantaneously realised, though this is unattractive. There is no evidence to support this idea and plenty of evidence that it is not the case (Milroy 1987, Jonassohn and Doerr 1999). Indeed, even if one were to posit strict rules of speech in a given linguistic community we would still be, as Labov once put it, 'left with the problems of accounting for discrete differences in rule systems between successive cadres of speakers who form a continuous linguistic tradition' (Labov in Stockwell and Macaulay (eds.) 1972, p.102)[70].

It may be objected that how I have defined 'language change' begs the question against internalist changes and that I have simply assumed that changes transmit. Once this is assumed, purely internal language change becomes impossible. However, transmission is clearly essential to change. The idea of purely language internal change implies that transmission does not occur in some cases and this implies a denial that a change can fluctuate, diachronically becoming less or more prominent within a linguistic community. In arguing for internalist changes, one accepts that language change and language use have little to do with one another. This would make the methodology of study into language change otiose and it is a working hypothesis that there is a relation between social processes and language change. I give a more positive account of change in the next section, though what is clear is that internalist changes look implausible.

The notion that change is motivated by communicative need is not without tensions. Keller raises this problem: 'To communicate implies (among other things) the wish to be understood. But if wishing to be understood results in stasis and homogeneity, how does the phenomenon of change occur at all?' (Keller 1994, p.95)[71]. There is a tension because it appears that communicative need can halt rather than cause and explain change. Roughly, this is because talking in the same way is one way of making communicative success likely, whereas talking in novel/unusual/marked ways is more likely to result in incomprehension. In light of the fact that communicative need seems to prevent change, one might point to language-internal motivations for change in order to explain it.

This worry is not warranted for two reasons. One reason is that the worry relies on a too-simplistic notion about what it means 'to communicate' as what one says depends very much on what one wants to communicate. *What* I communicate depends on a variety

[70] The same point is made in Croft (2000, p.10).
[71] This is an oft-made point, see Labov (2001, p.5).

of factors (questions I am asked, limitations imposed by good manners, my overt or covert aims) and so the open-ended nature of what I could conceivably say given the context implies that I communicate different things at different times. Wanting to be understood does not guarantee any cast-iron stability or homogeneity in what I say and in what is said; it depends on what I want my interlocutor to understand, what I think they can understand and much else. Another reason the worry is not warranted is that *the way* in which I communicate things also depends on a variety of factors. If I am part of a panel discussion on the merits of capital punishment and I want to convince an audience that capital punishment is ineffective and inhumane, I may say or write things differently than if I were writing a government-backed report upon the pros and cons of capital punishment. In both contexts I may want to communicate similar points, but the way in which I do so will vary. If for the sake of argument we accept a distinction between what is said and the way it is said, we can see that what I want to communicate is only one factor among many that will influence what I will actually say and the way in which I say it[72].

The worry that the need to be understood makes language resistant to change does not threaten the notion of communicative need being a motivating factor in change because different situations require, encourage and allow for different things to be said. Shakespeare and Milton wanted to be understood by their audiences and readers and it was through the use of novel expressions and neologisms that one understands *Hamlet*, *A Comedy of Errors* or *Paradise Lost*. The worry put forward by Keller is only a worry if all we did when we spoke was to communicate in a semantically strict sense. This is not the case as there is more to speaking than simply communicating a clear and distinct proposition: we suggest, hint, give ourselves away, bore and excite. What appears to be a tension is not. I have now outlined why I think that all language change is, at least in part, socially motivated. If this is correct, then any theory of change will need to take the idea of socio-cultural causality seriously, as there is no way to avoid it playing a part in such theories. This is significant because it entails that in principle an internalist account is bound to fail to provide us with explanations of language change, let alone predictions.

While I think internalist theories of language change are faulty, it is not clear to what extent this is representative of modern theories of language change (though Labov shows internalist inclinations). We have seen how this idea is consistent with nineteenth century ideas of language and linguistics (1.1) and also with respect to generativism. However, as generative linguistics has been little concerned with language change I wish to

[72] The distinction is meant for illustrative purposes and is not meant to imply commitment to any rigorous separation of semantics and pragmatics.

look at a non-generativist contemporary account of language change. I show how a theory can ostensibly take socio-cultural causality seriously but still adopt a problematic acceptance of internalist changes and language as a highly abstract object.

3.2.3 Problems with a contemporary theory of change

The notion that language is in a substantive sense autonomous from human beings underpins the idea that language can change for its own sake. If language were not autonomous, then one would plausibly always have to account for social forces in explaining language change. An internalist position suggests an answer to the question 'what is a language?', an answer that sees language as a closed object of investigation governed by strict rules or laws. The mark of the Formalist Attitude is obvious here. The internalist position is evident in modern theories of language change and is problematic. This is because language change 'for its own sake' is false, but also because language is often seen in a narrow and highly abstract way. These issues are linked, as with many approaches to the study of language problems with methodology can often be traced to problems with the construal of the object of knowledge. One such example is in the work of Jean Aitchison in her well-known *Language Change: Progress or Decay?* (2001). Here, an implicit internalist stance on change conflicts with her more qualified view that it is speakers who change language, which suggests language change be accounted for by social factors. One example of this tension:

> Language has a remarkable instinct for self-preservation. It contains inbuilt self-regulating devices which restore broken patterns and prevent disintegration. More accurately of course, it is the speakers of the language who perform these adjustments in response to some innate need to structure the information they have to remember (Aitchison 2001, p.169).

One could claim that in seeing a tension I am taking the analogy and metaphor too seriously and that the quote provides a way of thinking about language change rather than any serious claim about the nature of language and language change. I do not think this is the case and there are other quotations that imply attachment to an internalist position in Aitchison's theory. For example, Aitchison approvingly quotes Sapir's claim that 'Language moves down time in a current of its own making. It has a drift…The Linguistic drift has direction' (Sapir 1921, p.150) and expands on this point, claiming that 'clines are unidirectional for the most part. Just as streams always flow downhill, not uphill, so

language squeezes words together, it does not normally pull them apart' (Aitchison 2001, p.114). When discussing the causes of language change, Aitchison argues that change is 'double-layered': 'On the top layer, there are social triggers. These set off or accelerate deeper causes, hidden tendencies which may be lying dormant within the language' (Aitchison 2001, p.153). The quotations picked out can be seen to support an internalist view of language change and the view of language that underpins the internalist position. 'Language' is cast as an impersonal force and the comparison with natural objects and discussion of 'self-regulating devices' have a distinctly internalist ring.

I want to give further consideration to the idea that language change is 'double-layered', and that social factors provide a 'triggering' mechanism for the 'deeper' layer. Aitchison claims, social factors are not the "real' causes, but simply accelerating agents which utilized and encouraged trends already existing in the language' (Aitchison 2001, p.151). In the section, 'Doing What Comes Naturally', Aitchison expounds some of the 'real' (i.e. internal) causes of change. Examples are phonetic 'weak spots', which because of the structure of the speech apparatus, lead to changes in pronunciation. Aitchison picks out what she argues is the natural tendency of consonant dropping at the end of words, which is because end consonants are 'weakly articulated and difficult to perceive. Within the last millennium, the voiceless stops [p], [t], [k] have been lost at the end of words in French, Chinese and Maori, among other languages' (Aitchison 2001, p.155). Aitchison concludes:

> Overall, then, it is *normal* for consonants to disappear at the end of words over the ages. It has already happened in numerous languages over the centuries, and will undoubtedly happen in many more. It is as much a crime for words gradually to lose their endings as it is for rivers gradually to erode river beds (Aitchison 2001, p.157).

Aitchison refers to things such as consonant dropping as 'predictable developments' and claims there are phonetic tendencies that are 'guaranteed to cause change, others [changes] wait in the wings…until some chance circumstance allows them to sneak in and take hold' (Aitchison 2001, p.161). Such claims emphasise her view of social factors as ancillary 'triggers' and significantly, her support for guaranteed and predictable changes and her claim that consonant dropping is 'normal' throughout languages are clear examples of language 'changing for its own sake'. Her claim that consonants have 'disappeared over the ages' and will 'undoubtedly' continue to do plausibly relies on an internal law or linguistic teleology that has no recourse to the social factors in explaining the occurrence of the change. This strongly suggests some support for internalist changes.

Aitchison's claims about consonant dropping face a number of problems. First, if consonant dropping is an inexorable natural tendency then it is mildly surprising that the languages of the world still possess such phonetic 'weak spots'. Humans have had language for tens of thousands of years which, given the rate at which Aitchison describes consonant dropping happening, it is not implausible that it should already have happened in all languages. Another question is why there should be such phonetic weak spots *at all* in languages if it is an inexorable natural tendency of the language to remove them. That is, why would language 'begin' with qualities that are against its natural tendencies? The position suggests that language is internally evolving to be more consistent with its natural tendencies. This is, to say the least, not obvious. Aitchison does not offer answers to these issues, but one plausible explanation for there still being phonetic weak spots is that they can be, and are, associated with social values which make them resistant or vulnerable to being dropped[73]. Indeed it is not hard to find English speakers who see pronouncing end consonants as a mark of 'speaking well'. This explanation aside though, the issues raised here are enough to show that support for 'guaranteed' changes and changes that will 'undoubtedly' continue is hasty and to be viewed with suspicion. Therefore claims about 'predictable developments' in language change, even if one is using 'predictable' in a loose sense, appear over-confident.

A broader issue with Aitchison's theory is that she appears to privilege the 'internal layer' in her account which she deems is where the 'real' cause of changes are. Such privileging is evident even in theorists known for their (relative) emphasis on the importance of social factors. For example, at the beginning of the seminal *Sociolinguistic Patterns,* Labov claims 'the contribution of internal, structural forces to the effective spread of linguistic changes…must naturally be of primary concern to any linguist' (Labov 1972, p.2). One reason for thinking Aitchison privileges the internal layer is because she supports internalist changes that have nothing to do with social triggers and are a result of 'predestination rather than sin [i.e. speaking badly/lazily]' (Aitchison 2001, p.154). As such, the internal layer is causal in all changes whereas the social is not and so the former is more important. Another reason for thinking this is due to a consistent way of discussing language that casts the social as 'superficial', seeing 'sociolinguistic factors – fashion, foreign influence, or social need…not to be the 'real' causes, but simply accelerating agents which utilised and encouraged trends already existing in the language' (Aitchison 2001, p.151). Aitchison though does recognise such phraseology can appear problematic and

[73] It is interesting that Aitchison's stance on consonant dropping is in respects similar to ease of articulation theories which she is elsewhere disparaging about (pp.154-155).

offers the more neutral 'long-term' (internal) and 'immediate' (social), so it would be unfair to press this particular point too far. However despite this qualification her clear commitment to internalist changes combined with suggestions that language is a natural object and that social factors are 'superficial' suggest otherwise. The point here is that while internal considerations are plausibly part of the causes of change, this does not provide support for the internal layer being more 'real' or important and as the discussion of Aitchison's support for internalist changes has shown, such a position is problematic.

Aitchison's theory suffers from defects because, against initial appearances, it is allied to the internalist position. Aitchison sees social causes of change as ancillary to the natural tendencies of language, which is unjustified. Also, she supports guaranteed changes which suggest belief in laws and predictions which, as we saw in chapter 1 when considering organicism and the neogrammarians and in more detail in 2.1, have not materialised and are unavailable to the linguist.

3.2.4 Post-hoc explanations and satisfying belief

I have shown how internalist changes are problematic and how support for them is present in an influential account of language change. It now remains to discuss the issue of explanation and why, if the reasons for change are social in nature, we should believe in such explanations. Notably, Aitchison explains how we might have knowledge of the causes of language change in rather weak terms. When discussing the syntactic influence that dissimilar languages can have on one another, she claims '[i]t seems unlikely that these uncommon features arose coincidentally in the languages concerned, and most linguists assume that they spread from their neighbours due to cultural contact' (Aitchison 2001, p.140). When presented in this way, taking social causality seriously within the framework of an explanation for change looks unattractive because Aitchison relies on coincidence[74] as something upon which knowledge of causal connections can be based and also suggests that its status as a given is sufficient for such knowledge. Despite these issues, her claim is implicitly for a post-hoc explanation and for a variety of reasons, post-hoc explanations are seen as problematic. One reason is because they propose a non-asymmetric relationship between prediction and explanation and as with cp laws (2.2), post-hoc explanations do not offer predictions and are not directly testable or falsifiable (Mantzavinos (ed.) 2009).

[74] While Aitchison devotes a section of her book to the subject of causation, she does not engage with philosophical questions about the nature of causation and our knowledge of it. Therefore the quote provided is a fair representation about what she has to say on the subject.

April McMahon offers a different approach to language change from Aitchison. She recognises the problems of strict prediction and explanation and claims that 'consequently explanations in both fields [actuation and transmission] can be probabilistic or statistical at best' (McMahon 1995, p.45). Here the problem is of a different nature from Aitchison. As I argued in 2.2, statistical explanations do not explain anything, they only state the probability of X happening given condition(s) Y. As probabilistic or statistical 'explanations' are only restatements of the statistical probability it is not clear how they are explanations. While McMahon assumes less than Aitchison, her approach appears unfruitful and in fact, I want to broadly agree with Aitchison in her adoption of post-hoc explanations. Such agreement though needs to be justified by an account of why we should believe in post-hoc explanations and I do this in the next section by looking at William Labov's seminal study of language change in Martha's Vineyard (Labov 1972 [1962]) by which I aim to make post-hoc explanations plausible.

Before giving an account of post-hoc explanations, I would like to spend time outlining what I think my account is not. My account is not a Dewey/Rorty response which would replace explanation with 'warranted assertability' (Rorty 2008); i.e. the idea that post-hoc explanations do not imply belief in causal connections between objects or events but rather that the explanation has met certain society-specific criteria that makes it 'warranted'. Neither am I giving a Humean answer to the question by seeing belief in causal connections and explanations as a matter of 'custom' or 'habit' which are 'the great guide[s] of human life' (Hume 1975 [1777], p.44) but ones which we hold against rationality. My account is meant to make belief in causal explanations from post-hoc explanans plausible. My position has an advantage over Dewey/Rorty or Hume because 'assertability' or 'habit' differ in an important way from belief in causal relations as they cannot motivate or justify research programmes. Assertability and habit do not permit us to believe anything in particular about objects which assertability statements are about. In the case of assertability, all a scientist can do is to tick off her findings against a series of prescriptive criteria and state that her thesis is or is not assertable given those criteria. In the case of habit, the scientist is in an even worse situation, as her investigations are the result of a non-rational predilection. In either case, the scientist does not believe any particular thing about the objects, only particular things about the statements about objects (if they believe those objects exist at all). If this is the case, there would be little motivation for conducting research into how social factors affect language change (cf. 2.1). What is clear though is that when scientists do science they are attempting to answer questions

about the nature of the object of study and if they were not then research would be a rarefied form of Keynesian economics, charlantry, or both.

I have outlined what my position is not, now let me outline what it is. Post-hoc explanations can possess a number of qualities that are important for explanations in science generally and if we accept these qualities in other scientific theories as important to satisfying belief, we should accept them in post-hoc explanations. These qualities are coherence and consistency and when coupled with the principle of inference to the best explanation, they can make post-hoc explanations strong candidates for belief. While none of these qualities are sufficient for belief, they allow for post-hoc explanations to be compared, replaced, redescribed and even to a modest degree tested. Post-hoc explanations possess an evaluate framework and I now give some detail as to what this framework consists in.

- *Coherence*

Itkonen (1983) sees coherence as synonymous with 'intelligibility'. Less simply, it is seen by BonJour as 'a matter of how well a body of belief 'hangs together" (BonJour 1985, p.93) and consists of a number of qualities of explanations and theories that come in degrees[75]. Highly coherent explanations are those whose explananda hold logical connections with one another and to the explanandum and I follow Bartelborth in seeing coherence as being linked to 'explanations [which] can *embed* the explained in such a way in our background knowledge that it becomes more acceptable' (Bartelborth 1999, p.213). For example, if something is claimed I deem extremely unlikely (say that my adult life has been spent inside an elaborate computer game[76]), any credulity on my part will rely on it being explained how such events could have occurred and how this explanation embeds with my background knowledge. Coherence is especially important to post-hoc explanations because without the surety that nomic statements offer, coherence (generally via normic statements) provides a way in which explanations can be judged. Post-hoc explanations can vary in their coherence and so can be compared and seen as better or worse in terms of coherence.

- *Consistency*

[75] I have offered only a brief outline though see Bartelborth (1999) for more.
[76] This is the premise of the episode 'Better than Life' from the sitcom *Red Dwarf* and is also the subject of a novel by the same name (Naylor 1991).

Consistency comes in two flavours; one is 'mere consistency' and the other 'consistency'. Mere consistency is weak as it identifies a set of events, sense-data etc. that are logically possible and do not imply the negation of any other member of the set[77]. While all coherent explanations will be merely consistent, much that is intuitively nonsense will also be and so mere consistency is not useful as one can (merely) consistently claim that pigeons spread disease because they are evil. 'Consistency' is a more substantial notion implying congruence with other empirical research, which is similar to 'diachronic coherence' in that it suggests that if a thesis is well confirmed and well understood (the link between crime and poverty, say) one should be careful not to instantly abandon the thesis in the face of conflicting evidence. Consistency allows for a degree of confirmation and falsification of post-hoc explanations and theories which, while not analogous to confirmation and falsification in the natural sciences (2.2), is a way in which post-hoc explanations can be ranked. Explanations that are highly consistent can eliminate competing explanations in two ways: due to empirical research falsifying theses and by the relation of the thesis to other empirical work, which are two sides of the same coin.

- *Inference to the best explanation*

This staple of philosophy of science endorsed by all stripes of realist[78] relies upon an explanation being (perhaps among other things) coherent and consistent and being ranked among other possible explanations as the most coherent and consistent. If post-hoc explanations can been more or less consistent and coherent then there are grounds upon which to claim that some explanations are better than others and so inference to the best explanation can be invoked as reason to consider post-hoc explanations as explanations worthy of belief. I assume that support for inference to the best explanation is alien to warranted assertability/habit positions on explanation as inference to the best explanation implies belief while being expressly fallibilist.

Coherence and consistency are the kind of things that make scientific explanations plausible and are fundamental to the believability of accounts and explanations of every sort: statements to police, a child's story about a broken vase, a philosophical treatise, self-

[77] Coherence and consistency are sometimes taken to be synonymous. For example Lehrer (1990) labels mere consistency as coherence.
[78] See Harman (1965) for a dissenting voice.

diagnosis of a mental illness or *ad hoc* hypotheses used to shore up an anomalous theory (dark matter). As we will see, post-hoc explanations can be fruitfully understood within this framework.

