Modestly Modular vs. Massively Modular Approaches to Phonology

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Abstract

This thesis considers the extent to which phonology (that is, the phonological processor) can be considered a module of the mind. It is divided into two parts. In the first, an approach of 'modest' modularity owing to Fodor (1983) is explored. In the second, the 'massive' modularity model, due to evolutionary psychologists in general, but Caruthers (2006a) in particular, is examined. Whilst for Fodor (1983, 2000) the mind is only modular around its periphery (i.e. only its input and output systems are modules), for massive modularists the mind is modular through and through, up to and including its central capacities. The two authors, therefore, by extension differ in their definitions of modularity: Fodor (1983, 2000) sees 'informational encapsulation' as being essential to modularity, whereas for Carruthers (2006) domain specificity is much more important. The thesis concludes that whether phonology is a module or not then depends on the definition of modularity, for although a substance-free phonology which has no phonetic grounding could count as strong evidence for the informational encapsulation (and therefore the modularity) of phonology by Fodor's (1983) standards, some aphasiology data has shown that semantic treatments can remediate phonological word finding difficulties in aphasia, which would be indicative that phonology is not domain-specific, and therefore amodular in the terms of massive modularists like Carruthers (2006a).[1] In order to answer whether phonology is modular, then, we must first define, once and for all, what modularity (and indeed phonology) means. Until then, the debate remains, and so does my resolve to settle it.

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[1] This result is challenging to Jackendoff's (2002) parallel architecture of the language faculty too, I argue, which requires the phonological (integrative) module to be *both* informationally encapsulated and domain specific.

Dedication

To John. I dedicate this thesis to you.

Acknowledgements

I wish I had an 80,000 word limit for this acknowledgements section alone, as in this short space I doubt I'll ever do you all justice. Now of course it's customary that one thanks their primary supervisor first, but S.J. really does deserve to come at the top of this page for many more reasons than that. I've worried all the way through that he would leave before I did but alas, we stuck together 'til the bitter end, with him retiring the very same day I submitted. THANK YOU UNIVERSE.

I wouldn't be here if he hadn't have introduced me to Fodor's (1983) modularity of mind thesis in the third year of my BA (Hons), and I definitely wouldn't be here if it wasn't for the lol catz on his lecture slides (j.k. – although, they were canny good). I've been turning up at his office door unannounced for 6 years now with a knock, knock and an 'areet', and truth be told, I'm rather going to miss it. He's been a supervisor, a mentor – hell – even a friend (?). If you haven't had the pleasure of meeting the man yet I strongly urge you to slide into his inbox. My fondest memories of Newcastle Uni have to do with him (even if I have avoided every single social 'cause I can't people).

Special thanks are also due to my secondary supervisor Joel Wallenberg. He's a Jack of all trades and master of all (no, trust me – he is). He's one *the* smartest people I've had the pleasure of picking the brains of and is super sweet too, which is always a bonus, right? I'm so glad he came on board when we switched around my supervision. The quantitative part of my thesis won't have been possible if it weren't for Joel (I hadn't touched statistics for 10 years!), and nor would my musings on language change. Thank you, thank you, thank you for all that you've done (and been) – you really are a genius.

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Chapter 1: Introduction

Great attention has been paid to complex systems in the sciences, and for good reason. The phenomena we wish to understand are all complex, and the problems we need to solve in order to understand them are even more so. Take a balloon, for example, filled with helium gas. The balloon itself isn't necessarily complex, but how it interacts with the earth's atmosphere (e.g. how it floats because the gas which fills it (helium) is lighter than the air it displaces (which is, of course, mostly made up of nitrogen, oxygen, argon and carbon dioxide) is (Simon 2005)). The source of the balloon's complexity, therefore, isn't in itself, but in its *interaction* with a more complex system that is the earth's atmosphere (Simon 2005).

It might be fair to say then, that complexity arises when systems interact with each other in ways that amount to something more than they could ever be by themselves (Simon 2005); when they are more than just the sum of their parts, because they're a part of something else as well. Before the big bang the universe was not particularly complex. All there was in the cosmos was a gravitational singularity, that is, a point in time and space where gravity is infinite, space-time curves infinitely and the laws of physics as we know them cease to exist (Hawking 1988). It was after the fact that complexity arose, when stars (and then stellar galaxies) were formed, and solar systems within them. Our planet, the earth, is of course complex – both in terms of the elements that make it up, and its place in the solar system (and therefore the Milky Way).

It has been known for some time now that the complex systems we observe in the natural world have hierarchical structures; that they are made up of complex subsystems, which themselves are made up of complex subsystems, and so on and so forth (Simon 2005). The smaller subsystems interact with each other of course (or else they wouldn't form larger subsystems or systems) but what's interesting to this project in particular is that the frequencies associated with the subsystem interactions drop stealth the higher up the hierarchy you go (Simon 2005).

We call complex systems with this property nearly decomposable (Simon and Ando 1961), and these have been the subject of scientific research for tens to hundreds of years (Callebaut and Rasskin-Gutman 2005). A question of interest to *cognitive* scientists especially though is this one:

> *If complexity in our universe at all levels generally takes this hierarchical nearly decomposable (or modular) form, is this true of a product of the universe - the mind - as well? Is the human mind (and its components) modular?*

Jerry Fodor was among the first to try and answer this question and in his 1983 publication *The Modularity of Mind* he claimed that the mind is indeed modular, or it is at least around its periphery in the input and output systems. Evolutionary psychologists, however, believe Fodor's (1983, 2000) thesis of mind modularity to be too modest, and that the central systems are modular as well. The idea that the mind is modular up to and including its central capacities is what we call the massive modularity hypothesis. It was articulated and defended by Peter Carruthers in 2006, and it is against these two theses of mind modularity (Fodor's (1983, 2000) modest thesis and Carruthers' (2006a) massive one) that my research question is based.

I question in particular whether the phonological processor is modular. Being part of the language faculty (and therefore one of the input systems) both Fodor (1983, 2000) and Carruthers (2006a) would argue that it is, but they each have different definitions of modularity – and there lies the crux of the problem. For Fodor (2000), a modest modularist, informational encapsulation is the defining feature of modularity. Evolutionary psychologists, on the other hand, place greater emphasis on domain specificity in their definitions of modules in their massive modularity theories (e.g. Sperber 1994; Cosmides and Tooby 1994; Pinker 1997; Carruthers 2006a).