3.2.5 Applicability: Martha's Vineyard

Now to the positive account of post-hoc explanations and the seminal study of Labov (1972 [1962]) of phonetic change in Martha's Vineyard; a study known to anyone familiar with modern linguistics. While much has changed since the 1960's in the development of the 'dynamic paradigm', the Martha's Vineyard study is still pertinent to questions of linguistic methodology, causality and explanation, especially with respect to the importance of social factors. As Blake and Josey note in their reconsideration of the Labov study, 'the inquiry into real-time diachronic change in language communities is…often explained by transformations in the set of social dynamics' (Blake and Josey 2003, p.451).

Martha's Vineyard sits a few miles from the coast of New England and at the time of Labov's study had a permanent population of around 6000 with the addition of approximately 42,000 summer visitors, who had been given a pejorative appellation 'summer people' by the locals. The population distribution of the island was not uniform; the 'summer people' had bought up property on the north-east shore (known as up-island), whereas locals mainly lived on the western side of the island (down-island), which was predominantly rural. Among the industries on the island, a small number of the population (2.5 percent) were employed in the fishing industry in an area called Chilmark. Fishing on the island had a long history and Chilmark fishermen were seen in positive terms by other locals, who regarded them (in sharp contrast to the 'summer people') as brave, independent and skilful.

In his study, Labov focussed upon the diphthongs /aw/ (*out, house, trout*) and /ay/ (*while, pie, might*) and noticed that some locals had a stronger tendency than 'summer people' to pronounce the diphthongs from a more central start point, so [aw] -> [əu] and [ay]->[əi]. This was particularly prevalent in local males between the ages of 31-45 and within this group, especially prevalent in the fishermen of Chilmark and other rural areas, those 'stubbornly opposed to the incursions of the "summer people"' (Blake and Josey 2003, pp.453-454). Labov was assisted by linguistic information gathered from the island from thirty years beforehand, where there was shown to be a greater presence of these centralised diphthongs among the locals than was evident in his study. In the intervening

period, the only groups that had withstood change were the fishermen of Chilmark and other groups of local males between the ages of 31-45. The explanation given by Labov was that the fishermen of Chilmark represented a standard of 'authenticity' for other males in the area and one aspect of this authenticity was realised in the centralisation of diphthongs in the phonological system of the fishermen. From this, Labov proposed two things. Firstly, that the direction of the change was from the fishermen to the male demographic (the local men were copying the fishermen) and second that this was motivated by a desire on the part of the male demographic in question to be 'authentic', which made the men resistant to the changes that were happening in other parts of the island (i.e. up-island). As Labov summates it, 'one cannot understand the development of language change apart from the social life of the community in which it occurs' (Labov 1972 [1962], p.3).

Less informally, Labov's explanation looks like this, where 'CF' is Chilmark Fishermen and 'MD' is the male demographic that Labov picks out as imitative of CF (though some of CF are MD). 'OI' are the other islanders who have centralised the diphthong.

E1

C1 a sequence [aw] spoken by OI

C2 a sequence [əu] spoken by CF

C3 a sequence [əu] spoken by MD

C4 CF are seen as 'authentic' by MD

C4a the sequence [əu] spoken by CF is seen by MD as part of what makes an islander 'authentic'.

NS1 speakers tend to want to be 'authentic'.

E a sequence [əu] spoken by CF and MD

A few caveats are required. For simplicity, this explanation only considers the maintenance of the [əu] diphthong in MD, not other changes that were observed by Labov (the [ay]>[əi] shift). Also as with all explanatory accounts, there is the possibility for expansion or contraction of antecedents. For example C4a is strictly not needed, though it makes clearer the relation between perceived authenticity and a realisation of the [aw]/[əu] part of the phonetic system. On the other hand, there are grounding or background assumptions which might require their own explanation. For example, how it is that MD share a

conception of authenticity and what does 'authenticity' consist in? As I argued with respect to cp laws, an explanation of an event will contain an unspecified number of assumptions which is needed to secure the coordination of content of the explanation. As Clark and Brennan put it, coordination of content between parties cannot happen without 'a vast amount of shared information or common ground- that is, mutual knowledge, mutual beliefs and mutual assumptions' (Clark and Brennan in Resnick and Levine (eds.) 1991, p.127). So while caution is advised, tacit assumptions and implicit background conditions cannot be avoided if explanations are to be meaningful and not forbiddingly long.

To contrast my gloss of Labov's explanation and show how post-hoc explanations can be usefully compared, I give another potential explanation of the same event.

E2

C1 a sequence [aw] spoken by OI

C2 a sequence [əu] spoken by CF

C3 a sequence [əu] spoken by MD

C4 CF are seen as foreigners by MD

C4a the sequence [əu] spoken by CF is seen by MD as part of what makes CF 'foreign'.

NS1 speakers tend to want to be 'foreign'.

E a sequence [əu] spoken by CF and MD

I want to compare E1 and E2 against the criteria set out above to see Labov's explanation can be considered superior to E2.

Coherence. The coherence of an explanation is the extent to which the explananda are relevant to the explanandum and the explananda are mutually supportive. It initially appears that both the 'foreign' and the 'authentic' explanations have similar coherence as one can see from the explanations that the normic statements and the conditions surrounding them seem equally relevant. However, the extent to which the respective normic statements are true of the target of the explanation are not the same. In attempting to find an explanation, Labov did not find a correlation between 'foreignness' and the pronunciation of the [əu] diphthong, but to a set of positive qualities associated with the Chilmark fishermen (local, hardworking, strong) captured as 'authentic' and the resistance to the sound change among MD. The 'foreign' explanation then is less coherent because it does not tie up with an independently verified and relevant normic statement about the society

in question; it does not embed as successfully with background knowledge.

This account is rudimentary and could be enriched by an accompanying exegesis of 'authenticity', but the point is that we have grounds to consider the 'foreign' explanation as less good than the 'authenticity' explanation as E1 'hangs together' better than E2. One might object that Labov did not bother to see if 'foreign' was a relevant category in the target community and so even if there is a (statically significant, intelligible) relationship between perception of 'authenticity' of CF and pronunciation of the [əu] diphthong, it is radically underdetermined. Though there are clear limitations to underdetermination. This is because there is reason ex hypothesi to think that certain categories and concepts are not appropriate to the explanation. Categories such as 'Is a giant lizard', 'Speaks linear-B' and 'Owns two bicycles' are such categories because one has *no good reason* to think they apply in the first place as there is not an intelligible story one can tell about them. Another reason is that if one finds a category to be relevant, then other categories are less likely to be relevant. In the example above, the relevance of being an 'authentic' inhabitant of Martha's Vineyard would diminish the likelihood of 'foreign' having the same impact.

Consistency. Given the status of Labov's study as a seminal piece of sociolinguistic research, whose methods and broad findings about the relevance of social factors to change have been seen to be fruitful and confirmed (Milroy 1987, Blake and Josey 2003), it is fairly clear how my gloss of Labov's explanation is congruent with other coherent and consistent research and without engaging in a lengthy peer-review, I can only claim that Labov's study is consistent with other research[79]. As well its acknowledged influence, Labov's study also relied on data collected in Martha's Vineyard thirty years before, something which integrates it within the canon of research and gives us reason to see it as highly consistent.

The question of falsification might be raised here, asking under what circumstances one would be justified in giving up the explanation. It may be argued that unless one can state this in advance, one has licence to engage in intellectually disingenuous shifting of criteria to fit one's own theoretical inclinations. The answer to this is that one cannot give in advance the exact conditions under which a particular explanation would need to be abandoned and this is not for reasons of intellectual disingenuousness. As society is causally open, one would expect counter-instances to a hypothesis and if this were never the case there would be no need for post-hoc explanations, cp laws or disposition ascriptions. Also, as research is generated from a theory or particular explanation,

[79] See entries under 'Labov' in (Trask (ed.) 1997, Chapman and Routledge (eds.) 2005).

explanations of events often become refined, rather than refuted outright. I may explain the occurrence of cat poo on my kitchen floor as a result of my cat. Upon further investigation, my neighbour's action of taping up the cat flap while Fluffkin is in the house may require me to refine my explanation. Though the initial explanation was not false, it has less explanatory power and as explanations are subject to refinement, so must the conditions where those explanations might be brought into question. Refinement also helps to put into question competing explanations in contention before I had additional knowledge; for example the idea that my neighbour put the cat poo there or that Fluffkin is a very bad kitty indeed. For these reasons a requirement of setting out falsification criteria in advance is too strong.

Only on a case-by-case basis can a particular explanation or theory come into question. The sociological cliché of the link between crime and poverty has counter-instances, namely the recent recession, where crime levels across Europe have fallen despite an increase in real and relative poverty[80]. This fall could potentially be accounted for by the state's awareness of the crime-poverty link and responses to it by having 'tougher' policing and putting more resources into policing and security measures. Such state involvement may produce a counter-instance to the crime-poverty link, but the action of governments and individuals presuppose the truth of it.

Inference to the best explanation. Both the scalar nature of coherence and consistency allows explanations to be ranked as more or less good. It is therefore appropriate to invoke inference to the best explanation because we have a substantive sense in which explanations can be more or less good. Labov's explanation of sound change in Martha's Vineyard is both coherent and consistent and there are other plausible explanations that are less coherent or consistent with Labov's explanation.

One might object that even if post-hoc explanations have such qualities, we still have a worrying lack of surety about their truth because the best explanation out of a set of what may be very poor explanations is still a very poor explanation. My answer is that either the concept of a 'very poor' explanation presupposes some criteria for what constitutes a good explanation, in which case we can still accept the best, but with caution, or it begs the question. In the future, today's research and explanations may appear less coherent and less consistent, but this is an occupational hazard of explanation in science in general, rather than social explanation in particular. It is worth noting that a coherent

[80] See, Aldridge et al (2011) *Monitoring poverty and social exclusion*.

explanation that is highly consistent with other coherent research is given further support by dint of the fact that the research it is consistent with may also be highly consistent with *other* research. The explanations of today that underlabour for the explanations and theories of the future and post-hoc explanations of language change are not alone in this respect.

As support for internalist changes supports a view of language that sees it as autonomous from social factors, my arguments concerning the motivated nature of language and my support for externalist changes suggest that language is not autonomous from social factors as motivation and change are intimately connected to the things speakers do with language. As I have emphasised, Actual Speech needs to be recognised as the engine of change as linguistic action is social action and it is through linguistic action that language reproduces and potentially changes itself.

Language continues to appear a less autonomous object than many linguists suppose, but so far we have not directly considered an important issue in the philosophy of language and linguistics about what it is to know a language. At the end of chapter 2, I argued that possession of language by agents can be fruitfully seen as a power. By foregrounding communication, this idea was consistent with the importance of Actual Speech and implied that language was a social object existing in a causally open system, as opposed to a highly constrained and abstract object that is amenable to 'scientific' study, motivated by *de facto* stipulations of theoretical boundaries or *de jure* assumptions or arguments about the closure of the linguistic system. However, I have not dealt with the question of what it is that a speaker must know in order to speak a language, only that in knowing a particular language, one has a power to communicate in that language. The question of knowledge of language is relevant for two reasons then. It will clarify what I think language is and provide another opportunity for reflection on what it is to study language; bearing again upon the question of what linguistics is.

3.3 Knowledge of language

The question of what it is to know a language is an important matter for the philosophy of language and linguistics. The question overlaps with the question 'what is a language?' and an answer to one will imply something about the other. Whatever knowledge of language is, it describes the extent to which language structure is autonomous from and interacts

with external factors. Here, 'external factors' can be anything from context, tacitly known pragmatic rules or other cognitive systems, capacities or knowledge. There is agreement that knowledge of language is something without which communication would not be possible, so the question is both interesting and important.

This is where agreement can end though; an agreement sometimes based on no more than an acknowledgement that in English, 'to know' is a verb collocated with 'a language'[81]. Positions about the nature of knowledge of language fall roughly into two camps. One camp is occupied by those who hold to the possibility of a formal and precise description of what a speaker knows when they know a language ('competence', following Chomsky) and the other by those who deny this and see knowledge of language as a non-discrete ability whose rules are pervasively affected by language use, so that a systematic account is difficult or impossible[82]. In his earlier work, Davidson held the latter and explained what an account of knowledge of language should consist in, in this way:

> One natural condition to impose is that we must be able to define a predicate of expressions, based solely on their formal properties, that picks out the class of meaningful expressions (sentences), on the assumption that various psychological variables are held constant. This predicate gives the grammar of the language. Another, and more interesting, condition is that we must be able to specify, in a way that depends effectively and solely on formal considerations, what every sentence means. With the right psychological trappings, our theory should equip us to say, for any arbitrary sentence, what a speaker of the language means by that sentence (or takes it to mean) (Davidson 2001 [1965], p.8).

This is a paradigmatic example of what I again call an 'internalist' position; internalist because, ideally, what a speaker knows when they know a language is a finite set of grammatical rules plus a semantic framework which is sufficient for interpretation of any sentence in the language. Within linguistics, this position is reflected by generativism, evident in the following quotes:

> A standard, and I think basically correct, approach to [knowledge of language] is to assume that a person who knows a language has internalized a grammar, a system of rules and principles that assigns structural descriptions to linguistic expressions (Chomsky 1981, p.9).

> A fully adequate grammar must assign to each of an infinite range of sentences a structural description indicating how this sentence is understood by the ideal speaker-hearer. […] one of the qualities that all languages have in common is their 'creative' aspect. Thus an

[81] See Harris (1981 pp.36-37).

[82] This is associated with positions in the philosophy of language concerning the role of context in felicitous interpretation. It goes by a number of names including, 'semantic scepticism' (Stanley 2007), 'radical contextualism' (Cappelen and Lepore 2005) and 'occasion sensitivity' (Travis 2008).

essential property of language is that it provides the means for expressing indefinitely many thoughts and for reacting appropriately to an indefinite range of new situations (Chomsky 1965, pp.5-6)[83].

Holding the opposite view, we may consider Baker and Hacker:

Knowing a language is a matter of possession of a wide array of skills associated with speaking and understanding, of being able to use and respond cogently to the use of language. It is wholly unclear what is meant by 'an account' of what it is that someone knows when he knows English…If someone can speak a language then he can do a myriad of things with it; he can make assertions, issue statements, express beliefs…an indefinitely long array of acts and activities accessible to language-users (Baker and Hacker 1984, p.278)

This is an example of an externalist position on knowledge of language; externalist because what is emphasised is that knowing a language is an ability (or a power) to do certain kinds of things. Linguistic activity is linked with activity-in-general in a way that implies any distinction between 'knowledge of language' and 'knowledge' is fuzzy or even non-existent. Minimally for the externalist (for this is a spectrum rather than a single position), knowledge of language is non-autonomous and non-discrete. This does not rule out the existence of tacit or propositional knowledge of the rules of a language, but it does mean that the rules of one's language ('rules' here as understood by the internalist) are not sufficient for the understanding of arbitrary sentences in that language or for explaining linguistic creativity. Within linguistics, externalism is reflected in cognitive linguistics. According to Evans and Green:

In rejecting the [generativist] distinction between competence and performance, cognitive linguists argue that knowledge of language is derived from patterns of language use, and further, that knowledge of language is knowledge of how language is used…the organisation of our language system is intimately related to, and derives directly from, how language is actually used. It follows from this that language structure cannot be studied without taking into account the nature of language use.' (Evans and Green 2007, p.108).

In opposition to internalist proposals for the autonomy of linguistic knowledge, an acceptance of the importance of usage for an account of linguistic knowledge recognises that the 'context in which an utterance or usage event is situated is central to the cognitive explanation. This is particularly true of word meaning (Evans and Green 2007, p.112). As

[83] Lepore notes the link between Chomsky and Davidson with respect to their view of knowledge of language, though stresses that unlike Chomsky, Davidson's concerns about describing knowledge of language are 'not influenced by psychological or neuropsychological concerns. The 'constraints' on semantic theories I have discussed are not about how an ability is put to use, acquired, stored or represented' (Lepore 1982, pp.190-191).

such, knowledge of language is not autonomous in the way the internalist suggests.

The internalist/externalist distinction is also evident in a difference in what are considered the theoretical units of linguistic study. Internalists make use of the *sentence*, the largest meaningful unit in a theory which is seen as context-free and whose meaning is given by the concatenation of rules. In Wittgensteinian spirit, externalist linguists ascribe to the 'usage based thesis' and emphasise the *utterance*, which Croft defines as 'a particular, actual occurrence of the product of human behaviour in communicative interaction…as it is pronounced, grammatically structured, and semantically and pragmatically interpreted in a context' (Croft 2000, p.26). The 'utterance' has been described as 'unit like' and 'somewhat discrete' (Evans and Green 2007, p.110) in recognition that knowledge of language concerns more than the internalist admits and so cannot be defined with the same level of specificity as the 'sentence'.

I treat the two positions generally as it is beyond the scope of this enquiry to examine the myriad ways in which internalism/externalism shapes specific issues in the philosophy of linguistics and language; questions such as the relation between semantics and pragmatics or the viability of a theory of interpretation. Rather, what I propose to do in this section is to show why, so far as I have outlined them, the externalist position is correct and the internalist position is false. My argument has two stages. First I argue that what is taken to be 'knowledge of language' is not amenable to the formalised study that internalists desire because knowledge of language and knowledge are pervasively linked. Next, I defuse a worry taken from early Davidson that if internalism is false, then language learning is not possible. Lastly, I show how externalism about knowledge of language fits with my view of language as a power and my position on language change, arbitrariness and Actual Speech. This will provide unity to the themes I have covered in this chapter and in chapter 2 and will stress the importance of understanding the social nature of language and the importance of social activity to language, paving the way for the concluding chapter.

3.3.1 Knowledge of language and knowledge of the world

Internalism and externalism about knowledge of language imply a different relationship with respect to what one may call 'knowledge of the world'. As Harris describes what he calls a 'segregational' approach to language, 'for this theorist, semiological knowledge and knowledge of the world are two segregated domains. There is contact between the two but

nevertheless they…are to be studied independently' (Harris 1996, x). This characterisation can be fairly applied to internalism and if it were shown that 'knowledge of the world' and 'semiological knowledge' (which we can read as linguistic knowledge)[84] are not just in contact, but are pervasively interlinked, then the internalist view of knowledge of language would be put into question. One way of doing this would be to consider sentences[85] whose meaning putatively relies upon knowledge that is, for the internalist, non-linguistic. If it were shown that the meaning of sentences relied on non-linguistic knowledge in a general and pervasive way, then the internalist assumption of the separation between linguistic and non-linguistic knowledge and the autonomy of the former would be brought into question.