This thesis will, therefore, be split into two halves. After providing an introduction to modularity in general in Chapter 2 and drawing differences between modest modularity and massive modularity in Chapter 3, I will explore whether phonology can be considered a modest or massive module of the mind. In Chapter 4 I will consider the extent to which phonology can be considered informationally encapsulated and therefore a modest module and in Chapter 5 the extent to which it can be considered domain specific and therefore a massive one.

I do this, because there seems to be even less of a consensus among phonological practitioners at present, than there was forty or fifty years ago, and – whilst it would be naive to think that there ever will be *full* agreement as to the modularity or nonmodularity of phonology – I would hope that my work (which explores two different definitions of what it means to be modular) will contribute to the discussion in at least some small way. I do hope, however, that we do one day find an answer to this question – on the one hand, to inform linguistic theory, and on the other to better inform the field of aphasiology so that treatments of phonological anomia (which is an impaired ability to access words in and retrieve them from the mind that's due to a deficit at the phonological level of language processing) can be improved.

Returning to my previous point, there are a number of reasons for the lack of consensus concerning modularity among phonologists Hannahs and Bosch (2018) argue, which I will give an overview of very briefly here in order for one to see where my research sits within the wider research context. The first (and perhaps most obvious one) is Optimality Theory (or OT) (Prince and Smolensky 1993), which caused controversy among generativists (Hannahs and Bosch 2018). The framework on which it was built called into question some of the claims made within the generative phonology field. These included (but of course weren't limited to) one's reliance on rules to mediate between underlying and surface forms and the theory that the two (abstract underlying representations and concrete surface representations, that is) are opaquely related (Hannahs and Bosch 2018).

The development of OT, however, is of course not the *only* contributing factor. Again during the 1980s/early 1990s (which was around the time as OT's advent too), phonologists began to question some of the assumptions of abstractness underlying the (then-current) approaches to generative grammar. Within OT, e.g., there was an endeavour to ensure that constraints were grounded in phonetics (e.g. Pierrehumbert 2000; Archangeli and Pulleyblank 1994; Hayes et al. 2004). Outside of OT, there was a trying to tie phonology and phonetics together (as was the case with articulatory phonologists (e.g. Browman and Goldstein 1986), usage-based phonologists (e.g. Bybee 2003) and exemplar theorists (e.g. Bybee 2006; Johnson 2007) (Hannahs and Bosch 2018).

Two other recent developments are likely to have led to this fragmentation within the field, Hannahs and Bosch (2018) argue, however – sociophonetics and database studies. Sociophonetics, they say, having grown out of the Labovian sociolinguistic paradigm (in which sociolinguistic variation – such as the alternation of the allophones [n] and [ŋ] in the pronunciations of words like *singing* with and without g-dropping, respectively – is examined from the perspective of socio-economic contexts) generally has a heavier focus on phonetics than it does on phonology. For most sociophoneticians, the phonological system itself is of little interest, they point out, though this is, of course, a bit of a sweeping statement. Indeed there are (albeit a small number of) phonologists interested in interpreting sociophonetic variation in the light of phonological theory (Fruehwald 2013, 2016). This is something I touch on in Chapter 4.

The other recent development, database studies, has led to an interest in 'big' data and, for some practitioners, an assumption that the absence of big data to support a theoretical position invalidates it entirely (Hannahs and Bosch 2018). Whilst this seems to be a stance taken by empirical scientists in general, I should point out that the problem with this line of thinking in phonology in particular is that, from a competence/performance perspective, surface forms cannot always account for what is going on underlyingly.

There is more to be understood in phonology than what can be *observed* and so - if we don't engage with the systems underlying those observations and ask questions based on what we've found (no matter how big or small our datasets are) - we run the risk of missing important pieces of the puzzle, Hannahs and Bosch (2018) point out. For me, both empirical concreteness and theoretical abstraction have an important part to play in phonology, and so I draw on (but, note: don't rely on) empirical data from language change in Chapter 4, and speech and language pathology in Chapter 5, in order to inform my theoretical thinking. As Hannahs and Bosch (2018) put it:

> [w]hile empirical concreteness has contributed enormously to our understanding of what is possible in human language, there is value also in focused exploration into more abstract elements of language: elements that we cannot see or measure, such as phonological structures, non-surface-true generalizations, and relationships that can only be inferred through theoretical analysis

To circle back to my previous point, I make my main contributions to knowledge in Chapters 4 and 5. In Chapter 4, I conclude that phonology can be considered substance-free and therefore can be considered an informationally encapsulated module of the mind according to Fodor's (1983, 2000) definition of (modest) modularity. In Chapter 5, on the other hand, I present evidence from the field of aphasiology which is suggestive that phonology is not domain specific (and conclude that it is, therefore, *amodular* to the minds of massive modularists (like Carruthers (2006)) who demand domain specificity in their definitions of modularity).

Also in Chapter 5 (and 6), I show how my results are relevant to a broader range of questions than those about the modularity of phonology. Firstly, I argue that the models of the language faculty in the clinical literature are limited in that they neither offer a clear theory of the lexicon and lexical processing, nor do they account for the communication between the semantic and phonological submodules they posit in their proposals. Secondly, I argue that Jackendoff's (2002) parallel architecture model of the language faculty better does this, for Jackendoff (2002) there aren't just two component parts to the grammar (semantics and phonology) there are three (semantics, syntax and phonology) which are all, what he calls, *integrative* modules, and that they each communicate with one another via *interface* modules, which transform the output of one module it interfaces into an input interpretable by the other it interfaces and vice versa. I also argue that Jackendoff's (2002) theory of the phonetics-phonology interface (or interfaces) is superior to the one owing to Fodor (1983, 2000) and colleagues because theirs, being innate, cannot account for cross-linguistic variation. Jackendoff's (2002) theory, which does not assume innateness, can account for this better, I stress. It also provides an answer to the question of where, in the language faculty, is damaged when disorders of phonetic encoding/phonemic assembly arise – that answer being, in the phonology-phonetics interface.

Towards the end of the thesis I point out, however, that the way in which Jackendoff (2002) models the language faculty requires the phonological processor to be both informationally encapsulated (as Fodor (1983, 2000) does) and domain specific (as massive modularists like Carruthers (2006a) do) and so the data I collect and analyse in Chapter 5 casts doubt on not only phonology's domain specificity, but also Jackendoff's (2002) model's representativeness of phonology. To finish, I endorse a roughly Fodorian view that phonology is modular in that it is informationally encapsulated but not domain specific, albeit tentatively. Further research should be conducted with large sample sizes I stress, to confirm or deny whether this is indeed the case.

I think it is safe to say that this thesis is an ambitious one, for it considers a question to which many a body of research is relevant. It necessarily crosses the disciplines of philosophy of mind and language, linguistics, cognitive psychology, evolutionary psychology, language science and – to some degree – natural science and computer science, because, as Caruthers

(2006a) points out, we all seem to be asking the same questions about modularity but taking different routes in order to answer them, which is just journeying us further and further away from each other (and most likely the truth). I firmly believe that the only way we'll reach a clearer conclusion about the modularity of phonology than we have right now is to bring together different domains of inquiry, and so I hope that the conclusions I come to and questions I ask at the end of this cross-disciplinary thesis provide a useful starting point for the research of others to bring about the knowledge transfer between all fields that is so desperately needed for us to move forward. This research is unique in a number of ways, but particularly so in how broad a scope it has.

To begin, I describe the difference between the natural modularity we see in biology and the artificial modularity we see in technology, and distinguish between types of biological modules (detailing the difference between modules of mind and brain and other biological modules seen in our species and others, the difference between cognitive modules and neural modules, representative and computational modules and Darwinian and non-Darwinian modules) to demonstrate where the Fodorian and Carrutherian modules discussed in this thesis sit within the wider research context.

I provide a surface overview of the ways in which they are similar to (both computational, cognitive, natural modules of mind) and different (one is Darwinian, and the other is not) from one another, and the ways in which they are different from other modules that are e.g., representational rather than computational like them, neural rather than cognitive like them, biological, but not having to do with the mind or brain like them, and technological instead of natural like them, so that we have a rough picture of what Fodorian and Carrutherian modules are and aren't before I dive into a more in-depth discussion of Fodor's and Carruthers' understandings of modularity. Now with that being said, let us turn to Chapter 2.

Chapter 2: An Introduction to Modularity

1. Introduction

Describing the world in terms of the modular organisation of its parts dates as far back as 1543 when Copernicus' *De Revolutionibus Orbium Coelestium (On the Revolutions of the Heavenly Spheres)* marked the beginning of the scientific revolution (Callebaut 2005). It inspired Leibniz's and Kant's work on faculty psychology in the 17th and 18th centuries as well as Gall's work on phrenology in the 18th and 19th, and even now, in 2019, modularity is a driving force of much scientific research. In biology, both structural and functional modules are recognised, and engineers, having seen the value of modularity as it exists naturally in biological systems, have come to make use of modular design principles themselves in the technological domain (Wilhem 1997). In the cognitive sciences, modules as systems of representation have been proposed by the likes of Chomsky (1965, 1988), and in 1983 Fodor launched a debate on whether the mind is made up of functional modules – one that has, of course, continued to the present day. Neuroscientists have asked similar questions to the ones that cognitive scientists have asked about the mind about the brain, but I will have no more to say about those here, for that goes beyond the scope of this thesis.

This chapter provides some conceptual foundations for the enterprise that follows along the lines of Callebaut (2005). I first describe the various meanings and uses of the word 'modularity' (for example as *explanans* in the case of natural modules or *explanandum* in the case of artificial modules). I then provide as general a definition as possible, that applies to the wide range of contexts in which it appears in section 2. I will then describe the differences between the fields of computational cognitive science and neural cognitive science and their modules in section 3.1 before looking in some detail at the differences between cognitive and neural modules in section 3.2. Lastly, I will outline the two types of cognitive modules that have been explored in the cognitive science literature: (non-Darwinian) computational modules (in section 3.3.1.1) and Darwinian computational modules (in section 3.3.1.2). These will be the focus of the thesis, and so the chapter after this one (i.e., 3) will be devoted to their discussion.

2. Natural modules vs. artificial modules

In our world, modular systems – whether they be natural ones (as is the case in biology and the cognitive and neurosciences) or artificial ones (as is the case in technology) – abound

(Simon 1969). They have been discovered by a number of researchers working in a number of different research domains, but despite the differences in these researchers' definitions of modularity (there are many), they all seem to be in agreement that modules are ‘unit[s] that [are] component part[s] of […] larger system[s] and yet possessed of [their] own structural and/or functional identit[ies]’ (Moss 2001: 91).

Consider some examples from biology and technology. In biological organisms, cells come together with cells of similar structures to perform shared functions as tissues; tissues come together to create organs and organs, organ systems (Ritter 1998). The heart, for example, is made up of cardiac tissue (which is itself made up of cardiac cells) and functions as part of the cardiovascular system to pump blood around the body (Farley et al. 2012). The cardiovascular system works together with two other systems (the pulmonary and the systemic) as part of the circulatory one, to transport through the blood oxygen and essential nutrients *to* cells and waste products *from* them (Farley et al. 2012). The parts and systems of cars (which are of course technological artefacts), on the other hand, are susceptible to independent breakdown and repair because of their modular architectures. Damage to a car’s brake pedal would affect the functionality of its brake system, for example, of which it is a part, but not its exhaust or steering systems, of which it is not. The functionalities of those, the exhaust and steering systems, that is, would remain intact.

It is important to bear two things in mind here, however. Firstly, the structural and functional modularities mentioned by Moss (2001) are two very different things, and so don’t necessarily map neatly onto one another (I’ll return to this point in more detail in section 3.2.1 where distinctions between neural modules (i.e., modules of structure) and cognitive modules (i.e., modules of function) will be drawn). Secondly, one must bear in mind that different research domains make use of quite different research strategies. Modules are said to be ‘top-down’, for example, when they are the product of a researcher breaking down a complex system in a reverse engineering fashion to gain an insight into its inner workings. This is the type of module that is typically found in biology and is especially common in cognitive science (Bechtel and Richardson 1993). ‘Top-down’ modules aren’t empirically observed but brought forth as *explanans* (Moss 2001; Eble 2005) in straightforward Popperian (1935, 1959) fashion. Examples of the ‘bottom-up’ research strategy in which small, simple, subsystems are pieced together to give rise to a larger, more complex system can be found in technological arenas, on the other hand, such as engineering, as well as in neuroscientific research (Barbutti et al. 1993; Fontana and Buss 1994; Adami 2002; Husken et al. 2002). In this context, modularity arises as an *explanandum* as opposed to as *explanans* (Bolker 2000).