Let us say I am on a day out with my partner and we are getting hungry. Across the road my partner spies a Deli and suggests lunch in the park. She offers to go and buy lunch and asks what I would like. I respond:

(1) Please get me a tub of my favourite things.

This sentence *could* mean any number of things but let us suspend this question and simply say that my partner returns with dolmades and Kalamata olives. She does not return with pickled garlic and chorizo or any of the many other things the Deli has to offer. And when she returns with the dolmades and Kalamata olives, I am satisfied at her purchases and it does not cross my mind that she did not understand my speaking of (1). Indeed, I consider my speaking of (1) to have been well understood. Communicatively, things have gone well.

But how, from my speaking of (1), did my partner know what to get from the Deli? One answer on offer is that she is familiar with the rules of English and has internalised a grammar, a system of rules and principles that assigns structural descriptions to linguistic expressions and that if we had a true theory of the grammar, we would know what she understands by (1), or indeed what she understands by any other arbitrary utterance in English. This is not a satisfying explanation because it is not clear how my partner's knowledge of English might ascribe a structural description to (1) which would allow a theorist in possession of the linguistic facts to give the meaning of (1), namely that I am requesting something like dolmades and Kalamata olives. Even if my partner has semantic

[84] Harris has contrasted Saussure and Wittgenstein exactly in terms of the issue of linguistic/non-linguistic knowledge; 'Saussure assumes the possibility of a strict segregation between linguistic and non-linguistic phenomena within the universe of human activity…For Wittgenstein, on the other hand, language has no segregated existence' (1988, p.113). For further commentary see Garver in Sluga and Stern (eds.) (1999, pp.148-152).

[85] I mean this term in a theory-neutral and everyday sense.

intuitions about the words 'my', 'favourite' and 'things', there seems nothing in her grammar which produced a structural description of (1) which would sit with the fact that she knew what to get from the Deli.

Another explanation of why my partner understands what she does by (1) is that she knows my favourite Deli items and the items I do not like. This knowledge allows her to interpret 'a tub of my favourite things' in such a way that leads her to return with dolmades and Kalamata olives and this suggests that more than her linguistic knowledge has been involved in the interpretation of (1) and this knowledge may be linked to any other amount of propositional knowledge regarding Aegean cuisine or the currency system of the UK. In her felicitous interpretation of (1), knowledge of the world has played an essential part. It seems reasonable that by (1), my partner understood something like:

(1') Please get me a tub of dolmades and Kalamata olives.

Here, the internalist might respond that the noun phrase consisting of a possessive determiner, plus an adjective plus a non-specific noun, 'my favourite things' has been ascribed a structural description by means of a transformative rule so that 'my favourite things' is given the reading 'dolmades and Kalamata olives', giving us (1'). This would not help the internalist in defending their view of knowledge of language because (1') has not been given as the meaning of (1) 'in a way that depends effectively and solely on formal considerations', but rather on a number of non-linguistic, external factors including my partner's knowledge of my likes and dislikes, offers at the Deli, the amount of change in her wallet and so on. Even if we are generous and allow that 'dolmades and Kalamata olives' is covertly represented in the syntax in a way akin to quantifier domain restriction or ellipsis[86], its presence as a value of some part of the sentence is not due to linguistic knowledge, but to knowledge of the world.

The problem of separating linguistic knowledge from knowledge of the world is famously discussed in Davidson's 'A Nice Derangement of Epitaphs' (2006 [1986]) which represents a marked change to his earlier thought. Davidson is concerned by the fact that speakers use language in nonstandard ways, but are nevertheless understood. He considers malapropisms, one example of which is Mrs Malaprop's,[87] 'a nice derangement of epitaphs' which is understood to mean 'a nice arrangement of epithets' and he provides other

[86] This is a common claim by internalists and semanticists. For example see Chomsky (1975, pp.150-151) and in Stanley's article 'On Quantifier Domain Restriction' (Stanley 2007).
[87] A character from Sheridan's *The Rivals* (1968 [1775]).

examples to make plausible that such phenomena are ubiquitous. To explain the felicitous interpretation of non-standard utterances, Davidson contrasts what he calls 'prior theory', such as knowledge of the grammatical rules of a language and the 'first', or dictionary meaning of words, with 'passing theory' which are momentary hypotheses speakers develop 'on the hoof' in order to interpret particular utterances. Davidson sees the employment of passing theories as ubiquitous and essential to interpretation, though this means that 'we have abandoned not only the ordinary notion of a language, but we have erased the boundary between knowing a language and knowing our way around in the world generally' (Davidson 2006 [1986] p.265). He famously declares that 'there is no such thing as a language, not if a language is anything like what many philosophers and linguists have supposed' (Davidson 2006 [1986], p.265). What is known as the 'no language' thesis has been the subject of much debate[88], and is generally seen as a rejection by Davidson of his own earlier (internalist) account of linguistic knowledge and the corresponding aspirations of a theory of meaning (of which the first quote in this section is representative).

Davidson's thesis in 'Derangement' is relevant here in two ways. It suggests that linguistic knowledge as understood by the internalist fails in its aim of giving the meaning for an arbitrary sentence in a language (what would be partly constitutive of the prior theory). Also, what allows for the interpretation of a nonstandard utterance, or a quite standard utterance such as (1), is a passing theory, something about which one can outline no rules for in advance. Not only is the distinction between linguistic knowledge and knowledge of the world not separate, but what role knowledge of the world may play in the interpretation of an arbitrary sentence cannot be known. It is not just as Stroud suggests that, '[c]ommunicating involves saying something, and so doing something, and knowledge of the meanings of words alone cannot tell you what to do, or what someone else is doing' (Stroud in Davidson 1998, p.90) but it is that this 'doing' is itself so varied that one cannot add an incomplete prior theory to an incomplete theory of passing theories to produce a neat characterisation of a speaker's linguistic knowledge or competence.

We can see this if we return to (1). Here, it may be supposed that what is needed for a speaker/hearer to interpret (1) as (1') is that two speakers (X, Y) share a language (L) and possess a theory of the other speaker (tX, tY) and a theory of the theory the other speaker has about them (totX, totY) so that X has L, tY and totX and Y has L, tX and totY.

[88] See Bar-on and Risjord (1992), Pietroski (1994) and Davidson (1998) for an overview of the debates surrounding the 'no language' controversy.

We can even stipulate that theories have the same content and are different only with respect to their possessors. Now, the possession of L and of tX/tY and totY/totX cannot guarantee the successful interpretation of (1) as (1') as at any point events may occur which would require the implementation of a passing theory. Mishearing and slurred speech aside, this is because speakers can be witty, sarcastic, quote, use metaphor and generally be creative with language, as well as making mistakes such as spoonerisms and malapropisms. To illustrate, say we have a similar situation to the one described around (1), only that now, just before myself and my partner stop for lunch we overhear this snippet of dialogue:

A: Get me a tub of my favourite things.
B: I'm not a mind reader. What do you mean 'favourite things'?
A: Oh you know, like dolmades and Kalamata olives.

Then, when we come to stop for lunch soon after, I am taken by the muse and utter (1), referring as I understand it to the overheard conversation. My utterance of (1) is interpreted as (1'), but my partner has used more than just prior theory; she has used what might only be described as her general intelligence and has formulated a passing theory to understand (1) as (1'). Here, possession of L, a theory of the speaker and a theory of the theory the speaker has about you is not sufficient for the interpretation of (1). This is not to privilege passing theories over prior theories, but to emphasise that knowledge of language and knowledge of the world are pervasively interrelated. I agree with Pietroski's point that while 'passing theories are not themselves theories of languages in any traditional sense, they do not spring into existence ab initio. What speakers have learned will be relevant- indeed, *essential* -to the passing theories they construct' (Pietroski 1994, p.103), and this is true in two ways. Pietroski is referring primarily to language rules[89] as part of prior theories that inform passing theories, though as we have seen with the first example of the speaking of (1), knowledge of the world can be part of our prior theories also. What speakers have potentially learned about anything can come to play a part in the theories they construct, both of prior and passing theories.

Given my acceptance of the interrelated nature of linguistic knowledge and knowledge of the world, it could be argued my understanding of language as a power is brought into question. This is because the conclusion that 'there is no such thing as a language' in 'Derangement' is the result of an inability to separate knowledge of language

[89] 'If Chomsky is right, at least some (and perhaps much) of the interpretive work done by a prior theory is done by an innate system of rules' (Pietroski 1994, p.104).

from knowledge of the world. If I accept the conclusion, it is not clear how I can maintain languages exist and if there are no languages, then language cannot be a power. The first point against this worry is a scholarly one. It is not Davidson's post-Derangement position that languages do not exist, as he qualifies his 'no language' remark in terms of 'what many philosophers and linguists have supposed'[90]. Davidson sees no problem in accepting what he calls an 'institutionalised linguistic background',

> as long as it is taken as saying that members of a "speech community" share a host of overlapping, non-identical, habits of speech, and have corresponding expectations about what others in the community will mean by what they say (such a set of expectations is what is characterized by what I called a "prior theory") (Davidson 1998, p.109).

This is perfectly consistent with my position on language as a power. In 2.3.3 I argued that languages existed on scales of intelligibility which can be seen in the case of Portuguese/Spanish and I was explicit in seeing communication events as polygenic, involving other powers which it will act with. Strongly anthropocentric powers such as language are to be viewed in broad terms as the social realm is casually open. The 'no language' thesis appears to be directed at internalists and does not require the abandonment of the idea that languages exist.

Another reason why my understanding of language as a power does not conflict with my position on knowledge of language is that we still have a clear sense that different languages exist, as we refer to them in order to explain such things as the need for translators or the existence of language schools and French GCSEs. Our intuitive explanation of such things rests on the idea that people speak different languages and this means they can do different things. The unavoidability of describing such situations and examples without begging the question suggests this cannot be wrong.

One can accept a distinction between knowledge of language and knowledge of the world without accepting the autonomy of linguistic knowledge or the internalist account of its involvement in felicitous interpretation of sentences. What has to be given up is the idea that knowledge of language plus certain physiological abilities is sufficient for felicitous interpretation of sentences, suggesting that internalism should be abandoned.

3.3.2 A worry about learnability

[90] Pietroski (1994) also argues against seeing Davidson as sceptical on the existence of languages.

Though we may reject internalism, there are potential problems for externalist accounts concerning the interdependence of knowledge of language and knowledge of the world. I consider one of these to pre-empt a likely criticism that could motivate slipping back into internalism, but also to sharpen what I think an externalist account of linguistic knowledge should be.

The issue concerns the learnability of languages. In his pre-Derangement work, Davidson argued that 'a satisfactory theory must discover a finite basic vocabulary in verbal phenomena to be interpreted if it is to prove useful to a creature with finite powers' (Davidson 2001, p.xv) and was concerned his account of knowledge of language explained why languages were learnable. As speakers are mortal and do not possess an infinite memory, their ability to understand a potential infinity of sentences must be based on a finite set of rules, utilised in the interpretation of arbitrary sentences in their language. For a language to be learnable (or more generally to be a human language)[91] it must contain a finite amount of semantic primitives, described in the following way:

> Let us call an expression a semantical primitive provided the rules which give the meaning for the sentences in which it does not appear do not suffice to determine the meaning of the sentences in which it does appear. Then we may express the condition under discussion by saying: a learnable language has a finite number of semantical primitives (Davidson 2001 [1965], p.9).

Davidson goes on to discuss positions he argues generate infinite semantical primitives and are therefore unlearnable in principle. I do not deal with these as my concern is how this position relates to discussion of knowledge of language. If knowledge of the world impinges on knowledge of language pervasively (as discussions of (1), (1') and passing theories suggest) then according to Davidson here, an account of knowledge of language that does not see it as autonomous from knowledge of the world should be unlearnable because new rules can enter the language at any time and often do by means of passing theories. It is then appropriate that one rejecting an internalist account of linguistic knowledge responds either by showing that Davidson's criteria for learnability are too strong or by showing how an externalist account can in fact meet such criteria. As I consider the chances for the latter as bleak, I will focus on the former.

Haack offers a criticism of Davidson that I think can be generalised to show that

[91] Chomsky holds a similar concern, though he predictably gives greater emphasis to the innately specified character of knowledge of language and so downplays the extent to which agents learn a language from experience as 'we expect that languages are 'learnable' because there is little to learn' (Chomsky 2000, p.124).

Davidson's criteria for learnability are too strong. Haack notes, 'if we assume, which is plausible, that number words are part of the English vocabulary, then it would seem that English has an infinite number of words, and this seems to go against Davidson's view' (Haack 1978, p.234). Haack though is generous to the spirit of Davidson's view and suggests that the infinity of numbers can be generated by the morphological rules of the language and so this need not present a problem in terms of generation of infinite semantic primitives. However, this point raises the idea that items in a language are or at least can be infinite and this causes a problem for Davidson. Consider neologisms. Neologisms are a frequent and undeniable occurrence and while we may be able to account for some of the features of the word 'internet' (admittedly no longer a neologism) or 'intersectional'[92] by reference to features and rules of English (consider ecological and genetic motivation in 3.2.4), knowing those rules and features cannot give us the meaning of 'internet' in a way that the morphological rules concerning number-word formation might plausibly give the meaning of 'three hundred and fifty six'. More is needed, for example ostensive definition or experience with the object or concept concerned. Learning new words and concepts is neither an innate process (1.3.3) nor one obviously derivable from synchronic semantic primitives. If we consider again Haack's general point about an infinity of words going against Davidson's view about the learnability of languages, we can see that while is this is not a problem for numbers, it is a problem for neologisms because additional semantic primitives can enter the language at any time.

That languages change not only semantically, but phonologically, grammatically and syntactically, requires that an agent's knowledge of language loosely mirrors changes in the language if an implausible gap is not to open up between the two. It is a diachronic fact that new rules can enter the language at any time (as well as be removed, altered) and there is no obvious upper limit to these changes, some of which will add[93] semantic primitives to the language. Therefore we have reason to think Davidson's account of a learnable language must be too strong, for the semantic primitives of a language can increase without obvious limit and all languages are by definition learnable or acquirable. One may think I am missing the point as Davidson is concerned with accounts of some aspect of language (quotation, indirect discourse) *generating* an infinite number of semantic primitives by ascribing each token of a feature of a language the status of a unitary predicate. It may

[92] See http://rdues.bcu.ac.uk/newwds/2008.html Accessed: 5/5/11:12.00

[93] One may argue I have not sufficiently specified the nature of 'addition' and that some rule additions to a language are derivable from pre-existing rules within the language. While this might be the case in some instances, the idea that rule addition is not simply derivable from existing rules is commonly accepted, see (Holt (ed.) 2003).

be argued that language change is not like this because change happens slowly enough for speakers to learn new semantic primitives and so does not generate semantic primitives in the relevant sense. I think the similarities are more impressive than the differences though, as with additive changes there will be clear cases where 'meanings are not given by the rules already mastered' and where this is the case, according to Davidson, '[i]t is natural to say that such a language is *unlearnable*' (Davidson, 2001 [1965], p.8). The fact speakers learn new vocabulary and grammatical rules then does appear to go against Davidson's account of a learnable language.

This leads to with another reason we can consider Davidson's criteria too strong is because the individual speaker seems to be in a position which is the same as if they knew an 'unlearnable' language. As Haack points out,

> Speakers of English differ among themselves with respect to the number of grammatical rules which they accept or behave in accordance with. Even if there were an infinite number of grammatical rules of English, so long as different speakers shared some of a finite number of these, they could produce new sentences, communicate with one another … it is not required that a speaker know all the lexical items of a language, so it is not required that a speaker know all the grammatical rules of the language (Haack 1978, p.235).

Internalists appear to take for granted that speakers of a language understand any arbitrary sentence in their language in virtue of the rules that make up their knowledge of the language. This is false. We ask the meaning of words and sentences all the time and as English speakers, will probably die without complete knowledge of English. Given that this is the case, there is little difference between speakers who are partly ignorant of their language (all of us) and a language that contains unlimited semantic primitives. There being little difference suggests that having a language with infinite semantic primitives does not make a language unlearnable because all speakers are in a position as if they did know such a language and yet they still clearly learn languages.

What this means for a view of language which sees linguistic knowledge and knowledge of the world as non-autonomous is that it is not in principle unlearnable because Davidson's criteria are too strong and so the worry is defused. Language learning is not just a matter of having an internally represented grammar, it is also a matter of having the second-order ability to learn and develop interpretative strategies. Here language change is relevant because actuation implies communicative success. For acts of actuation to transmit additional semantic primitives through the language, a passing theory will be required whenever the new element is encountered by speakers for the first time. It is plausible then that a language with potentially infinite semantic primitives would still be

learnable. As 'Derangement' clearly shows, without something like 'general intelligence' compromising the strict distinction between linguistic knowledge and knowledge of the world, novel utterances and novel ways of understanding novel utterances would be a mystery, as would be many examples of language change.

3.3.3 Knowledge, speech and change

The discussion of knowledge of language is relevant to topics in this chapter and to broader issues within this thesis. The failure of the internalist account of knowledge of language shows again how attempts to see language as an autonomous object capable of being strictly defined and studied, something associated with the Formalist Attitude, falters as the 'linguistic' and 'non-linguistic' have been shown to be intimately interrelated. This interrelation is also apparent in consideration of arbitrariness and language change. An argument in 3.1 and 3.2 was that the interrelation of social activity and speech is overlooked by linguists and philosophers, something I argued in chapter 2 can be partly explained by scepticism about social causation. In the case of arbitrariness, where 'arbitrariness is the rule, not the exception' (Lord 1974, p.20) it was seen that the opposite looks to be the case. Once arbitrariness is properly understood and the various ways in which language is motivated considered, the pervasive and interesting links with social activity become clearer. The same is the case with language change, where changes cannot avoid being mediated by a social process of transmission for a change to be completed. A position which holds that 'language is not properly regarded as a system for communication' (Chomsky 2002, p.76) can only be blind to the fact that what speakers use language to do influences change and it cannot be sensitive to the idea that Actual Speech, rather than abstract laws or universal parameters, is the engine of change.