Now, given that this is a thesis on cognitive modularity and not neural modularity (or technological modularity, for that matter), and since we've established that modules in the cognitive sciences are all 'top-down' – I will have no more to say about the distinction I've drawn between top-down and bottom-up research strategies here. To this thesis, what matters most is what constitutes a module in the eyes of the researchers who posited them not how their characterisations came to be, and so this will be the focus of the subsections that follow.

3. Modularity in cognitive science

Just as developmentalists and evolutionists disagree about what it means to be a module and for a system to be modular in biology, the thesis that the mind is modular in its architecture has triggered a tremendous amount of often heated debate in the cognitive sciences as well (Callebaut and Rasskin-Gutman 2005). Fodor (1983) is often cited as having had the most impact on recent theorising on these matters, for an entirely new discipline, evolutionary psychology (which rests on a massively modular conception of cognitive architecture, that is, that the mind is composed largely (or even entirely, actually) of computational modules (Callebaut and Rasskin-Gutman 2005)) was born out of cognitive scientists contesting Fodor's (1983) thesis that the mind is less modular than that (see section 3.3). One of the most obvious reasons why modularity is such a controversial topic in the cognitive sciences, however, is that there are two different (and conflicting) approaches to the study of cognitive scientific research: computational cognitive science and neural cognitive science (Calabretta and Parisi 2005), and so I'll now turn my attention to that.

3.1 Computational cognitive science vs. neural cognitive science

The theoretical paradigm of computational cognitive science (or cognitivism) is based on an analogy of the mind as the software of a computer, according to which the mind is a system that manipulates symbols computationally (Newell and Simon 1976). More recently, though, a different kind of cognitive science called neural cognitive science (or as some prefer, connectionism (Rumelhart and McClelland 1986)) has arisen, that rejects the analogy of the mind as computer software, instead interpreting our cognitive capacities using theoretical models known as neural networks that are inspired by both the structure and the functioning of the nervous system (Rumelhart and McClelland 1986). For connectionists, the mind is not a computational system of symbol manipulation; rather, it is the result of the many interactions that take place among neurons in a neural network. For them, the mind 'consists entirely of quantitative processes in which physiochemical causes produce physiochemical effects' (Calabretta and Parisi 2005: 309).

Cognitivists and connectionists also differ in their assumptions about modularism and nativism. While computational cognitive science tends to be strongly modularistic, neural cognitive science does not (Calabretta and Parisi 2005). Meanwhile, whilst cognitivists tend to be nativists, connectionists tend not to be (Calabretta and Parisi 2005), as Table 2.1 depicts:

Two types of cognitive science			
Computational cognitive science or cognitivism	Mind as a computational system of symbol manipulation	Modularist	Nativist
Neural cognitive science or connectionism	Mind as the global result of the many physicochemical interactions that take place in a network of neurons	Anti-modularist	Anti-nativist

Table 2.1 A table showing the two different approaches to studying cognitive science

But alas, although cognitivists can be differentiated from connectionists in terms of their subscriptions to modular and nonmodular, and nativist and anti-nativist, ideals, one should note that cognitivists are divided on some things as well, in particular on their views of the adaptive nature of inherited traits. Evolutionary psychologists, for example, believe that cognitive modules are one of the many biological modules, and so think of the modular structure of the mind as being the result of evolutionary pressures from the environment as the modular structures of biological systems are. They, therefore, embrace a strong form of adaptationism as well as nativism (Calabretta and Parisi 2005); not only do they believe that modules of the mind are a part of a human's genetic material, they believe that they arose as a consequence of evolution (Calabretta and Parisi 2005).

Not all cognitivists subscribe to this idea, however. For example, while Noam Chomsky sees the mind as a modular system that has a specific subsystem that is specialised for language (Chomsky 1965, 1988), he does not believe that language in humans emerged under any specific evolutionary pressure (Fodor 2000). Nor does Fodor (1998), actually, who is in fact in favour of a strong form of nonadaptive modularism (Fodor 1998). I will leave the debate between proponents of adaptive modularism and nonadaptive modularism here for now but will return to it when I come to describe the differences between the cognitive modules in more detail in section 3.3.

3.2 Cognitive modules vs. neural modules

Although a cognitive scientist (a.k.a. Fodor 1983) popularised the term 'module', the concept is not a new one. It is not uncommon for the complex systems of the mind *or* the brain to be analysed in terms of collections of less complex subsystems. In the neurosciences, there has been a long-standing interest in decomposing into component structures called modules as well (Meyering 1994).

I should probably point out that when I speak of neural modules in this section, I don't mean the modules of the artificial neural networks modelled by connectionists that are described in section 3.1, since, as I said there, they tend to be nonmodular in their architecture. What I mean by neural modules in this context are the natural modules of brains themselves rather than the modules of artificial models of them. In this section, then, I draw a distinction between the (more functional) modules of the mind that are proposed by cognitivists and the (more structural) modules of the brain that are proposed by neuroscientists.

3.2.1 Function vs. structure

By the end of the nineteenth century neurologists Wernicke, Sherrington and Cajal had introduced a new and empirically based theory of the brain according to which its functioning took place in a cellular system of neurons (the brain's operative units (Meyering 1994)) which were arranged into more or less functional groups (Schnelle 2010). This was the most influential view of the brain's architecture until the turn of the twentieth century, which brought with it enough empirical and conceptual progress to lead Hebb (1949) to propose a neural sub-structure like the one in Figure 2.1:

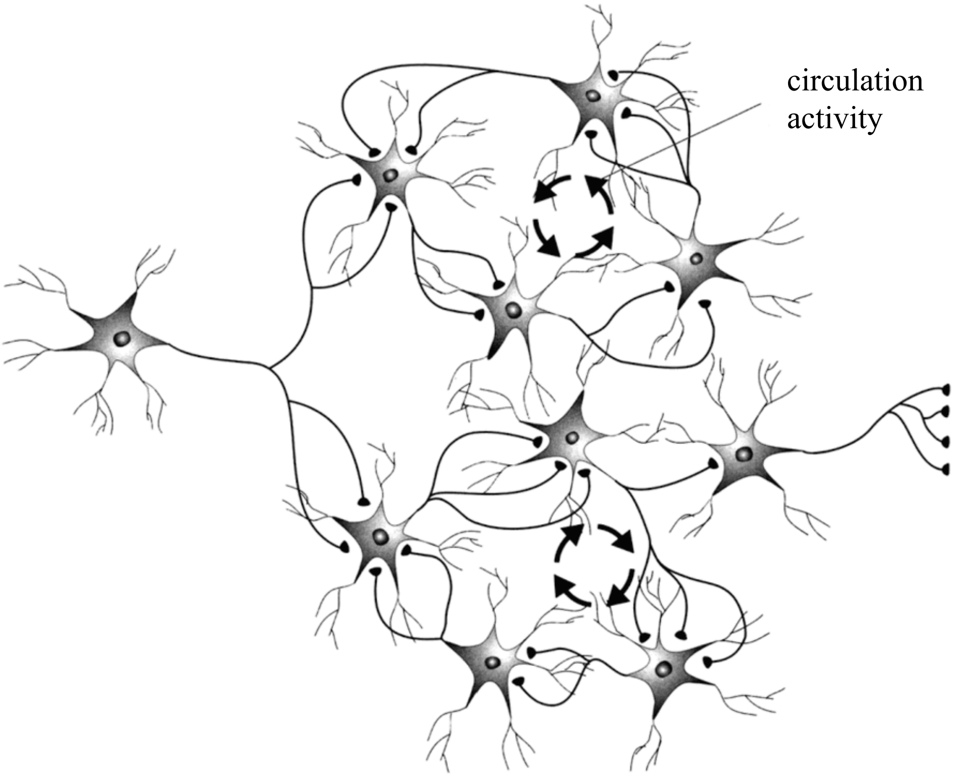

Figure 2.1 Hebb's (1949) neural sub-structure schema (adapted from Schnelle 2010: 9)

According to Hebb (1949), cellular organisations operate in very different areas of the cortex, which is itself divided into four anatomically distinct areas: the frontal lobe, the parietal lobe, the occipital lobe and the temporal lobe, which each contribute to different sub-aspects of more globally organised functions such as perception, memory and motor control (Kandel et al. 1995). Hebb (1949) individuates the modules of the brain as anatomical entities that come together in their operations. This is supported by research in more recent years (e.g. Lettvin et al. 1959; Powell and Mountcastle 1959; Kilmer et al. 1969; Hubel and Wiesel 1974) and so in general, it seems fair to say that the concept of a module in the neurosciences is one of *structure* rather than of function (Arbib 1987).

By contrast, the models of mind modularity that are proposed by cognitivists are characterised in a fundamentally *functional* way. Particularly prevalent in the cognitive science literature is the one developed by Fodor (1983). He began with a cognitive analysis of global psychological functions (such as linguistic and perceptual behaviour) and developed a functional taxonomy of them based on the characteristic operational differences between the systems involved in their functioning (Meyering 1994).

For cognitivists, modules are *postulated* rather than *observed* entities, then (Calabretta and Parisi 2005). They are components of theories of mind, which hypothesise that the mind can be broken down into a number of functionally independent components that work together to explain some phenomenon of interest (Calabretta and Parisi 2005). This was evidenced in formal linguistics of the Chomskyan variety, which observed the linguistic judgements of native speakers to interpret that there are three autonomous modules that work together in linguistic competence: syntax, semantics and phonology. This purely theoretical notion of a module is defined and defended by Fodor (1983) in *The Modularity of Mind.* Evolutionary psychologists, who tend to disagree with Fodor (1983) about the extent to which the mind is modular and as a consequence, his definition of a module (a point I will come back to in section 3.3), have a cognitivist orientation too. They conceive of the mind as a collection of specialised and genetically inherited adaptive modules (which are, again, like Fodorian modules, in that they have a cognitivist orientation in that they are functionally independent (Calabretta and Parisi 2005)).

3.2.2 Closing the gap between functional and structural modules

There is a tendency for humans to fill in the unspecified details of ontologically ambiguous objects (Boyer 2001; Bloom 2004). This is particularly apparent in our responses to theories that postulate information structures but leave all of the parameters other than those of the theory itself unstipulated (as in the spirit of Chomsky (1965), for example, who theorised that the grammar is made up of a syntactic, a semantic, and a phonological component only) (Barrett 2006). It is important to recognise, however, that underspecification must be taken seriously if theories like Chomsky's are to have any real value (Barrett 2006). It is a mistake to criticise a theory about psychological entities by filling in parameters that weren't meant to be filled in in the first place and then question the plausibility of the postulated structure based on what one has filled in (Barrett 2006). While it might well be reasonable to ask of Chomsky where in the brain syntax, semantics and phonology are located, that question is in many ways irrelevant to whether his hypothesis is correct or not because it was framed in terms of information, not in terms of brain structure. As Marr (1982) rightly pointed out, while there is a relationship between hypotheses about information structure and hypotheses about brain structure, that relationship is an asymmetric one, and there are many ways in which an information structure might be instantiated.

Although, then, one can look for correspondences between the two types of modules (i.e. the theorised modules of cognitive science and the observed modules of the brain), one cannot assume that there will be any. The brain's organisation into structural modules by neuroscientists mightn't marry up with the cognitive scientists' compartmentalisation of the mind into functional modules, but that does not necessarily mean that either of the theories must be wrong as they are each built on entirely different foundations.

3.3 Cognitive modules: Some differences

Ever since the cognitive revolution in psychology began some 60 or so years ago with Chomsky's (1959) review of Skinner's (1957) *Verbal Behavior*, the evidence in favour of cognition having a modular architecture – which is, by way of explanation, to say 'that cognition is subserved by a number of innately channelled […] systems whose operations are largely independent of, and inaccessible to, the rest of the mind' (Carruthers and Chamberlain 2000) – has been mounting up. Initially the evidence only supported Fodor's (1983) conception of cognition being modular around the edges (i.e. the idea that only the input and output systems of the mind, that is, those responsible for the five senses of opthalmoception, audioception, gustaception, olfaception and tactioception and the system responsible for our linguistic competence) are modular (Carruthers and Chamberlain 2000). More recently, however, there has been evidence (from e.g. Atran (1990), Baron-Cohen (1995) and Sperber et al. (1995)) to suggest that the central systems (i.e. those charged with the generation of beliefs from perception or indeed, other beliefs) are modular in structure as well (Carruthers and Chamberlain 2000).