Despite the non-autonomy of the linguistic and non-linguistic, I have argued above that particular languages exist (2.3). While knowledge of language and knowledge of the world are integrated and *integrate*, one cannot explain why I've disappointed Japanese tourists visiting the Royal Pavilion or why Richard Nixon and Mao Zedong required translators on the former's 1972 state visit to China without presupposing that particular languages exist, the evidence for which is that some speakers (of English) can do things that other speakers (of Mandarin) cannot and vice versa. The consideration of knowledge of language, arbitrariness and change can be framed more broadly in terms of an old but recurring debate around the relationship between competence and performance or *langue*

and *parole*. Interrelations between linguistic structure and language use and what this means for linguistics will be the central issue in the next and final chapter.

Chapter 4
Language, linguistics and their place within the sciences

This final chapter gathers arguments developed throughout the thesis in order to draw some conclusions about the nature of language and linguistics. From the outset, it has been stressed that the methodological standards of natural science are related to how language is conceived and this has influenced ideas about what linguistics is. Seeing linguistics as one of the natural sciences or especially associated with them can be motivated by a pre-existing view on what language is, or one can begin with a view of what constitutes 'scientific' study and fit the object of knowledge to this mould. In either case, this can and has led to a study of language which is too abstract, insensitive to the social reality of language and which fails to police its own self-imposed borders of enquiry.

Having given my own, albeit partial, account of language, it is appropriate to show what such an account means for linguistics. In this chapter I add more detail to the former in order to discuss the latter. In the first part, I offer a model of how language functions as a social object and for a speaker and the relation that exists between the two. The purpose of such a model is manifold. First, against accounts that have failed, to explain in a general way how language can change. Second, to show how my observations and arguments concerning language are consistent with this model. And lastly, to explain more fully what language is. Informed by the discussion in the first part, I then move on to answer a question fundamental to linguistics: what kind of a science is it? By 'kind' here, one can mean several things. One can identify a particular discipline in terms of the traditional prism of the natural and social sciences and argue, by comparison with paradigmatic disciplines under those rubrics (physics and sociology respectively) that a discipline is one or the other, or perhaps uniquely between the two (see 1.1). Also, in identifying the kind of science a particular discipline is, one can make broader claims about what laws prevail and the manner of explanation and prediction the discipline offers. The way in which one answers the first question relies upon the answer to the second, as the distinction between the natural and social sciences is often based on metaphysical distinctions in the object of knowledge and the perceived explanatory and predictive power of natural and social scientific theories[94]. The second part of this chapter will take arguments discussed in

[94] See Bhaskar (1979), Braybrooke (1987), Martin and McIntyre (eds.) (1994) and Mantzavinos (ed.) (2009).

previous chapters about the object of enquiry and the laws and explanations available to linguistics to their conclusion by arguing that linguistics is a social science. This chapter contains nothing as ambitious as a general theory of language or of linguistics. Rather I seek to provide some underlabouring for linguistics and open the path for further investigation that does not commit to the Formalist Attitude. To this purpose, I will also seek to show how my observations fit with two current trends in linguistics; cognitive and integrationist linguistics.

4.1 Language, structure and agency

The desire to understand how structure and act, *langue* and *parole*, or competence and performance[95] interact is old and attempts to model and describe the relationship between language structure and agential use of that structure has been a persistent question in linguistics and the philosophy of linguistics (Saussure 2006 [1922], Jameson 1974, Pateman 1987, Thibault 1997). This is partly because a true model could not only tell us interesting things about the nature of language, but such a model would be a methodological tool in linguistics for understanding language change, language stability and the importance of Actual Speech and social activity to language. The desire for understanding is also motivated by a problem, which I will call the speech/structure problem (S/S for short). It is this:

S/S problem: a linguistic act is inexplicable without a structure constraining and directing it to a high degree. On the other hand, it is implausible that language structure is instantaneously realised and ready-furnished to constrain and direct linguistic acts without there first being semi-linguistic acts that create that structure. This is a problem because it suggests each is temporally and logically prior to the other.

Barthes recognises exactly this problem when he states, 'one cannot handle speech except by drawing on the language. But conversely, a language is possible only starting from speech' (Barthes, 1968, p.16). The S/S problem is not only a 'mists of time' question about how language began (though it is also that). Rather, the problem requires an answer if we are to understand how language functions diachronically in a society and how linguistic acts and structure interact. That the issue concerns the diachronic is clear, as the concept of

[95] These terms are associated with structuralism and generativism respectively.

interaction presupposes temporal progression and the S/S problem partly concerns matters of temporal priority[96]. This is an important point to grasp because within the Formalist Attitude there has been a lack of recognition of the diachronic, motivated by the desire to provide a sufficiently 'scientific' object of knowledge where language structure is seen as stable across a homogenous linguistic community. There have been attempts to characterise the relationship between structure and act which have either failed to take into account certain aspects of language or misrepresent the nature of language. So to bring the issue into sharper focus, before proposing my model I will look at several accounts of how linguistic acts and linguistic structure interact and why they are problematic.

4.1.1 The models, their supporters and their problems

Formulations of the *langue/parole*, competence/performance, type/token kind each possess specificities and carry particular theoretical commitments in tow. In order to discuss the broader issue of the relationship between linguistic structure (hereafter LS) and linguistic acts (hereafter LA), I want to offer a general definition of each that can reasonably represent much of what is meant in *langue/parole* type distinctions.

> LS: The sum total of lexio-grammatical units and combination (grammatical, syntactic) rules necessary for any linguistic act and utilised in linguistic acts.
>
> LA: Any act of articulated sound using a combination of rules and lexico-grammatical units taken from LS and performed in a context.

Such a definition of LS and LA implies an important relationship between LS and LA though it does not fully determine the nature of that relationship. The aim here is to be as neutral as possible concerning the relationship and with this as a basis, I consider some models of how LS and LA interact.

In structuralist and post-structuralist thought and arguably within the philosophy of language, the view of the relationship between LA and LS has been influenced by a Durkheimian model of society, where analogous to 'society', language is seen as a 'social

[96] This is not meant to suggest that the S/S problem is only a diachronic matter. Given that it is a logical problem, it is relevant to synchronic linguistics, as Saussure's and arguably Thibault's (see 4.2.3) interest in the question attests.

fact'[97]. Durkheim describes social facts as 'a category of facts which present very special characteristics: they consist of manners of acting, thinking, and feeling external to the individual, which are invested with a coercive power by virtue of which they exercise control over him [an agent]' (Durkheim 1982 [1895], p.52). Here what is stressed is the power of the social structure to shape the agent and the agent's corresponding lack of power to shape the structure. For a Durkheimian account of the relationship between LS and LA, language is a supra-individual structure whose reality is evidenced by its ability to constrain and enable human action. One commonly recognised example of the influence of Durkheim on linguistic thought is in Saussure's description of language as a 'social crystallisation' (Saussure 2006 [1921], p.29)[98]. The relationship can be shown diagrammatically thus:

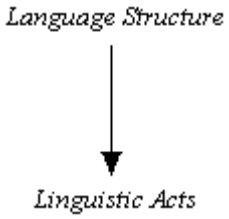

Fig 1.1: LS/LA relation under a Durkheim stereotype

This model describes Saussure's *langue/parole* (LS and LA) distinction, evidenced by Saussure's contention that, '[w]hat [sign] can be chosen is already determined in advance. No individual is able, even if he wished, to modify in any way a choice already established in the language. Nor can the linguistic community exercise its authority to change even a single word' (Saussure 2006 [1922], p.104). In the Durkheimian model the emphasis is unequivocally on the constraint of the social fact upon the agent, so what any speaker says when they speak will, irrespective of what they are trying to achieve, be rigidly constricted by linguistic structure[99]. What follows from this view is that speakers cannot add, subtract or change the linguistic structure. Considered in terms of the S/S problem, the motivation for adopting the Durkheimian model can be seen; insofar as linguistic structure constrains and enables speech, acts of articulated sound that do not rely upon the linguistic structure are not linguistic acts. This gives a criterion upon which to recognise a linguistic act as

[97] See also Barthes (1967), Davidson (1998) and Johnstone (2000).

[98] The influence of Durkheim on Saussure and structuralism is widely recognised: 'Doroszewski's findings concerning Durkheim's influence [on Saussure] have been accepted as established fact by the majority of authors of books related to the history of linguistics' (Koerner 1973, p.48). See also Jameson (1974, pp.27-28) and more recently Thibault (1997, p.8).

[99] This position expresses an internalist view of knowledge of language, see 3.3.

distinct from articulated sound and it ensures the closure of the LS domain, as linguistic structure contains the units and rules sufficient for speech. This has advantages and captures several features of the LS/LA relationship that are arguably worth preserving. First, it gives LS reality above and beyond the linguistic acts of individuals; it is a 'full-blooded' social structure like a monetary system or a company hierarchy, irreducible to the performance of consumers/employees/speakers. Second, it gives a clear sense of how linguistic acts are related to and reliant upon linguistic structure by postulating a seamless and unidirectional relationship between LS and LA. Nothing is given the status of LA unless it is constrained and exhausted by LS. Jameson makes just this point about structuralism, where the *langue/parole* distinction 'make[s] possible a methodological separation of pure sounds (as, for example, the articulations made by a speaker of a language utterly unknown to us) from meaningful sounds' (Jameson 1974, pp.15-16). Indeed, what goes for structuralism here goes for any position adopting the Durkheimian model.

While having advantages, there are problems for the Durkheimian model that make it unattractive. The main problem is that it is too prescriptive; the LS/LA relationship, which is seamless and unidirectional, rules out acts of articulated sound that we would intuitively accept as linguistic acts. We can see this if we consider portmanteau words such as 'Brunch'[100], 'Smirt'[101] and 'Spork'[102]. In the cases of 'Brunch' and 'Spork', these are now established within the English lexicon, but not so with 'Smirt'. Whether it will become so is not the question, but that it might become so, as 'Brunch' and 'Spork' have, raises a problem for the Durkheimian model. A portmanteau word not included within LS must, if it is to be included, be used as a meaningful word. This suggests that at a point before being part of LS, it is *already* meaningful, though this is what the Durkheimian model, postulating a seamless and unidirectional relationship between LA and LS, refuses. One might object that such portmanteau words are not really meaningful as they are directly parasitic on words that *are* meaningful in the ordinary sense, but this is not correct for two reasons. First, words such as 'screwdriver' and 'breakfast' appear just as parasitic as 'Spork' but are taken to be meaningful in their own right and second, the objection does not appreciate that portmanteaus are not simple additive semantic formulas but often describe

[100] According to Merriam Webster: 'a meal usually taken late in the morning that combines a late breakfast and an early lunch.'

[101] This word does not feature in any major standard dictionary as of 1/2/12. It can be defined as 'socializing in a romantic way whilst smoking outside a place such as a bar, restaurant etc, where smoking is prohibited or illegal'.

[102] A trademark used for a plastic eating utensil having a spoonlike bowl and tines. Source: http://www.thefreedictionary.com/Spork, accessed: 5/12/2011.

a new and distinctive object or idea. A spork is not the semantic equivalent of 'a spoon and a fork', but is a single item that has salient features of both (though not all features). For the Durkheimian model, LA are seen as inconsequential in forming LS and as such, the model falls upon one horn of S/S problem in failing to deal with the idea that there are LA that precede LS.

This is not only an issue with portmanteau words, but with any additive language change[103] and the prescriptive nature of the Durkheimian model leaves linguistic change inexplicable. As well as this, it does not recognise our intuition that it is agents that use language to achieve social ends and words such as portmanteaus come into existence as a result of agental response to new or changing social situations. For Saussure, *langue* is a 'product passively registered by the individual. It never requires premeditation' (Saussure 2006 [1922] p.30) and as such it is what Harris describes as 'a purely mechanistic psychological explanation of social conformity' (Harris 2001, p.229), an explanation which casts the agent as playing a necessary, though highly constricted role. This issue has been discussed before in 3.2 and in 1.4, where I argued that both Derrida and Saussure do not recognise the importance of Actual Speech and the social aims tied to speech. It can now be seen that this is plausibly partly as the result of the adoption of the Durkheimian model.

It may be objected that the use of and reliance on a Durkheimian model is the result of a methodological choice and that the study of *parole,* being too varied and heterogeneous, would be impractical. As such, it could be claimed that the Durkheimian model with its emphasis on *langue* is just a practical and pragmatic tool for linguistics. In discussing the *langue/parole* separation, Thibault makes much this point,

> [L]inguists and semioticians…have tended to assume that Saussure's distinction between an 'internal' linguistics of *langue* and an 'external' linguistics of *parole* amounts to a description of the concrete reality of language. In actual fact, the distinction between *langue* and *parole* belongs to a theory of *linguistics*. It is not inherent in the concrete reality of language…[this has] given rise to a confusion between methodology, on the one hand, and ontology, on the other (Thibault 1997, p.6).

Accepting the usefulness of the distinction between methodology and ontology, the corollary of which is the pragmatic distinction between *langue* and *parole*, and 'internal' and 'external' linguistics causes problems. By accepting Saussure's distinction one supports the assumption of a static model of language and of speakers as being in a passive relation to

[103] If one subscribes to the structuralist position that meaning is constituted by the differential value of all linguistic units in *langue*, then this would plausibly extend to all linguistic changes.

LS; the Durkheimian model. This seems unhelpful as it leaves an important area of linguistic study, language change, inexplicable. The claim that such a model is useful for linguistics then appears false. This suggests that a model of the LS/LA relationship which does not create a dichotomy between ontology and methodology might be more useful and allow for language change. Such a model should be able to take into better account such important phenomena, as well as our intuition that it is speakers who use language, change it and develop interpretative strategies to understand it. I have more to say about Thibault in 4.2.4.

In contrast to the structuralist, it might appear that generativism implicitly adopts a different perspective concerning the relation between LS and LA. Labov outlines the broad differences between approaches that give us reason to think this:

> There are two opposing answers to the question, 'What is language?' The idealist conception is that language is a property of the individual, a species-specific and genetically inherited capacity to form rules of a particular type, relatively isolated from other activities of the human intelligence. The materialistic conception is that language is a property of the speech community, an instrument of social communication that evolves gradually and continuously throughout human history, in response to a variety of human needs and activities (Labov 1987, p.2).

The thought is that generativists (idealists) reject the existence of social facts which structuralists accept and instead focus on individual competence (I-language). For the generativist as opposed to the materialist then, the relation between LA and LS looks reversed, leading to what one might call a Weberian model. This appellation is appropriate because of the emphasis Weber placed on individual action and what he saw as the epiphenomenal status of 'collective' or 'organic' supra-individual structures. Weber saw it as,

> 'convenient or even indispensable to treat social collectivities, such as states, associations, business corporations, foundations as if they were individual persons…But for the subjective interpretation of action in sociological work these collectivities must be treated as *solely* the resultants and modes of organization of the particular acts of individual persons, since these alone can be treated as agents in a course of subjectively understandable action' (Weber 1968 [1922] pp.14-15).

In terms of the LS/LA relation, the Weberian model would look like this:

Fig.1.2 LS/LA Relation under a Weberian Stereotype

For the Weberian model, it is the individual speaker who in virtue of LA, creates and sustains LS. If, *qua* structuralism, LS is viewed as a social object, a Weberian model rejects the reality of LS as nothing more than a *façon de parler* that can be reduced to the total collective action of speakers (LA). This deflationary account of LS is not truly representative of generativism. Because of the generativist dichotomy between competence and performance, LS would be appropriately seen as a biological endowment of which the I-language is the realised steady state, rather than a 'social crystallisation' or E-language. While the I-language is the object of study, it is considered static and universal. This is remarkably similar to structuralism, whose object as Johnson points out 'is a "shared" system, that is a system that is of interest only insofar as it can be treated as identical from individual to individual' (Johnson 2000, p.408). In respect of the relationship between LS/LA then, both structuralists (materialists) and generativists (idealists) are in accord.

While the role of LA in the relation has long been a point of dispute in linguistics[104], there have been few advocates of a Weberian-style relation. Whereas in the social sciences, anti-realism and empiricism has been characterised by a denial of the reality of social structures (Weber 1986 [1922] Winch 1959), with only agential action being significant (voluntarism), this has only rarely been the case in linguistics[105]. This is plausibly because the reality of LS can be defined in non-supra-individual ways (a biological endowment) and because the removal of the role of LS leaves speech unexplained. Whereas it is plausible that economic activity is explicable without a full-blooded account of the banking system, or an account of England's 2005 Ashes victory with reference only to the actions of individual players, the same is not true of language. Despite the relative scarcity of Weberian accounts of the LS/LA relation, interest in the role of the individual

[104] See the description of the controversy between anomalists and analogists in Robins (1997, pp.25-28).

[105] While not promoting voluntarism, linguistic nominalism and behaviourism of the early 20th century were notable for their refusal to countenance the reality of LS. For example see L. Bloomfield in Katz (ed.) (1985). As Johnson observes, 'For the most part, linguists make statements about languages rather than about speakers' (Johnson 2000, p.408).

speaker has increased in recent years and in 'The Individual Voice in Language' (2000) Barbara Johnson offers something approaching a Weberian perspective, showing scepticism about the existence of grammars insofar as they are post-hoc generalisations of speech acts. For example she argues that,

> [k]nowledge of language is fundamentally private and individual…This is the result of the fact that people are not born knowing how to talk. Although we say that many American children "learn English," in fact no two learn exactly the same thing. One person's language is different from another's because each individual has a different set of linguistic memories and each may make different generalizations on the basis of what he or she hears (Johnson 2000, p.411).

In light of this Johnson recommends that language be thought of 'as linguistic action rather than linguistic competence' (Johnson 2000, p.411) and that language rules (grammars) be postulated 'without claiming that the "rules" one thereby formulates were actually causal in the process of text-building' (Johnson 2000, p.413). While Johnson does not offer a model of the LS/LA relationship, her position suggests something akin to a Weberian model. There are advantages here which should be considered as they emphasise the flaws of the Durkheimian model and suggest what a more plausible model of the relationship between LS and LA should consist in. One advantage of a Weberian model is that it allows for language change. Whereas the Durkheimian model postulates a seamless relationship between LS and LA, this is not the case with the Weberian model as it is meaningful speech (LA) that informs LS. LA is not given in advance as 'discourse is not created or interpreted via the application of a priori rules' (Johnson 2000, p.413) and as the relationship is not seamless, language change is possible. The Weberian model also captures the intuitive idea that without speakers speaking, whatever passes for LS would not exist. While a Weberian model sees LS as a *façon de parler* reducible to LA, without LA no generalisations (LS) would be postulated so whereas the Durkheimian model sees LS as not reliant upon LA for the maintenance and existence of LS, the Weberian model captures such reliance.