Recall also from section 3.1 that while cognitive modularism is typically associated with nativism, not every cognitive modularist has taken an evolutionary perspective (Carruthers and Chamberlain 2000). Chomsky (1988) and Fodor (1998), for example, have been inclined to ignore all evolutionary theorising, thinking of modules as mere by-products of the expansion of the hominid neocortex instead (Carruthers and Chamberlain 2000). Evolutionary psychologists, on the other hand, share a different position, that evolution by natural selection is the only non-cultural explanation we have for the development of organised, functional complexity (as Pinker and Bloom (1990) and Pinker (1994) so decisively point out) (Carruthers and Chamberlain 2000).

In order to make sense of the various interpretations of modularity in cognitive science, Gobet (2015) helpfully distinguished two meanings of modularity: the *functional* (for example the modules proposed by Fodor (1983) that will be covered in section 3.3.1.1) and evolutionary psychologists (which will be the focus of section 3.3.1.2)) and the *knowledge meaning* (read 'representational') which has some kinship with the notion of modularity used by Chomsky (1965, 1988), but not his later work.

This is not dissimilar to Samuels' (2000) earlier classification of Fodor's (1983) and evolutionary psychologists' modules as computational modules, but while Fodorian and Carrutherian modules bear similarities to one another in terms of their functionality, they differ in that the modules of evolutionary psychology are *Darwinian* (take an evolutionary perspective) whereas Fodorian (1983) modules are not. These distinctions are especially relevant to those who wish to assess the evidence for the existence of computational modules in cognition as I do, and so the following sections will serve to dichotomise them further.

3.3.1 Computational modules

3.3.1.1 Non-Darwinian modules

Despite Chomsky's (1965, 1988) work on modularity in linguistics, it was Fodor (1983)'s seminal *Modularity of Mind* that dominated the theoretical discussions in philosophy and psychology, and provided the modern origins of the more general modular models of cognition we know today (Lyons 2001; Carruthers et al. 2005). Fodor (1983) argued that there is a trichotomous architecture to cognition in that it's composed of: *transducers* which serve to convert stimuli into signals to be used in processing, *central systems* which are responsible for the likes of general inference, reasoning, and the generation of beliefs and desires and *input systems* which act as intermediaries between the sensory transducers on the one hand and the central cognitive system on the other by taking the transducers' signals and converting them into hypotheses about the external world in a format that the central system is able to operate on (Meyering 1994). The primary function of input systems Fodor points out is 'to so represent the world as to make it accessible to thought' (1983: 40).

According to Fodor (1983), there is a world of difference between the rational computations of an open cognitive system (i.e. central cognition) and the unconscious, automatic computations of a closed perceptual one (i.e. the part of cognition that is made up of input systems). The cognitive abilities of input systems, Fodor (1983) stresses, differ from those of the central system in two ways. Firstly, input systems are domain specific in that they can only generate hypotheses with respect to a very limited class of distal properties (Meyering 1994). Secondly, modules are informationally encapsulated in that their internal processes are impervious to influence from the rest of cognition, or, to put it more simply than that, that information outside the module is inaccessible from within it (Callebaut 2005; Carruthers et al. 2005).

By contrast, he says, central systems are characterised by properties that are diametrically opposed to domain specificity and informational encapsulation. For Fodor (1983), central systems are Quinean (in that the hypotheses they develop such as scientific theories, for example, are evaluated in light of everything else that a person happens to believe, and therefore their confirmations are global, as opposed to local, phenomena) and isotropic (in that in forming those sorts of scientific theories, everything a person knows may in principle be relevant for the development and confirmation of those ideas) (Meyering 1994).

In light of these characterisations, Fodor (1983) went on to argue that the input systems (which are situated around the mind's periphery) are modular while central processors are not, for according to Fodor (1983), modules must be nine things:

(1) fast in their processing;
(2) mandatory in their operations;
(3) computationally shallow;
(4) inaccessible to the rest of cognition;
(5) associated with fixed neural architecture and as a result
(6) exhibit characteristic and specific breakdown patterns;
(7) have ontogenies that seem to be endogenously determined and follow a characteristic pace and sequencing, as well as being
(8) domain specific and
(9) informationally encapsulated.

There is indeed overwhelming evidence for each of those features in the sensory and linguistic systems (which will be described in due course in Chapter 3), but Fodor has since softened his requirements for modularity somewhat, and now conceives of modules as 'computational system[s] with […] proprietary database[s which] operate[…] to map [their] characteristic inputs onto […] characteristic outputs [and] in the course of doing so, [have] informational resources [that] are restricted to what is in [their] proprietary database' (Fodor 2000: 63). For Fodor (2000), then, the *sine qua non* of modularity is informational encapsulation: cognitive processes are modular precisely because they exhibit encapsulation, and central cognition (which does not), is resolutely ammodular as a result (Carruthers et al. 2005).

Other researchers (e.g. Tooby and Cosmides 1992; Pinker 1997; Scholl and Leslie 1999; Carruthers 2003a, b; 2006a) have increasingly argued otherwise, that the mind is more modular than the Fodorian perspective of modularity allows. In doing so, they have been required to adjust Fodor's (1983) definition of a module somewhat, though; I will elaborate on this in section 3.2.1.2 of this chapter, and even more so in Chapter 3.

Before drawing this section to a close, I would like to point out that the computational modules described above are a very different sort of structure than knowledge meaning (or representational) modules. Whilst knowledge meaning modules are systems of representations and so are, in a sense, inert in that they won't eventuate in behaviour until cognitive mechanisms manipulate their representations, computational modules are the processing devices that do that very thing (Samuels 2000). Computational modules are typically assumed in the literature to be symbol (or representation, it makes more sense to say) manipulating devices that take representations held in knowledge meaning systems as inputs and manipulate them according to formally specifiable rules in order to generate output representations (Pylyshyn 1984; Segal 1996). They are, therefore, importantly different with regards to the functional roles they play in our cognitive economy, as well as with respect to their features.