The Weberian model however finds itself on the other horn of the S/S problem from the Durkheimian model as there is the issue of accounting for the existence of LA if LS is epiphenomenal. For example, Johnson's claim about the 'fundamentally private' nature of language appears to rely on too-narrow an understanding of what constitutes a public language, the standard of 'public' being the sharing of *exactly* the same lexicon, grammatical rules etc.; the kind of account evident in support for the Durkheimian model.

Against such an understanding, one might use 'private' as a contrastive term, but this is ultimately implausible. Seeing language as private because no two speakers 'learn exactly the same thing' suggests that someone who formulates the past-tense of the verb 'to spell' as 'spelt' would not be speaking the same language as one who used 'spelled'. This is not a plausible account of what a public language is as there being a *choice* between 'spelt' and 'spelled' relies upon language being public. If a speaker who says 'spelt' thinks 'spelled' is incorrect, the fact they can consult an English grammar book and come away convinced of their mistake again emphasises this. If this were not the case then the idea there are *choices* or *options* within languages would make little sense. While Johnson may be reacting against one implausible account of language (the Durkheimian), her own position is, for different reasons, implausible.

Having discussed the Weberian and Durkheimian models, their proponents and the problems besetting each, I want to argue for a conception of the LA/LS relationship that captures the insights of each model while avoiding their respective problems and both horns of the S/S problem. The function of this model is not just to avoid problems, but to bring together insights from this thesis: that Actual Speech is an important consideration in explaining how change is possible (1.4/3.2), that language is a power (2.3) and that language is not arbitrary (3.3). My model will not imply my other observations, but it will be shown that it is happily consistent with them and a plausible extension of them.

4.1.2 A transformational model of linguistic activity

Following Roy Bhaskar, I propose a 'transformational model' to describe the relationship between LS and LA. In *The Possibility of Naturalism* (1979) Bhaskar approaches the issue of the society/agent relation and outlines the drawbacks of Durkheimian/Weberian accounts, which he argues diminish the role of agent and society respectively by either reifying agents (Durkheim) or adopting voluntarism (Weber) and denying the existence of society. Against this Bhaskar argues for a transformational model of social activity. It is not important to repeat his arguments in detail as my arguments here make similar criticisms, though of course directed toward linguistics. However it is worth noting an observation Bhaskar makes with respect to the agent/society relation as it is similar to the S/S problem:

> It is still true to say that society would not exist without human activity, so reification remains an error. And it is still true to say that such activity would not occur unless agents engaging in it had a conception of what they were doing (Bhaskar 1979, p.42).

This suggests (and Bhaskar argues) that society and agent are mutually irreducible necessary conditions of each other and the absence of this insight leads to reification or voluntarism. If we apply this to language and the LS/LA relation, we can see an analogy to the S/S problem, as a privileging of either LA or LS leads to a problematic denial of the role of speakers or a problematic denial of the existence of LS. A transformational model deals with the S/S problem directly and opposed to the Durkheimian or Weberian models, recognises that linguistic structure pre-exists any act of speech (one horn of the S/S problem) but equally recognises that linguistic structure exists in virtue of speech and linguistic structure is reproduced by speech (the other horn of the S/S problem). In terms of S/S problem, the transformational model tries to have its philosophical cake and eat it. It looks like this:

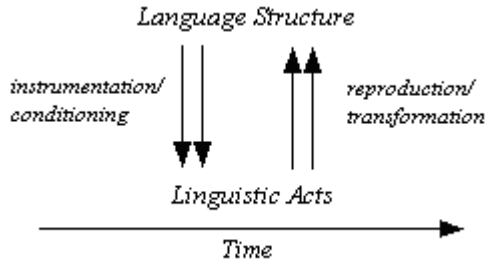

Fig 1.3: LS/LA relation under a transformational model

The diagram shows the LS/LA relation as dynamic and non-synchronic. The inclusion of the time arrow quantifying over LS and LA recognises the diachronic functioning of language, though seeing it as a 'diachronic model' is not accurate insofar as this implies an acceptance of a synchronic/diachronic dichotomy. The double up/down arrows represent the duality of praxis and structure, describing the ways in which speech and structure interrelate and are mutually supportive:

Reproduction: Linguistic acts sustain or reproduce language structure, as spending reproduces the monetary system. Without linguistic acts, we would have little reason to think language structure existed. Here again we can see the importance of directedness (2.3.2).

Transformation: As well as reproducing, linguistic acts potentially transform language

structure. As I outlined in 3.1 and 3.2, linguistic action is social action and language is used by agents to achieve things, which does *not* imply that language remains the same (3.2.2). If I want to describe a new invention, impress a peer with wit or any number of things, I have motivation for being linguistically creative, which has the potential to change language structure.

Instrumentation: Language structure allows to some degree the instrumentation of speech, for if we had not learned or acquired language rules then it would be difficult to explain speech (3.3) or to put it conservatively, to explain the obvious ways in which speech is rule-influenced.

Conditioning: Structure though is not instantaneously realised in the speaker as speakers are constantly being conditioned[106] by their linguistic environments, whether this be by learning pre-existing but previously unknown rules in the language or by learning about changes in the language.

Under the transformational model (hereafter TM), LS and LA are a mutually irreducible necessary condition for each other. Without recognising that the relationship between LS and LA is not seamless (transformation/conditioning) as it is for the Durkheimian model, change becomes inexplicable. On the other hand and what was a problem for the Weberian model, without recognising the need for constraint and a degree of continuity (instrumentation/reproduction), speech becomes inexplicable. The TM then is a less problematic model than the Durkheimian or Weberian models, though retains the advantages of each. Like the Weberian model the TM is able to account for language change because while LS informs LA, the relationship is not seamless. LA is necessary for the existence, maintenance and transformation of LS and this requires it not be reducible to it. This point is important with respect to my proposal of Actual Speech made in 1.4 where I argued this was suppressed in Saussure and Derrida in order to maintain a clearly defined purview for linguistics. I defined it like this:

> Actual Speech: A spoken linguistic act A which, belonging to a language L reproduces and potentially changes language L.

[106] An interesting overlap between the transformational model and language change is evident. 'Transformation' and 'conditioning' are analogous to 'actuation' and 'transmission' in theories of language change.

This definition fits into a wider conception of the relationship between LA and LS. Under the TM Actual Speech captures the sense of LA as the model recognises the analog nature of communication as opposed to a presupposition of synchronic stasis or a homogenous speech community. It is important to recognise though that linguistic acts are acts of agents trying to achieve something with language and Actual Speech is not simply the correct use of grammatical rules and lexical items. Actual Speech is activity and it is in trying to achieve something with language that acts of speech are *acts*. As well as this, the TM does not cast language as a closed system or an object amenable to study by a discipline requiring 'hard and fast frontiers'. On the contrary, there is recognition that language functions in an open system and things such as messy speakers and their messy neologisms, solecisms and revived archaisms are ineluctable facts of language. In many respects then, the TM stands against the assumptions of the Formalist Attitude.

It may be argued that the TM has a significant disadvantage over other models, because in not representing LS as static, it cannot give a clear and bounded description of LS and thus exposit its contents. While static models have drawbacks, it may be argued, assuming stasis is a useful and necessary methodological assumption. Consider again Thibault's quotation above concerning the methodological justification for the distinction between 'internal' and 'external' linguistics and my objection to it. There I pointed out that in spite of claims about the usefulness of such a distinction, it does not allow one to explain language change or to decide if 'smirt' (to smoke and flirt) is or is not part of the English language. Indeed, if one accepts the assumption of stasis is useful, one is admitting that models do not aim at a *linguistic* description of LS, just a useful one in some respect or other. However, a model which sees the relation as dynamic (reproductive and transformative) rather than static need not be concerned by the possibility and actuality of liminal and less-than-clear-cut cases as it does not see the LS/LA relation as seamless and so expects them. This is not the case with the assumption of stasis though and liminal and less-than-clear-cut cases provide a problem.

4.1.3 Thibault's transformational model

Using a transformational model to describe the LS/LA relation based on Bhaskar's analysis has been attempted before. In *Re-Reading Saussure: The Dynamics of Signs in Social Life* (1997), Paul J. Thibault suggests a LS/LA relation similar to mine and examining his position will

help to place mine within a wider context of linguistic thought but more importantly, will show how my model is preferable.

My approach and Thibault's have a number of similarities. Thibault is concerned to avoid the reifying consequences of the Durkheimian approach and to account for the role of speakers as more than mere passive recipients of LS. In this spirit, Thibault claims of *langue/parole* that, '[t]he relationship between the two perspectives is not a dichotomy. The dichotomous reading can only put the relation between *langue* and *parole* and between society and individual, in a straitjacket' (Thibault 1997, p.344) and he points to a Durkheimian reading of Saussure (Thibault 1997, p.8) as responsible for this 'straitjacket'. To remedy this, Thibault accepts aspects of Bhaskar's transformational model and applies it to the LS/LA relation in order to express a relationship that, like my model, emphasises that 'signs are not fixed and closed; they are open and dynamic processes' (Thibault 1997, p.254). However, despite recognition of the drawbacks of a Durkheimian account of the LS/LA relation and the adoption of a position influenced by the transformational model, there are problems with Thibault's position which stem from an adherence to structuralism and a desire to rehabilitate Saussure's linguistics. Thibault equivocates at various points, appearing committed to the utility of a static model and denying the reality of *langue* and *parole*, which are both inconsistent with his acceptance of a transformational model. For example, he endorses a position, ascribed to Saussure, which remains problematic:

> [T]he properties possessed by *langue,* which is a transindividual social-semiological system, and the properties possessed by individual language users in acts of *parole* are, ontologically speaking, very different (Thibault 1997, p.67).

Thibault here tries to justify the separation of *langue/parole* that Saussure imposes on linguistics by distinguishing between properties of *langue* and properties possessed by speakers in acts of *parole*. While not immediately clear what constitutes 'very different' in this context, there is a potential problem in stressing ontological differences between *langue* and *parole* as one of the things the transformational model emphasises is that LS (*langue*) and LA (*parole*) are ontologically linked in being a necessary condition for each other. As they are mutually irreducible, one cannot describe one without reference to the other and this is as much an ontological point as a methodological one. What speakers need to be in possession of for acts of *parole* is something converging with *langue* and therefore it is not clear they have a 'very different' ontology and in one salient sense, they do not.

Also, Thibault's stance that the ontology of *langue* and *parole* are 'very different' appears in contradiction with claims that *langue* and *parole* are theoretical abstractions of

langage. At several points, Thibault appears sceptical of the reality of *langue* and *parole*: 'A synchronic state of *langue* is an analytical abstraction from the concrete reality of *langage*. In this sense, it is an idealization' (Thibault 1997, p.12). If *langue* is an abstraction then by implication *parole* must be and Thibault's claim that *langue* and *parole* are 'not inherent in the concrete reality of language' (Thibault 1997, p.6) suggests that neither abstract term is real. This is a problem because Thibault's acceptance of the transformational model appears to ignore that it is developed in order to *avoid* the conclusion that either the individual agent or society is irreal or reified; by analogy LS (*langue*) and LA (*parole*). However, in seeing *langue*/*parole* as abstractions from *langage* that are not part of its 'concrete reality', Thibault does just this. This also makes his claims about *langue* and *parole* being ontologically 'very different' obscure as it is unclear if an analytical abstraction has ontological properties.

Another problem for Thibault is that he fails to consistently recognise the importance of the diachronic and the analog nature of communication due to his acceptance of the methodological necessity of Saussure's separation of *langue* and *parole* and 'internal' and 'external' linguistics, which I criticised in 4.2.1. On this point Thibault supports Saussure's position, which he sees as this:

> The primary task of the linguist is to study the internal principles of organization, so defined. 'External' linguistics, which is seen by Saussure as 'secondary' to the more 'essential' internal linguistics, is concerned, on the other hand, with individual and social *uses* of language (Thibault 1997, p.6).

The separation of internal/external linguistics and the privileging of the latter suggests a separation of the competence/performance kind where language use is cast as the mere instantiation of language structure. Such a distinction was brought into question in 3.3, where I argued that what was seen by internalists as 'knowledge of language' was not sufficient for understanding linguistic acts. The point is that even if this separation is motivated by *methodological* concerns belonging 'to a theory of linguistics…not inherent in the concrete reality of language' (Thibault 1997, p.6), it still assumes that this kind of separation and privileging allows for fruitful study. However, it is not clear that an account which relegates the *uses* of language to secondary status, opting for the more 'essential' study of the internal principles of organization, will be fruitful in understanding language. Therefore, as recognition of the interconnectedness and mutual irreducibility of LS and LA is essential to TM, there is a tension in Thibault accepting TM *and* defending Saussure.

All this makes the aim of Thibault's support for the transformational model ultimately unclear. It is unclear if he sees *langue* and *parole* as real or as analytical abstractions

and the acceptance of Saussure's separation of 'internal' and 'external', even on methodological grounds, assumes a relation between *langue* and *parole* such that study of *langue* is prioritised and separated from the uses language is put to. Thibault's desire to defend Saussure leaves his position closer to the Durkheimian model than his theoretical intentions inferred from the adoption of a transformational model would suggest. With these problems present, my model by comparison is more consistent with the tenor of Bhaskar's analysis. I do not see LS (*langue*) and LA (*parole*) as abstractions and there is no assumption of a static model or for a methodological/ontological distinction of the kind Thibault and Saussure suggest. As such there are not the accompanying problems.

I have now developed in some detail my answer to the question 'what is a language?', an answer which began in 2.3 and continued throughout chapter 3 by critical engagement and positive argument. I have shown how some answers to the question, influenced by the Formalist Attitude, are beset with problems and I have offered an account which avoids problems by being sensitive to the social nature of language and the fact that language is used by agents for social purposes in a causally open and causally unclosable environment. In offering a transformational model of linguistic activity, I have shown how arguments made in the thesis integrate and are consistent with the transformational model.

4.2 What kind of a science is linguistics?

The thesis began by showing how the question of the scientific status of linguistics has been and is a concern for linguists. It also showed how the natural sciences have provided a model for linguistic study and even where linguistics is not seen as one of the natural sciences, its association with them justified the idea that linguistics has something to teach the social sciences about being scientific. This is not wishful thinking on the part of linguists, but is a view shared by prominent social scientists. As Levi-Strauss once claimed, linguistic publications 'must also welcome psychologists, sociologists, and anthropologists eager to learn from modern linguistics the road which leads to the empirical knowledge of social phenomena' (Levi-Strauss 1993, p.31). In these concerns and criticisms this thesis finds itself in some agreement with other critical studies of the claims of linguistics such as Itkonen's *Grammatical Theory and Metascience* (1978), Harris' *The Language Myth* (1981)[107] and to a lesser extent Pateman's *Language in Mind, Language in Society* (1987). All these have been

[107] Unlike most linguists, this has been an abiding concern for Harris. See Harris (1980, 1987, 1988, 1996).

critical about how linguistics has been influenced by formal approaches and positivism. Especially for Harris in his development of integrationist linguistics (which I look at soon), there has been an emphasis on the problematic abstractions of 'autonomous' linguistics and on the idea that linguistics is happily established with the family of the sciences.

In this final section I outline some of the implications of my arguments for linguistics as a science. I then conclude by contextualising my thesis within the current field of linguistics by looking at two schools of linguistics that are broadly compatible with my position.

4.2.1 Scientific status and theoretic boundaries

What kind of science is linguistics? Linguistics is not a natural science and attempts to associate itself with them have been seen to fail. Nor is linguistics' claim to being the paradigm of scientific rigor among the social sciences warranted insofar as this rests on it having a special association with the natural sciences. This does not mean that other social scientists might not learn from linguistics, but it also means that linguistics cannot be closed to learning from social science. I claim that linguistics should be seen as a social science for the following reasons, which mirror criteria I set out in 1.1:

> - *The 'laws' governing language are not those of the natural sciences.* There are no 'general laws which account for all particular linguistic phenomena' (Saussure 2006 [1922], p.20). Whereas the laws of nature are immutable and necessary, this is not the case with language and more broadly the objects of social science, whose rules or powers are normative, contingent and subject to change, as has been shown in discussion of language change (2.1/3.2). In a theory of grammar[108] for example, the statement 'English has two articles: 'a/an' and 'the" cannot be a law in the natural scientific sense, because it is mutable and unfalsifiable; if someone says 'Seo[109] apple is gone', one has simply failed to speak (demotic) English and it is unclear this can be doubted.
>
> -*The relation between prediction and explanation.* The natural sciences explain events with reference to proposed laws and one test of these laws is the extent to which they can be used to offer accurate predictions. In contrast to this expectation, the

[108] Grammatical theory has not been dealt with in this thesis, but for a discussion of it, see Itkonen (1978).
[109] Anglo-Saxon feminine definite article.

explanations offered in the case of language change are post-hoc and do not refer to laws (2.2/2.3) but to normic statements, which are not open to the same degree of confirmation as laws of the natural sciences (3.3). As was argued in discussion of arbitrariness (3.1), language is pervasively motivated and there are many kinds of motivation (ecological, structural, iconic etc.) which are likely to simultaneously influence motivation in any one case. Again in such cases, explanation can only be post-hoc. Itkonen makes this point in a similar vein; 'even though I understand people's actions in a way in which I cannot hope to understand physical events, I know what people have done or will do with much less certainty than I know which events occurred or will occur' (Itkonen 1978, p.196).

- *Linguistics is not predictive.* Linguistics is not predictive, which is widely accepted for the social sciences (2.2) generally. Language functions in an open and uncloseable system and this is made abundantly clear by failures to predict language change (1.1/2.1/3.2). The indisputable predictive success of the natural sciences emphasises this difference. As was argued in 3.3, knowledge of language cannot fruitfully be seen as apart from knowledge of the world when considering interpretation and this suggests that grammars cannot generate or predict the meaning of arbitrary sentences in a language in the way internalists wish[110].

- *Linguistics' object of knowledge is social and exists in an open system.* Language exists in virtue of people and it is by agents' Actual Speech, which always aims at *doing* something, that language is reproduced and changes (4.1). Language is also an object of common knowledge or mutual belief and is a strongly anthropocentric object (2.3). As well as this, language functions in a causally open system and unlike the paradigmatic natural sciences which can create a degree of closure in the context of laboratory experiments, language functions, like the objects of knowledge of other social sciences, in an unclosable system. This is clear if we consider the importance of language to other social institutions and its role in all aspects of social life.