3.3.1.2 Darwinian modules

Whereas Fodor's (1983, 1998) view of the mind like Chomsky's (1965, 1988) was and is anti-Darwinian in that it is opposed to the idea that modules are naturally selected for (Carruthers and Chamberlain 2000), evolutionary psychologists (e.g. Carroll 1988; Garfield 1991; Barkow et al. 1992; Hirschfield and Gelman 1994; Sperber 1994, 2002; Charland 1995; Segal 1996) and their philosophical associate Pinker (1997) tend to subscribe to the very much Darwinian-like adaptationist view of the Modern Synthesis that's due to Williams (1966) and Dawkins (1976). This views variety between organisms as being due, in part, to their genetic material adapting as a response to their ever-changing environments as they fight for survival in them (Callebaut 2005).

This adaptationist view of the mind is one of the four central tenets of evolutionary psychology (the others being *computationalism, nativism* and *massive modularity*) (Samuels

1998). The first, computationalism, is the view of the mind as an information processing device that can be likened to ‘a computer made out of organic components rather than silicon chips’ (Cosmides et al. 1992: 7). Thus, Darwinian modules are not Chomskyan modules but rather a kind of computational module. The second claim of evolutionary psychology is that much of the human mind’s structure is innate, which rejects the familiar empiricist proposal that the human mind consists of little more than general-purpose learning mechanisms (the view that has been dominant in psychology for most of the twentieth century) in favour of the nativist stance that is associated with Chomsky and his followers (Samuels 2000). The third fundamental hypothesis that evolutionary psychologists endorse is that the mind is massively modular. This is the view that the human mind is largely (or perhaps even entirely) made up of Darwinian modules that comprise both the peripheral systems (which Fodor (1983) argued are modular) and the central capacities (which Fodor (1983) argued are not) (Cosmides and Tooby 1992; Gigerenzer 1994; Leslie 1994; Pinker 1994; Sperber 1994, 2002; Baron-Cohen 1995; Samuels 1998, 2000).

The final tenet of evolutionary psychology, adaptationism*,* meanwhile, is roughly the view that cognitive modularity is the product of evolution (Samuels 2000). Its advocates suggest that human minds were designed by natural selection in order to solve adaptive problems (e.g. ‘evolutionary recurrent problem[s] whose solution promoted reproduction, however long or indirect the chain by which it did so’ (Cosmides and Tooby 1994: 87)) and so for them the modules that make up our minds are simply adaptations that were ‘invented by natural selection during the species’ evolutionary history to product adaptive ends in the species’ natural environment’ (Tooby and Cosmides 1995: xiii). Its advocates wonder why it is uncontroversial that the nonpsychological modules of organisms (for example those in the eyes or the liver) are generally best understood as adaptations (Sperber 2002), but it is controversial to argue that psychological modules may have arisen through evolution as well (Callebaut 2005). I too have asked myself this question.

4. Concluding remarks

The aim of this introductory chapter was not to spell out what modularity in the cognitive sciences is in greater detail than is necessary for a proper understanding of what follows, nor was it to survey the various lines of criticism that have been addressed to any of the notions of modularity discussed here. Rather, this chapter was written to provide the reader with a bit of background about modularity and how different researchers in different research domains define it in order to situate my own work within the wider research context. I’d also like to point out that what was discussed in section 3.3 by no means exhausts the ways in which the term ‘module’ is used in contemporary cognitive science – for a more comprehensive review, one should see Segal (1996) – but what was necessary for this thesis on computational modularity was to draw a distinction between Chomskyan modules (which are said to be systems of mental representations and so will not be paid any more attention to from here on out) and mechanisms that are computer-like in character and are so-called computational modules (which will, of course, be the focus of it). Computational modules can be thought of as being Darwinian (i.e. those that are adaptive and proposed by evolutionary psychologists) or non-Darwinian (those that are proposed by Fodor (1983) and are not). Since this thesis aims to assess the evidence for a) whether phonology can be considered a computational module of the mind in the Fodorian (1983) sense of the word and b) whether it can be conceived as a computational module according to Carruthers (an evolutionary psychologist)’s (2006a) definition of a modularity, Chapter 3 will be devoted to discussing what it means to be a module according to Fodor (1983) and Carruthers (2006a) more thoroughly. The following figure demonstrates the ways in which all of the modules reviewed are related. Of course, the ones I am concerned with in the thesis are the bottom two: non-Darwinian (or Fodorian modules) and Darwinian modules (i.e. those proposed by evolutionary psychologists):