I make a number of claims about the social sciences above, among them that they do not

[110] Interpretation and a theory of it is of course a much larger and hotly debated issue. For an explanation of the current field from a broadly internalist view, see Stanley (2007) and for a contrary stance, see Travis (2008).

offer predictions, their objects of knowledge exist in causally open and unclosable systems and that such differences are constitutive of distinction between the social and natural sciences. I have discussed these issues in 2.2 and 3.2 and they are widely regarded as uncontroversial in the philosophy of social science (Sorokin 1938, Bhaskar 1979, Braybrooke 1987), so I do not defend them further. Given the above reasons, linguistics appears more like a social than a natural science. For anyone concerned this condemns linguistics to being a 'non-science' Braybrooke offers these, I think wise, words:

> The charges that social science fails as science lose a good deal of their plausibility anyway if we cease to treat science as an all-or-nothing matter. We need not exclude every enquiry that fails to be as rigorous as classical mechanics. We can instead look upon science as a matter of degree (Braybrooke 1987, p.43).

Another way of approaching the question of the kind of study linguistics is, is to ask what linguistics *is for*. Throughout its modern history, there has been a promotion of the study of language for its own sake (Saussure 2006 [1922], Hockett 1958, Evans and Green 2007). Harris has also claimed that 'languages seemed to the historical grammarian to live lives of their own' (Harris 1981, p.50) and as we have seen, this view is evident in contemporary thought on language (3.2). If language did have 'a life of its own' in anything more than a metaphorical sense then the motivation for the study of language would arguably be analogous to the study of the objects of natural science: to exercise humankind's natural curiosity and discover the nature of the object of enquiry, giving us a broader picture of the external world. This gives a fairly clear sense of what study of language 'for its own sake' is. This though is based on the supposition that language is an autonomous object, and as this is not the case, so language study for its own sake is not viable. In the well known *Short History of Linguistics,* Robins recognises this and another motivation for linguistics,

> Despite [a] general acceptance of the gift of articulate speech, most cultures in the world have engendered among certain of their members some realisation of the scope of the power of language. This linguistic self-consciousness may be first stimulated…by a particular orientation of man's inherent and disinterested curiosity about himself and the world around him (Robins 1997, p.1).

Here Robins suggests the study of language is motivated and justified because of the importance of language to the social life of human beings, something that textbooks on

linguistics typically recognise, whatever their philosophical and methodological leanings[111]. Language is important because it is a pervasive and ceaseless part of our social activity: political debates, relationships, rituals and how we find our way about the world and do things in the world. It is plausible therefore that linguistics is part of the investigation of what it is to be human, an investigation that belongs most obviously to the social sciences. Language is of interest because of what *humans do* and its 'scope and power' is a scope and power to do things. As was argued in chapter 3, when humans use language they always do something, even when they fail to do what they intend. As studying something 'for its own sake', often presupposes the 'it' is something separable from human activity, something studied 'for its own sake' will tend to only nominally recognise this human-orientated motivation for studying language. If one is to take this motivation seriously, investigation of language should not only study language per se ('not if a language is anything like what many philosophers and linguists have supposed'), but be open to the study of communication and be willing to expand its theoretical purview, something the representatives of Formalist Attitude have consistently refused to do.

It is not the purpose of this thesis to describe communication and what the study of communication is and as there are many controversies about the role and nature of communication, such engagement would not be possible here[112]. Therefore I limit myself to a few indicative remarks about what a study of communication, rather than of language *per se*, might entail. What is clear is that communication is not synonymous with *linguistic* communication (Sperber and Wilson 1986, Fiske 1990, Harris 1996) and communicational systems interact and rely upon each other in order to make communication successful. A wink may suggest that I am being sarcastic when I 'compliment' a piece of pop-art, someone's personal history may make it clear to those who know them that their yes does not mean 'yes' and their no does not mean 'no'. Even if one supposes (as Saussure did) that linguistic systems are primary vis-à-vis other communication systems, it does not follow that language is neatly independent of other communication systems in meaning-making or that the study of language can or should be conducted as an autonomous discipline. Language is important to humans because it is a communicative system, a system which is utilised by agents in making themselves understood and if this is a reason for studying language, then it is also a reason to not separate the study of language from the

[111] This is evident across a range of linguistic study. See the opening pages of Chomsky (1966), Lord (1974), Holmes (2001) and Evans and Green (2007).

[112] As Fiske has claimed, communication is something that 'few can define satisfactorily. Communication is talking to one another, it is television, it is spreading information, it is our hair style, it is our literary criticism: it is endless' (Fiske, 1990, p.1).

study of communication or be hostile to changes in the theoretical purview of linguistics. Language does not have a life of its own and should not be studied for its own sake.

My position agrees in a number of ways with proposals forwarded by Jones (2003) concerning a realist approach to linguistics. For example, Jones suggests that a 'realist linguistics, it seems to me, would place language squarely within the domain of social phenomena and therefore amenable only to social scientific explanation (Jones in Cruickshank (ed.) 2003, p.103). This thesis has argued that linguistics should be considered a social science and has attempted to vindicate social scientific explanation from the skepticism of some linguists. Jones also argues that the natural processes 'of body and brain on which it [language] undoubtedly depends are not innately specified but are themselves *formed* in the development and exercise of the social practice of linguistic interaction itself' (Jones in Cruickshank (ed.) 2003, p.104). While I am happy with the idea there are innate properties humans posses which allow them to use language, my proposal of Actual Speech and support for the transformational model puts the linguistic act at the centre of linguistic enquiry and so is in keeping with the tenor of Jones' point. Finally, Jones attacks the 'fallaciously motivated disjunctions and dualisms of form and meaning, and of ('external') communication and ('internal') language' (Jones in Cruickshank (ed.) 2003, p.104). My position coincides with that of Jones' here too, as both in criticising the internalist view of knowledge of language and the problematically restrictive theoretical purviews of Chomsky and Saussure, I reject any dichotomy between external and internal linguistics. In view of these agreements then, my view of linguistics could be described as a realist one.

To give some sense of where I think my conclusions point to, I will look at two approaches to linguistics that share some of my concerns and are in respects consistent with and supportive of some of my arguments. Before this though, I look at some remaining issues. Despite problems with the Formalist Attitude and the insistence on a clearly delimited object of knowledge, there are intuitions and concerns that make the apparent security of boundaries offered by the Formalist Attitude attractive. I look at some of these in order to defuse concerns and clarify my position.

4.2.2 The universality and independence of language

As has been frequently noted, language is something without which other social structures could not exist. Searle has claimed, 'in order to have institutional facts at all, a society must

have at least a primitive form of language…the institution of language is logically prior to other institutions' (Searle 1995, p.60). This is due to the symbolic nature of social structures, which are brought about by acts of agents, typically partly linguistic, that give objects or actions significance beyond their 'brute' reality: filling in a housing benefit form, making a statement under oath, appointing someone to committee chair or buying a fish supper presuppose or directly involve language, often both[113]. Language is not only deemed necessary for the existence of other social structures, but throughout rationalist and Enlightenment thought language possession has been cited as the distinctive trait which defines humanity. Chomsky: 'one fundamental contribution of what we have been calling 'Cartesian linguistics' is the observation that human language, in its normal use, is free from the control of independently identifiable external stimuli or internal states…in contrast, for example, to the pseudo language of animals' (Chomsky 1966, p.29)[114]. Language is not only seen as inexorably intertwined with our human existence, but as partly constitutive of that human existence.

In chapter 1, I pointed out that one reason linguistics was considered a natural science was that its object of knowledge, language, appeared to be natural in being immutable and universal among humans, as is the swim-bladder for fish or skeletal pneumaticity among birds. Another linked reason, and one evident in discussion of the Durkheimian model and the Formalist Attitude generally, is that 'those who want to count linguistics among the natural sciences can refer to the fact that language evolves independently of human will' (Keller 1994, p.61). As language appears out of the control of speakers, it is plausible that it has speaker-independent laws in a similar way to physical or biological laws. One aim of this thesis has been to question the idea that language is 'natural' in the sense of being universal and independent of speakers. My position that language is a power to use a *particular* language rather than language as I-language or 'language in general', and my arguments about ways in which languages are motivated and change, suggest that language is neither natural or independent in a way that would associate it with the objects of natural scientific enquiry.

There are also other reasons to think that language is not a natural object. While 'society' is the non-natural object *par excellence*, it displays similarities with language that put into question the naturalness and independence of language. Like language, society appears

[113] Austin often notes this distinction; 'to congratulate is necessarily to say certain words…to make certain more or less indescribable movements with the vocal organs' (Austin 1975, p.114).

[114] For example see also Della-Mirandola's *Oration on the Dignity of Man* (1996 [1486]) and Descartes' *Meditations* (1968 [1637]).

to be immutable and universal, for where we find language we also find societies and vice versa. Like language, society varies in organisation and presupposes the existence of people. Like language, society relies upon the constant activity of individuals to reproduce and potentially change it (4.2). Despite these similarities, there are few claims for society to be treated as a natural object and therefore studied as a natural science, so one has good reason to doubt that language is natural. While one may argue social phenomena are reducible to physical phenomena and so social science reducible to natural science, this is a different claim. For a reductionist view of the social sciences wishes to *eliminate* those sciences, leaving their objects of knowledge as epiphenomena of a general theory of physics (Fodor 1981), whereas seeing language as natural defends its claim to be an autonomous science, which as Saussure put it, 'has a place ready for it in advance' (Saussure 2006 [1922], p.30). Given that society and language both appear similarly 'natural', those wishing to see language as an object of natural scientific study first have to explain why, against appearances, society should not also be also be deemed natural. One may of course bite the bullet and claim that the distinction is not useful, though this would require an account of how the study of society sits alongside that of the natural sciences and as this would effectively collapse the distinction between natural and non-natural, it is not clear what would be implied by the claim that language is natural.

What we have seen in chapter 1 concerning the theoretical purview of linguistics, in chapter 2 in discussion of social causality and in chapter 3 concerning knowledge of language and the integration between the social and the linguistic, is that the nature of language and a theoretical purview responsive to that nature is not closable and delimitable in the way that the Formalist Attitude has maintained. Again this suggests we drop the claim that language is a 'natural object' with the accompanying claim that it be studied as one of the natural sciences. Instead and as I argued in 2.3, language should be classified as a social object as it relies upon the existence of human beings. What this means for linguistics is that there can be no once-and-for-all closure of its boundaries and that attempts to do so are misguided and likely to fail. As has been pointed out, linguistics does not come furnished with 'clear and unobjectionable data. Such is our [linguists'] fate, in common with psychologists, economists, paleontologists, and a host of others' (Halle and Higginbotham 1986, p.292). Like the social sciences, linguistics does not have a field of enquiry it can mark out in advance or is not open to change. One might consider this to be nothing new, for one cannot rule out changes in the field of enquiry and in what is accepted as data in any of the sciences. Even the most predictively successful sciences and

those that achieve a degree of causal closure in experimental contexts cannot secure their theoretical boundaries against future developments. This is true, but with the social sciences change happens in two ways, making boundary changes more likely. In common with physics and cosmology, discoveries and controversies in the social sciences may lead to changes in theoretical purview. So there is a clear sense in which the (arguable) discovery of the unconscious in psychology and the discovery of the Planck constant in quantum mechanics are analogous.

There is a difference in the social sciences though in that they can change as a result of human *invention*[115]. The advent of globalisation has made it difficult for specialists in national or regional economies to explain the state of those economies by reference to activity within regional/national economies, whereas this would have been more plausible at the beginning of the industrial revolution. In a globalised world a grasp of the global economy and global economics is needed to understand individual economies, as the expansion of free markets and the economic interdependence that comes with it has shifted the theoretical purview of the study of national economies. If we consider linguistics, we can consider the case of writing; an invention that has influenced language in helping to formalise pronunciation, facilitated the maintenance of larger lexicons and given languages a degree of access to their linguistic past beyond that of a non-literate society.

However, a generativist might respond in this way to defend the boundaries of linguistics: 'Granted that language is in some respects social and that the purview of language study cannot be comprehensively closed due to the fact that linguistic activity is social activity. However, possession of language presupposes mental structures and whatever the social nature of language, language can holistically only be understood by focussed study on its various properties. Therefore, investigation of these mental structures and/or grammatical rules that underpin linguistic activity is a valid way to approach and understand language.' Such a defence is of course not only open to the generativist, but potentially any position wishing to restrict its domain. The first thing to point out against the interlocutor is that in many of the positions considered throughout this thesis, there has been a failure to explain phenomena a position seeks to explain given the theoretical confinement imposed upon it. As was seen with Chomsky in 1.3, Lass in 2.1 and internalism in 3.2, insistence on maintaining a highly abstract approach led to problems in

[115] I mean this term broadly to include such things as globalisation and chivalry. While globalisation is not an invention in the sense that the cathode ray oscilloscope was an invention, considerable planning and political pressure has been brought to bear to expand and sustain globalisation and the neo-liberal economic philosophy underpinning it.

providing answers to questions within the scope of enquiry. With this in mind, one can respond to the interlocutor by accepting that things can possibly be fruitfully studied piecemeal, but with the caveat that this cannot offer a defence against any *particular* abstractions, which are judged (at least) on their own criteria for success. Indeed, what could speak more against the viability of a theoretical purview than failing on its own terms?

The second point is related specifically to the claims of the interlocutor as a generativist. A problem can be seen if we apply the generativist's argument to sociology; if we are to study language (which has at least a social element) in terms of brain structures, then why not society? Having society as well as language is something that separates humans from rocks and clouds, so is plausibly as applicable to Chomsky's transcendental argument that motivates the study of brain structures in investigating language. By these lights we could have a 'generativist sociology' that would take as its object the human mind, specifically the cognitive module(s) that makes society possible. One might postulate, analogously to the LAD, a social acquisition device (SAD) and analogously to a universal grammar, a universal society (US), the steady state of which forms into an individual's I-society. The US would be a cognitive structure to which social formations could be reduced in terms of their counterfactual dependence on the structure and the similarity of all 'surface social relations' to a few 'deep social relations'.

This is obviously not representative of sociological investigation and there are plausible reasons why. One reason is that societies often have features (a system of voting or a caste system) that while shared with other societies, are difficult to explain by reference to structures that all societies share. Indeed, the UK and New Zealand are both democracies, but one would not know where to start in explaining by means of enquiry into brain structures why the UK has a first-past-the-post system and New Zealand an idiosyncratic blend of proportional representation and the alternative vote. While angels or Martians might see any differences between human societies as superficial and deem them effectively identical[116], their differences matter a great deal to the human beings in those societies and an explanation of their features and differences cannot be satisfied by pointing to putative brain structures that might be common to all humans. If they could be explained by a generativist sociology then history, economics and sociology would be, if not otiose, then much diminished in explanatory power and perhaps not worth pursuing.

For these reasons, such a defence of theoretical delimitations fails. However, the

[116] See Chomsky (in Martinich (ed.) 2008, p.687).

conclusion that linguistics should have no 'hard and fast frontiers' needs to be clarified for what it means for the practice of linguistics. As well as this, there is the worry that the removal of the requirement that a discipline have hard and fast frontiers could, by means of a slippery slope argument, lead to a destructive theoretical free-for-all.

4.2.3 Abstraction and avoiding a 'Theory of Everything'

In chapter 2, I defused a worry held by Chomsky, Lass and others that to allow considerations such as Actual Speech, communication and what Lass called 'messy' speakers within the purview of linguistics would be to undermine the scientific legitimacy of the subject. I showed how the typically too abstract understandings of language of the Formalist Attitude are associated with a deductivist position that, as we saw with Lass and Chomsky, is unworkable in their respective theories of language, conceptual acquisition (1.3) and language change (2.1). As has been argued by philosophers and linguists on several occasions (Itkonen 1983, Pateman 1987, Collier 1994), a D-N account of language phenomena only looks plausible under the auspices of a strongly idealised and delimited domain of enquiry typical of the Formalist Attitude. I showed how considerations of non-deductive accounts of causality are more robust and intellectually respectable than some of their opponents believe (2.2) and how suspicion of such phenomena is unwarranted.

There is a legitimate worry though in the form of a slippery slope argument concerning disciplinary boundaries. If one allows that certain phenomena (speakers, the diachronic, context etc.), to be included within the purview of a discipline because such phenomena can be seen to have an influence on phenomena within the purview of that discipline, then there is no obvious point at which one can draw a line between what concerns and what does not concern a discipline. If one accepts the plausible idea that everything bears *some* causal relation to everything else, then one might argue there is no justifiable delimitation of a field of enquiry, resulting in what Chomsky has referred to as a 'theory of everything' (Chomsky 2000, Rescher 2006). But what is problematic about a theory of everything (hereafter ToE)? In the context of linguistics, Chomsky argues the following:

> The Study of Communication in the actual world of experience is the study of the interpreter, but this is not a topic for empirical enquiry, for the usual reasons; there is no such thing as a theory of everything….[communication] is far too complex and obscure to merit attention (Chomsky 2000, pp.69-70).

A problem is that a ToE (here a theory of communication) would be so complex as to be ungraspable by a single human mind. While we might aim for a ToE, we would by dint of practical necessity have a series of specialisms and specialists, narrowing their theoretical purview due to epistemological limits (though they may narrow it for other reasons too). Therefore, one problem with a ToE is that it is doomed to failure from the outset due to human limitations and this is why Chomsky thinks 'there is no such thing'. Even within the context of highly specified enquiries, (cosmology, fluid dynamics) no individual specialist understands the entire scope of their field of enquiry and this makes clear the ineluctable necessity for degrees of specialisation, which implies *some* level of abstraction within study.

If being practically impossible were not problem enough, another problem is that a ToE would tend towards triviality. A ToE is motivated by the idea that every phenomenon is in a causal chain with every other and this motivates the abandonment of theoretical boundaries. The worry is that there is difficulty in offering illuminating causal explanations of events and properties because the answer to 'why is X the case' where 'X' is anything whatsoever, is 'X is the case because of everything'. An example: while one might perceive a link between the economic history of the United Kingdom and the chemical properties of coal (consider the importance of the combustible qualities of coal to the industrial revolution), it is difficult to see how the study of chemistry could impinge on the study of economics because of this fact. A ToE implies they are related and as such, an account of the industrial revolution should include an account of the combustible qualities of coal, the gravitational pull of the earth, Dickens' favourite hat and so on. Its findings, being undifferentiated in terms of importance, would be trivial.

For these reasons I share Chomsky's suspicion of a ToE. However, what is important in terms of the current discussion is what a rejection of a ToE can do to defend *any particular purview*, for I argue it can do little. The slippery slope worry cannot be a sufficient or necessary part of a defence of a particular purview as it applies equally to Newtonian physics and pre-atomic chemistry as it does to the generativist, structuralist, functional, integrationist or cognitive linguist. The worry applies to *any* defence of a theoretical purview and so is not useful in defending any specific one and Chomsky is mistaken in using it to defend his enquiries. Of course, rejection of a ToE implies that enquiries must be delimited in some degree, but it does nothing to justify any particular delimitation or abstraction. Given the fact a ToE is practical impossibility anyway, it cannot pose any threat.

It is incorrect then that expanding a theoretical purview or leaving it expressly open

puts one on a slippery slope leading to a ToE. What we saw with Saussure's stance on writing, Chomsky on 'external' considerations or Lass on 'messy' speakers, was a worry that purview expansion leads to vagueness or a confusion about what is 'natural' to language. However, if we consider that the scope of studies have changed (compare Newton's and Einstein's physics or the early nineteenth century view that linguistics was a sub-branch of philology), it is clear that this has not led to profligate theoretical expansion. So from an historical perspective too, such worries appear unfounded. Indeed, against those who worry that theoretical expansion leads to a ToE, the boot may be on the other foot. As Fodor points out, if one adopts a prescriptive approach to disciplinary boundaries, 'he might just as well attend to the construction of grammars that predict only intuitions about sentences with more than seven vowels, or sentences whose twelfth word is 'grandmother'….Once you start to stipulate, it's Liberty Hall' (Fodor in Katz (ed.) 1985, p.158). Not only is stipulation not a prophylactic against changes to disciplinary boundaries, stipulation can licence precisely what those who worry about change or expansion in boundaries wish to avoid. What is required to defend theoretical boundaries is an assessment of particular boundaries.

The ToE worry is defused, but there is still a legitimate question about the justification of purview setting *simpliciter*. I advocate that linguistics should be expressly open in respect of its theoretical boundaries and this may appear to support methodological anarchism as espoused in Feyerabend's *Against Method* (1982). Therefore I want to offer some criticism of methodological anarchism and then give an outline of how theoretical purviews can be justified. Feyerabend's position is well known, so only a brief outline is required. In *Against Method*, Feyerabend argues against methodological monism, 'the idea that science can, and should, be run according to fixed and universal rules' (Feyerabend 1982, p.295). According to Feyerabend, methodological monism fails to account for the character of scientific advancement and if adopted, would lead to scientific stagnation. The proposed remedy is the adoption of 'methodological anarchism', a position suspicious of any delimitation of disciplinary boundaries, summed up in this way: 'There is only *one* principle that can be defended under *all* circumstances and in *all* stages of human development. It is the principle: *anything goes*' (Feyerabend 1982, p.28).

It might appear that disavowal of any delimitation would lead methodological anarchism toward a ToE which, as I have argued, is a practical impossibility. But this need not faze the methodological anarchist, who could respond in this way: 'Granted some delimitation is inevitable, but what I am interested in is that delimitations should not get in

the way of scientific advancement.' This is persuasive and similar to the position I advocate. What is different from my position though is that the methodological anarchist is blind to the necessity of a scientific tradition and provides no way of judging when a 'scientific advancement' might have taken place. As Bhaskar has argued, 'any anarchistic move which helps progress on one criterion will *impede* it on some other' (Bhaskar 1989, p.34). In the context of scientific enquiry, a question about an object O requires that it be defined in such a way as one can be fairly sure that one is answering a question about O, even if O turns out not to be what was thought. For example, phenomena ascribed to 'phlogiston' were in fact explained by magnetic fields and the Higgs boson might turn out to be within a lower gigalectron volt range than previously thought. This presupposes not only recognition of fallibility, but a tradition of seeing an object in a particular way and trying to answer questions about it. In being blind to tradition and providing no criterion for scientific progress, the claim of methodological anarchism to defend such progress is meaningless.

I have dealt with the issues of a ToE and methodological anarchism and it is now appropriate to ask that if linguistics should have no principled delimitation or 'hard and fast frontiers', when would it be appropriate to change theoretical purview? I do not think it possible to give criteria that would cover all possible boundary disputes, as the world is such that we are sometimes compelled to ask new questions that require new approaches. However, I propose several rules of thumb which I think useful. These rules of thumb are meant to be modest, both because any broader investigation is not possible given the scope of this thesis and as scientific practice and methods change, it is not clear that anything firmer than rules of thumb are possible. I propose three rules of thumb for justifying change in purview:

> *Current domain problems.* If theoretical problems are present within a domain or discoveries occur which cause problems, then there is reason to change the theoretical boundaries of the domain. As Ishmael notes in *Moby Dick* (Melville 1992 [1851] pp.145-157), the inclusion of cetaceans with the study of ichthyology is a problem for the study of fish because cetaceans display mammalian characteristics. Either one expands the study of fish to include cetaceans (which by-the-by Ishmael recommends) or cetaceans are excluded from ichthyology, the current orthodoxy. The point is not that in taking cetaceans out of ichthyology we have a more truthful zoological taxonomy, but that the example shows how such issues can motivate change.

New domain solutions. If expanding the current domain to establish a new domain which includes phenomena which allow for the explanation and/or prediction of events which the current domain could not explain or predict, then there is justification for the new domain. For example, inclusion of the unconscious and suppressed experiences within a theory of psychology may be justified if it helps explain the action of agents, the principal goal of psychology.

Comparison of new domain and current domain problems. If a new domain contains fewer problems than in a current domain then there is motivation to adopt the new domain over the current domain. This may appear synonymous with (i), though what is stressed here is that the new domain need not be problem-free and there may be questions that still require answers (consider the standard model *versus* steady-state theory). However, the new domain can still prove the more fruitful than its predecessor. This is analogous to the principle of inference to the best explanation also discussed in 3.2, though here the concern is with the wider issue of theoretical domains.

If there are problems with the current domain but no solutions from a proposed new domain, then we still have reason to remain with the current domain because the new domain solves nothing. However if there are problems with the current domain and fewer problems with the new domain then we would have reason to adopt the new domain, though our purview would not necessarily offer any immediate solutions to problems. Of course, deciding what counts as a domain-problem is itself theory-relative and thus a more complex matter than I present it here (Kuhn 1970), though further engagement is beyond the scope of this thesis. I have now considered worries about my argument that linguistics has more in common with the social than the natural sciences and that it should reject any requirement to have 'hard and fast frontiers'. Finally, I look at two current movements in linguistics that overlap in some concerns and conclusions of this thesis and for which this thesis can offer a degree of philosophical underlabouring.

4.2.4 Conclusion: Cognitive and integrationist approaches

Within contemporary study of language there are two relatively new approaches that share my concern that linguistics has tended toward fruitless abstraction and has not taken

sufficient account of the social, contextual and agent-orientated nature of language. These are cognitive and integrationist linguistics. By historically and philosophically contextualising and criticising the Formalist Attitude in some of its manifestations, this thesis can be seen as a contribution to debates about the scientific status of linguistics that the concerns of such approaches raise. I wish to give an outline of each approach not in order to provide critical comment as to their respective fruitfulness and viability, but to point towards some shared emphases and agreements and show how this thesis may plausibly provide some underlabouring for them.

First to cognitive linguistics, whose foundations were set out in the work of linguist George Lakoff and philosopher Mark Johnson, particularly in their *Metaphors we live by* (1981), *Philosophy in the Flesh* (1999) and Johnson's *The Body in the Mind* (1990). In many respects, cognitive linguists is consistent with the tradition of orthodox linguistics in that it claims to study language for its own sake, focussing on investigating and expositing the systematicity and structure of language systems (Evans and Green 2007, p.5). Its intellectual history has grown out of differences with generativist linguistics, which evolved into a number of commitments that inform and underpin its theoretical endeavours. These are known as the 'Generalisation commitment' and the 'Cognitive commitment'. In contrast to generativist support for the modular nature of mind and the postulation of specific language modules (syntax, conceptual, phonological), the Generalisation commitment implies a rejection of the existence of such modules. Rather than seeing individual modules as possessing individual functions (to categorise, conceptualise, parse), cognitive linguists see such functions or abilities as possessed by the cognitive system in general. So for example, what makes up the 'language system' or the linguistic ability of the speaker is, according to cognitive linguists, based on fundamental organising principles common to the cognitive system globally. As Lakoff sees it, the generalisation commitment 'is a commitment to linguistics as a scientific endeavour, a commitment to seek general principles' (Lakoff 1990, p.46).

The cognitive commitment follows from the generalisation commitment and 'represents the view that principles of linguistic structure that hold should reflect what is known about human cognition from other disciplines, particularly the other cognitive sciences' (Evans and Green 2007, p.40). Given the generalisation commitment posits that cognitive capacities are general and non-modular in nature, converging cognitive linguistic research with research from other fields is a plausible way of seeking to empirically justify the generalisation commitment. In a statement of the intent of cognitive linguistics which

implicitly recognises both commitments, Fauconnier writes,

> In contrast to this sharply autonomous view of language structure, cognitive linguistics has resurrected an older tradition. In that tradition, language is in the service of constructing and communicating meaning, and it is for the linguist and cognitive scientist a window into the mind. Seeing through that window, however, is not obvious. Deep features of our thinking, cognitive processes, and social communication need to be brought in, correlated, and associated with their linguistic manifestations (Fauconnier in Janssen and Redeker (eds.) 1999, p.95).

Cognitive linguistics shares with this thesis a concern that the desire for linguistics to be an autonomous discipline has led to problematic abstractions and as such cognitive linguistics is less focussed on maintaining strict disciplinary boundaries, as the cognitive commitment implies. Cognitive linguistics is committed to working and bifurcating with the cognitive and social sciences. Indeed in discussing work on linguistic motivation (Radden and Panther 2004) and language change (Croft 2000), this thesis has touched on cognitive linguistic work, so it is clear that there are areas of agreement and that my analysis and criticism of the Formalist Attitude supports the broader scope of cognitive linguistics and the greater attention given to linguistic utterances and the role of communication in social life.

Another approach that shares concerns and emphases with this thesis, though one quite different from cognitive linguistics, is integrationism, developed by Roy Harris in *The Language Myth* (1981), *The Language Machine* (1987) and *Signs, Language and Communication* (1996). Integrationist linguistics involves the 'investigation of the renewal of language as a continually creative process' recognising that 'Human beings inhabit a communicational space which is not neatly compartmentalised into language and non-language' (Harris 1981, pp.164-165). Harris has offered these axioms of the integrationist approach:

1. What constitutes a sign is not given independently of the situation in which it occurs or of its material manifestation in that situation.
2. The value of a sign (i.e. its signification) is a function of the integrational proficiency which its identification and interpretation presuppose (Harris 2009, p.70).

The term 'integrational proficiency' is anything that influences the communicative process, be this context, mood or different kinds of knowledge possessed by speakers. This means that 'communication events *cannot be decontextualised*. Episodes of communication are episodes in the lives of particular people at particular times and places. Signs are products of such episodes' (Harris 2009, p.70). Central to integrationism is the 'principle of

cotemporality'. The principle of cotemporality stresses that 'linguistic acts are assumed to be immediately relevant to the current situation, unless there is reason to suppose otherwise' (Harris 1981, p.157) and linguistic and non-linguistic acts are continually integrated with one another, having equal importance in lived experience and in interpreting action. Without recognition of this principle, making sense of linguistic activity (making a political speech, writing a mountaineering guidebook) is difficult because there is no fruitful separation between the linguistic and non-linguistic[117], so the 'linguistic' cannot be understood without an account of how it is integrated with other facets of human life (personal/national history, gesture, ethics). Integrationism can be plausibly seen as an attempt at a 'study of the interpreter' that Chomsky has been keen to dismiss (4.3.3) and integrationist excursions into subjects such as psychoanalysis (Harris 2009) and economics (Jones 2011) indicate the heterogeneity of the approach.

Integrationism is suspicious of attempts to study language within the framework of either the social or natural sciences and integrationists sometimes deny that study of language should aim at 'scientificity' (Harris 2009, Jones 2007) or at discovery and exposition of the systematicity and structure of language systems. As Harris has described the situation for Saussure (though this might be applied to modern linguistics also):

> [T]he study of language threatened to fragment between disciplines which had little in common: phonetics, psychology, philology, neuropsychology, social anthropology, etc. The fragmentation pursued in the interests of science and its incessant quest for 'harder' facts, left a disturbing impression that somehow language has slipped though the net of understanding (Harris 1988, pp.126-127).

Integrationism shares concerns with this thesis and is in certain respects consistent with it. The emphasis on the linguistic activity of agents and their ability to transform language is in keeping with my proposal of the transformational model (4.2) and recognition in the principle of cotemporality that language functions in an open system where there is no easy separation of the 'linguistic' and 'non-linguistic' is consistent with my account of linguistic knowledge in 3.3. Perhaps more so than cognitive linguistics, integrationism is aware of linguistic history and often critical of the scientific aspirations of linguistics and the philosophical background of linguistic theories. In this respect in particular, this thesis can provide a degree of philosophical support and underlabouring.

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[117] This stance has something in common with Davidson's criticisms and stance in 'A Derangement of Epitaphs' and Goldstein (2004) notes links between integrationism and Davidson's 'Wittgenstein turn'.

The primary purpose of this thesis has been to argue against claims that linguistics is a natural science or is especially affiliated with the natural sciences: a position I have called the Formalist Attitude. Chapter one provided historical background by tracing the development of linguistics in the nineteenth and early twentieth centuries in terms of conceptions of language and the practice of linguistics. I then showed how the Formalist Attitude is present and problematic in modern linguistics in the form of generativism and less conspicuously, but still problematically, in Derrida's consideration of linguistics.

In chapter two I turned to problems of considering social factors in linguistics and the suspicion linguists have of causation and explanation of social phenomena. I looked at Lass' *On Explaining Language Change* and argued that despite an awareness of the problems of a highly formalised conception of linguistics, he nevertheless remained unjustifiably sceptical about any non-deductive account of language change which included reference to social factors. I then assessed the viability of *ceteris paribus* laws as a way of accounting for causation and explanation in the social sciences and argued that while *ceteris paribus* laws are more robust than some critics claim, they are vulnerable to the charge of vacuity. In response to this I argued that *ceteris paribus* laws were best treated as power ascriptions and I gave an account of how language can be usefully and justifiably viewed as a power, which provided an answer to the question, 'what is a language?'.

Chapter 3 considered a number of issues in linguistics and philosophy of language congruent with the overall concerns of the thesis. These were arbitrariness, language change and knowledge of language. In each case, it was shown that approaches to these issues which saw language as an autonomous object encountered problems and that consideration of social or external factors shed light on the issues and helped to solve problems.

In chapter 4 I began by inquiring into the relationship between the speaker and language structure. I argued for a transformational model that avoided the problems of the Durkheimian and Weberian models and recognised the mutual irreducibility and interconnectedness of structure and agency. Lastly, I brought together observations of this thesis and concluded that linguistics, in opposition to the Formalist Attitude, should be viewed as a social rather than a natural science and should be less concerned to secure a strict delimitation of its theoretical purview and object of knowledge.

Thought about language over the last fifty years has to some degree recognised the social importance of language and the importance of the social to language. Such recognition, while welcome, does not automatically translate into our attempts to describe,

explain and theorise about language. For such recognition to be fruitful, its consequences for the way in which language is described, explained and theorised about need to be scrutinised. This thesis has made a contribution by showing how interconnected the theoretical assumptions of linguists are with their view of language and also the extent to which factors outside the linguist's chosen theoretical domain are too often problematically refused, ignored or sidelined. In doing this and offering some remedy to such problems, this thesis can help to inform future work in the philosophy of linguistics and linguistics itself.

Bibliography

Aitchison, J. *Language Change: Progress or Decay? (3rd Edition)*. Cambridge: Cambridge University Press, 2001.

Aldridge, H. Parekh, A. MacInnes, T. Kenway, P. *Monitoring poverty and social exclusion*. Joseph Rowntree Foundation, 2011.

Aristotle. *De Anima*. Tancred-Lawson, H. (Trans.) London: Penguin, 1987.
Metaphysics. Tancred-Lawson, H. (Trans.) London: Penguin, 1998.

Austin, J.L. *How to do things with Words*. Sbisa, M and Urmson, J.O. (eds). Cambridge, Mass: Harvard University Press, 1975.

Bacon, J., Campbell, K. and Reinhardt, L. *Ontology, causality and mind. Essays in honour of D. M. Armstrong*. Cambridge: Cambridge University Press, 1993.

Baker, G. P. and Hacker, P. M. S. *Language, Sense and Nonsense: A Critical Investigation into Modern Theories of Language*. Oxford: Basil Blackwell, 1984.

Bar-On, D and Risjord, M. 'Is There Such a Thing as a Language?' *Canadian Journal of Philosophy* 22, 2 (June 1992), 163-90.

Bartelborth, T. 'Coherence and Explanations.' *Erkenntnis* 50, 2/3 (1999), 209-224.

Barthes, R. *Elements of Semiology*. Lavers, A. and Smith, C. (Trans.) New York: Hill and Wang, 1967.

Benczes, R. 'Book Review of 'Studies in Linguistic Motivation'.' *Cognitive Linguistics* 18, 1 (2007), 91-100.

Bhaskar, R. *The Possibility of Naturalism*. Brighton: Harvester, 1979.
Reclaiming Reality: Critical Introduction to Contemporary Philosophy. London: Verso, 1989.
A Realist Theory of Science. London: Verso, 2008.

Bird, A. *Philosophy of Science*. London: McGill-Queen's University Press, 1998.

Blake, R. and Josey, M. 'The /ay/ diphthong in Martha's Vineyard Community: What can we say after 40 years of Labov?' *Language in Society* 32, 4 (2003) 451-485.

BonJour, L. *The Structure of Empirical Knowledge*. Harvard University Press, 1988.

Braybrooke, D. *Philosophy of Social Science*. Prentice Hall: Englewood Cliffs, 1987.

Cappelen, H. and Lepore, E. *Insensitive Semantics*. Oxford: Blackwell, 2005.

Cartwright, N. *How the Laws of Physics Lie*. Oxford: Clarendon, 1983.

'In Favor of Laws That Are Not Ceteris Paribus after All.' *Erkenntnis* 57, 3, (2002), 425-439.

Chapman, S. and Routledge, C. (eds.) *Key Thinkers in Linguistics and the Philosophy of Language*. Oxford: Oxford University Press, 2005.

Chomsky, N. *Aspects of a Theory of Syntax*. Cambridge, Mass: MIT Press, 1965.

Cartesian Linguistics: A Chapter in the History of Rationalist Thought. New York: Harper and Row, 1966.

Problems of Knowledge and Freedom: The Russell Lectures. New York: Random House, 1971.

Reflections on Language. New York: Pantheon, 1975.

'Knowledge of Language: Its Elements and Origins.' *Philosophical Transactions of the Royal Society of London. Series B, Biological Sciences* 295, 1077 (1981), 223-234.

Knowledge of Language. New York: Praeger, 1986.

Language and the Problems of Knowledge. Cambridge, Mass: MIT Press, 1988.

The Minimalist Program. Cambridge, Mass: MIT Press, 1995.

New Horizons in the Study of Language and Mind. Cambridge: Cambridge University Press, 2000.

On Nature and Language. Cambridge: Cambridge University Press, 2002.

Collier, A. *Critical Realism: an Introduction to Roy Bhaskar's Philosophy*. London: Verso, 1994.

Crane, T. *Dispositions: A Debate*. London: Routledge, 1996.

Croft, W. *Explaining Language Change: An Evolutionary Approach*. London: Longman, 2000.

Cruickshank, J. (ed.) *Critical Realism: The Difference it Makes*. London: Routledge, 2003.

Crystal, D. *Language and the Internet*. Cambridge: Cambridge University Press, 2006.

Danto, A. C. *Analytical Philosophy of History*. Cambridge: Cambridge University Press, 1965.

Darwin, C. *The Origin of Species*. London: Wordsworth, 1998 [1859].

Davidson, D. 'Replies to My Critics.' Crítica: Revista Hispanoamericana de Filosofía, 30, 90 (1998), 89-112.
Enquiries into Truth and Interpretation. Oxford: Oxford University Press, 2001.
The Essential Davidson. Oxford: Oxford University Press, 2006.

Della-Mirandola, P. G. Oration on the Dignity of Man. Caponigri, A. R. (Trans.), 1996 [1486]

Derrida, J. *Of Grammatology*. Spivak, C.G. (Trans.) London: Johns Hopkins, 1997.
Positions. Bass, A. (Trans.) London: Continuum, 2002.
Writing and Difference. Bass, A. (Trans.) London: Routledge, 2006.

Descartes, R. *Discourse on Method and the Meditations*. Sutcliffe, F.E. (Trans.) London: Penguin, 1968 [1637].

Dummet, M. *The Seas of Language*. Oxford: Clarendon, 1993.

Durkheim, E. *The Rules of Sociological Methods*. Walls, W.D. (Trans.) London: The Free Press, 1982 [1895].

Earman, J. Roberts, J. and Smith, S. 'Ceteris Paribus Lost.' *Erkenntnis* 57, 3 (2002), 281-30.

Eddington, A. *The Philosophy of Physical Science*. Ann Arbor: University of Michigan Press, 1958.

Evans, V. and Green, M. *Cognitive Linguistics: An Introduction*. Edinburgh: Edinburgh University Press, 2007.

Feyerabend, P. *Against Method*. London: Verso, 1982.

Fodor, J. A. *Representations: Philosophical Essays on the foundations of Cognitive Science*. Brighton: Harvester, 1981.
'You Can Fool Some of The People All of The Time, Everything Else Being Equal; Hedged Laws and Psychological Explanations.' *Mind* 100, 397 (1991) 19-34.

Goldstein, L. 'Philosophical integrations.' *Language Sciences* 26 (2004), 545-563.

Gumperz, J. and Levinson, S. (eds.) *Rethinking Linguistic Relativity*. Cambridge: Cambridge University Press, 1996.

Haack, R. J. 'Davidson on Learnable Languages' *Mind* 87, 436 (Apr 1978), 230-249.

Haiman, J. 'The Iconicity of Grammar: Isomorphism and Motivation.' *Language* 56, 3 (1980), 515-540.

Halle, M and Higginbotham, J. 'Scientific Linguistics.' *Natural Language and Linguistic Theory* 4, 2 (1986), 291-294.

Halliday, M.A.K. *Language as social semiotic: The social interpretation of language and meaning*. London: Edward Arnold, 1984.
On Grammar. London: Continuum, 2002.

Harman, G. 'The Inference to the Best Explanation.' *Philosophical Review* 74, 1 (1965) 88-95.
(ed.) *On Noam Chomsky: Critical Essays*. New York: Anchor Books, 1974.

Harris, R. *The Language Myth*. London: Duckworth, 1981.
The Language Machine. London: Duckworth, 1987.
Language, Saussure and Wittgenstein. London: Routledge, 1988.
Signs, Language and Communication. London: Routledge, 1996.
Saussure and his Interpreters. Edinburgh: Edinburgh University Press, 2003.
Integrationist Notes and Papers 2006-2008. Bright Pen, 2009.

Hockett, C. F. *A Course in Modern Linguistics.* New York: Macmillan, 1958.

Holdcroft, D. *Saussure: Signs, System, and Arbitrariness.* Cambridge: Cambridge University Press, 1991.

Holmes, J. *An Introduction to Sociolinguistics (Second Edition).* London: Longman, 2001.

Holt, D. E. (ed.) *Optimality Theory and Language Change.* London: Kluwer Academic, 2003

Hume, D. *A Treatise of Human Nature.* London: Penguin, 1969 [1739/40].
Enquiries concerning Human Understanding and concerning the Principles of Morals. Oxford: Clarendon, 1975 [1777].

Itkonen, E. *Grammatical Theory and Metascience: A critical investigation into the methodological and philosophical foundations of 'autonomous' linguistics.* Amsterdam: John Benjamins, 1978.
Causality and Linguistic Theory. Bloomington: Croom Helm/Indiana University Press, 1983.
Universal History of Linguistics: India, China, Arabia, Europe. Amsterdam: John Benjamins, 1991.

Jameson, F. *The Prison-House of Language: A Critical Account of Structuralism and Russian Formalism.* Princeton: Princeton University Press, 1974.

Jebsen, H. and Biel, H. (eds.) *Let's Go: The Budget Guide to Spain, Portugal and Morocco.* New York: St. Martin, 1986.

Jensen, J. B. 'On the Mutual Intelligibility of Spanish and Portuguese' Hispania 72, 4 (1989),

848-852.

Johnson, B. 'The Voice in Language' *Annual Review of Anthropology* 29 (2000), 405-424.

Johnson, M. *The Body in the Mind*. London: University of Chicago Press, 1990.

Jonassohn, K. and Doerr, K. 'The Persistence of Nazi German.' *Montreal Institute of Genocide and Human Rights Studies Occasional Paper* (1999). Source: http://migs.concordia.ca/occpapers/n_german.html

Jones, P. 'Why there is no such thing as "critical discourse analysis"' *Language and Communication* 27 (2007), 337-368.

'You want a piece of me? Paying your dues and getting your dues in a distributed world' *AI&Soc* 25, 4 (2010), 455-464.

'Value for Money? Putting Marx through the Mill.' *Language Sciences* 33, 4 (2011), 689-694.

Joseph, J. *Limiting the Arbitrary: Linguistic Naturalism and its Opposites in Plato's Cratylus and Modern Theories of Language*. Amsterdam: John Benjamins, 2000.

Katz, J. (ed.) *The Philosophy of Linguistics*. Oxford: Oxford University Press, 1985.

Kenny, A. *The Legacy of Wittgenstein*. Oxford: Blackwell, 1984.

Koerner, E.F.K. *Ferdinand de Saussure: Origin and Development of his Linguistic Thought in Western Studies of Language*. Wiesbaden: Vieweg, 1973.

Toward a Historiography of Linguistics Amsterdam: John Benjamins, 1978.

Konieczna, E. and Kleparski, G. A. 'Motivation in Language: The case of metonymically motivated innovative nouns in children's speech.' *Odisea* 7 (2006), 103-117.

Kowalenko, R. 'How (not) to think about idealisation and ceteris paribus-laws.' *Synthese* 167 (2009), 183-201.

Kripke, S. *Naming and Necessity.* Oxford: Wiley-Blackwell, 1981.

Kuhn, T.S. *The Structure of Scientific Revolutions.* Chicago: Chicago University Press, 1970.

Kusch, M. 'Fodor V. Kripke: Semantic dispositionalism, idealization and ceteris paribus clauses.' *Analysis* 65, 2, (2005)156–163.

Labov, W. 'The Social History of a sound change on Martha's Vineyard in Massachusetts.' Master's essay, Columbia University, 1962.
Sociolinguistic Patterns. Philadelphia: University of Pennsylvania Press, 1973.
'Some Observations on the Foundation of Linguistics.' Unpublished Paper (1987).
Principles of Linguistic Change: Social Factors. Oxford: Blackwell, 2001.

Lakoff, G. 'The Invariance Hypothesis: Is abstract reason based on image-schemas?' *Cognitive Linguistics* 1, 1 (1990), 39-74.

Lakoff, G. and Johnson, M. *Metaphors we Live by.* London: University of Chicago Press, 1980.
Philosophy in the Flesh: The Embodied Mind and Its Challenge to Western Thought. New York: Basic Books, 1999.

Lange, M. 'Who's afraid of Ceteris Paribus laws? Or: How I learned to stop worrying and love them.' *Erkenntnis* 57, 3, (2002) pp.407-423.

Lass, R. *On Explaining Language Change.* Cambridge: Cambridge University Press, 1980
Historical Linguistics and Language Change. Cambridge: Cambridge University Press, 1997.

Lehrer, K. 'The Coherence Theory of Knowledge.' *Philosophical Topics* 14, 1 (1990) 5-25.

Lemert, C. 'Language, Structure and Measurement: Structuralist Semiotics and Sociology.' *American Journal of Sociology* 84, 4 (1979), 929-957.

Lepore, E. 'In Defence of Davidson.' *Linguistics and Philosophy* 5, 2 (1982), 277-294.

Levi-Strauss, C. Structural Anthropology. London: Penguin, 1993.

Lind, G. 'Language Gaps and Language Links in Scandinavia.' *Anthropological Linguistics* 14, 2 (Feb., 1972), 62-69.

Lord, R. *Comparative Linguistics*. London: English Universities Press, 1974.

Lyons, J. *Chomsky (3rd edition)*. London: Fontana, 1991.

MacIntyre, A. *After Virtue*. London: Duckworth, 1981.

Mantzavinos, C. (ed.) *Philosophy of the Social Sciences*. Cambridge: Cambridge University Press, 2009.

Martin, M. and McIntyre, L. C. (eds.) *Readings in the Philosophy of Social Science*. Cambridge, Mass: MIT Press, 1994.

Martinich, A. P. *The Philosophy of Language (fifth edition)*. Oxford: Oxford University Press, 2008.

McLaughlin, B. Beckermann, A. and Walter, S. (eds.) *The Oxford Handbook of Philosophy of Mind*. Oxford: Oxford University Press, 2009.

McMahon, A. *Understanding language Change*. Cambridge: Cambridge University Press, 1994.

Melville, H. *Moby Dick*. Ware: Wordsworth, 1992 [1851].

Milroy, L. *Language and Social networks (second edition)*. Oxford, Basil Blackwell, 1987.

Mitchell, S. 'Ceteris Paribus: An Inadequate Representation for Biological Contingency' *Erkenntnis* 57, 3, (2002), 329-350.

Molnar, G. 'Are Dispositions Reducible?' *Philosophical Quarterly* 49, 3 (1999), 1-17.

Powers. Oxford: Oxford University Press, 2003.

Morpurgo-Davies, A. *History of Linguistics: Nineteenth Century Linguistics vol. 4: Nineteenth-Century Linguistics*. London: Longman, 1998.

Morreau, M. 'Other Things Being Equal.' *Philosophical Studies* 96, (1999) 163–182.

Morriss, P. *Power: A Philosophical Analysis (Second Edition)*. Manchester: Manchester University Press, 2002.

Mumford, S. *Dispositions*. Oxford: Oxford University Press, 1998.

Naylor, G. *Better Than Life*. London: Penguin, 1991.

Nerlich, B. *Change in Language: Whitney, Breal and Wegener*. London: Routledge, 1990.

Norris, C. *Fiction, Philosophy and Literary Theory*. London: Continuum, 2007.
Derrida. London: Fontana, 1987.

Parret, H. *History of Linguistics Thought and Contemporary Linguistics Foundations of Communication*. Berlin: de Gruyter, 1976

Pateman, T. *Language in Mind, Language in Society*. Oxford: Oxford University Press, 1987.

Paul, H. *Principles of the History of Language*. London: Longmans, Green & Co., 1891.

Pinker, S. *The Language Instinct*. London: Penguin, 1995.
The Stuff of Thought: Language as a Window into Human Nature. London: Penguin, 2007.

Pietroski, P. 'A Defence of Derangement.' *Canadian Journal of Philosophy* 24, 1 (Mar 1994), 95-117.

Pietrosky, P. and Rey, G. 'When Other Things Aren't Equal: Saving Ceteris Paribus Laws From Vacuity.' *British Journal for the Philosophy of Science* 46 (1995), 81-110.

Plato. *IV: Cratylus, Parmenides, Greater Hippias, Lesser Hippias*. Fowler, H.N. (Trans.) Cambridge, Mass: Harvard University Press, 1926.

Polom□, E. (ed.) *Research Guide on Language Change*. Berlin: Mouton de Gruyter, 1990.

Putnam, H. *Representation and Reality*. Cambridge, Mass: MIT Press, 1988.

Quine, W. V. *Word and Object*. Cambridge, Mass: MIT Press, 1960.

Radden, G. and Panther, K-U. (eds.) *Studies in Linguistic Motivation*. Berlin: Mouton and De Gruyeter, 2004.

Rescher, N. *Studies in Metaphilosophy, Vol. 9: Collected Papers*. Frankfurt: Ontos Verlag, 2006.

Resnick, L. B. and Levine, J. M. (eds.) *Perspectives on socially shared cognition*. Cincinnati: American Psychological Association, 1991.

Robins, R.H. *A Short History of Linguistics, (4th edition)*. London: Longman, 1997.

Rorty, R. 'Philosophy as a Kind of Writing: An Essay on Derrida.' *New Literary History* 10, 1 (1978), 141-160.
Truth and Progress: Philosophical Papers. Cambridge: Cambridge University Press, 1998.

Rupert, R.D. 'Realization, Completers, and Ceteris Paribus Laws in Psychology' *British Journal for the Philosophy of Science* 58 (2007), 1-11.

Sanders, C. (ed.) *The Cambridge Companion to Saussure*. Cambridge: Cambridge University Press, 2004.

Sapir. E. *Language*. New York: Harcourt, Brace and World, 1921.
'The Scientific Status of Linguistics.' *Language* 5, 4 (1929), 207-214.

Saussure, F. *Course in General Linguistics*. Harris, R (Trans.) Chicago: Open Court, 2006

[1922].

Schiffer, S. 'Ceteris Paribus Laws.' *Mind* 100, 1 (1991), 1-17.

Schurz, G. 'Ceteris Paribus Laws: Classification and Deconstruction.' *Erkenntnis* 57, 3 (2002), 351-372.

'Pietrosky and Rey on Ceteris Paribus Laws.' *The British Journal for the Philosophy of Science* 52, 2 (June 2001), 359-370.

Searle, J. R. *Speech Acts: An Essay in the Philosophy of Language.* Cambridge: Cambridge University Press, 1969.

The Construction of Social Reality. London: Penguin, 1995.

Mind, Language and Society. New York: Basic Books, 1999.

Seuren, P. *Western Linguistics: An Historical Introduction.* Oxford: Wiley-Blackwell, 1998.

Chomsky's Minimalism. Oxford: Oxford University Press, 2004.

Sheridan, R.B. *The Rivals.* Oxford: Oxford University Press, 1968 [1775].

Silverberg, A. 'Psychological Laws and Nonmonotonic Reasoning.' *Erkenntnis* 44 (1996), 199-124.

Sluga, H. and Stern, D.G. *The Cambridge Companion to Wittgenstein.* Cambridge: Cambridge University Press, 1999.

Sorokin, P. *Social and Cultural Dynamics.* New York: Porter, 1982 [1938].

Sperber, D. and Wilson, D. *Relevance: Communication and Cognition.* Oxford: Basil Blackwell, 1986.

Spohn, W. 'Laws, Ceteris Paribus Conditions, and the Dynamics of Belief.' *Erkenntnis* 57, 3 (2002), 373-394.

Stanley, J. *Language in Context: Selected Essays.* Oxford: Oxford University Press, 2007.

Staten, H. *Wittgenstein and Derrida*. London: University of Nebraska Press, 1986.

Stockwell, P. and Macaulay, R. (eds.) *Linguistic Changes and Generative Theory*. Bloomington: Indiana University Press, 1972.

Stove, D. 'Deductivism.' *Australasian Journal of Philosophy* 48, 1 (1970), 76-98.

Thibault, P. *Re-reading Saussure: The function of signs in social life*. London: Routledge, 1997.

Trask, R. L. *A Student's Dictionary of Language and Linguistics*. London: Arnold, 1997.

Travis, C. *Occasion-Sensitivity: Selected Essays*. Oxford: Oxford University Press, 2008.

Van Fraassen, B. *The Scientific Image*. Oxford: Oxford University Press, 1980.

Waugh, L. 'Against Arbitrariness: Imitation and Motivation Revived, with Consequences for textual meaning.' *Diacritics* 23, 2 (1993), 71-87.

Wasow, T. 'The Wizards of Ling.' *Natural Language and Linguistic Theory* 3, 4 (1985), 485-491.

Weber, A. *Economy and Society*. Fishchoff, E. et al (Trans.) Berkeley: University of California Press, 1968 [1922].

Westphal, S. 'Force, Understanding and Ontology.' *Bulletin of the Hegel Society of Great Britain* 57/58 (2008), 1-29.

Winch, P. *The Idea of Social Science*. London: Routledge and Kegan Paul, 1959.

Wittgenstein, L. *Tractatus Logico-Philosophicus*. Pears, D. F. and McGuiness, B. F. (Trans.) London: Routledge, 2001 [1922].

Online Resources:

www.guardian.co.uk/uk/2011/dec/05/anger-police-fuelled-riots-study

http://www.thefreedictionary.com/Spork.

http://rdues.bcu.ac.uk/newwds/2008.html