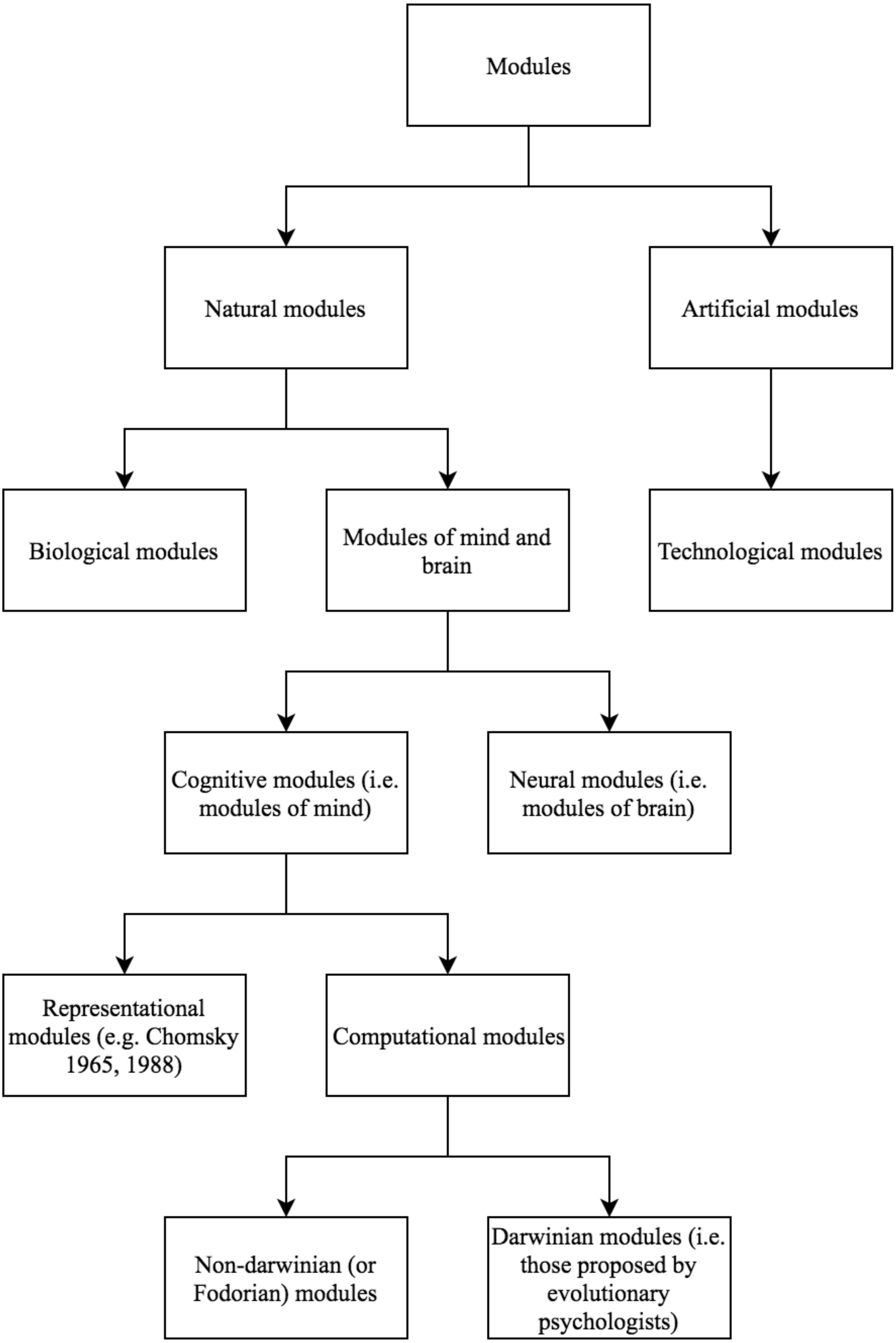

Figure 2.2 The different types of modules and their relationships with one another

Chapter 3: Modest Modularity vs. Massive Modularity

1. Introduction

Recall from Chapter 2 that the modularity hypotheses of today derived, first and foremost, from Chomsky's (1965) work on generative grammar. In *Aspects of the Theory of Syntax* he made two central claims about the architectural structure of the language faculty: (a) that it is a module of the mind distinct from ones responsible for, say, music and mathematics and (b) that it can itself be divided into a number of submodules relating to language meanings, structures and sounds (the semantic, syntactic and phonological modules, respectively) (McGilvray 2005). The thesis was revived by Fodor in 1983 with his publication of the *Modularity of Mind,* in which it was extended to all of the low-level cognitive processes subserved by the input and output systems around the mind's edges. This has launched a debate in the cognitive sciences is ongoing still (Barrett and Kurzban 2006).

Now, whilst Fodor (1983, 2000) made the case for a sort of minimal (or indeed modest) low-level peripheral-systems modularity of mind theory according to which one's mental structure can be divided into modular information processing modules in the input and output systems that reside around the mind's periphery (for example for vision, audition, face-recognition, various motor-control systems and, most relevantly to this thesis, language-processing, and an amodular modular faculty of central cognition (in which concepts are deployed, beliefs formed, inferences drawn and decisions made (Browne 1996)), evolutionary psychology researchers (e.g. Symons 1987; Cosmides and Tooby 1992, 1994; Tooby and Cosmides 1992; Sperber 1994, 1996; Pinker 1997) have argued that a broader notion of modularity is possible. For these researchers, the central systems are modular as well and aren't as different from the peripheral systems as Fodor (1983, 2000) thought (Symons 1987; Tooby and Cosmides 1992; Cosmides and Tooby 1994; Sperber 1994; Pinker 1997).

Many attempts have been made to disprove this proposal (formally known as the massive modularity thesis (Sperber 1994; Samuels 1998; Carruthers 2005)) (e.g. Elman et al. 1996; Deacon 1998; Ramachandran and Blakeslee 1998; Buller and Hardcastle 2000; Fodor 2000; Panksepp and Panksepp 2000, 2001; Quartz and Sejnowski 2002; Buller 2005), and many researchers position their theses somewhere in between those two poles (e.g. Carey 1985; Carey and Spelke 1994; Spelke 1994; Smith and Tsimpli 1996; Hauser and Carey 1998;

Hermer-Vazquez et al. 1999; Cosmides and Tooby 2001). These in-between theses go beyond the scope of this one, though. Thus, this is all I will say about them.

The first section of this chapter provides an overview of Fodor's (1983) modestly modular view and explicates the nine properties he attributes to peripheral modular systems in section 2.1.1 before describing the differences he draws between peripheral and central cognitive systems and explaining why central systems are decidedly nonmodular to his mind in 2.2. In section 3, meanwhile, I map out Carruthers' (2006a) three main arguments for massive modularity (the argument from design in section 3.2.1, from animals in section 3.2.2 and from computational tractability in section 3.2.3), before describing the definition of a 'module' that would follow should those arguments have grounds. The result is a thesis of modularity that is some distance from Fodor's (1983, 2000) (in particular in its definition of modularity which refers to a more restrictive list of features (Prinz 2006) and in its claim that cognitive systems needn't be informationally encapsulated to be modular as Fodor (2000) so vehemently suggests (Barrett and Kurzban 2006)). I explore this in detail in section 3.3, before making some concluding remarks in section 4.

2. Modest modularity

2.1 Fodorian modules

Recall also from Chapter 2 that Fodor (1983) characterises modules by appeal to nine properties as domain specific, innately specified processing systems that deliver 'shallow' (non-conceptual) outputs (Marr 1983), are mandatory in their operations, swift in their processing and isolated from and inaccessible to the rest of cognition, are associated with specific neural structures and so are liable to characteristic patterns of breakdown and develop according to a paced and distinctively arranged sequence of growth (Carruthers 2006a).

Each of these features call for clarification before I describe why the mind is modular around its edges and but not at its centre for Fodor (1983), and so I will comment briefly on the various elements of this account (i.e. on their (i) characteristic breakdown patterns, (ii) fixed association with fixed neural architecture, (iii) mandatory way of operating, (iv) fast processing, (v) 'shallow' outputs, (vi) characteristic pace and sequencing, (vii) domain specificity, (viii) informationally encapsulation and (ix) limited central access) here. To make the exposition as streamlined as possible, the features will be thematically clustered and examined on a cluster by cluster basis along the lines of what Prinz (2006) did in *Is the Mind Really Modular?*. Dissociability and localisability will be explored first in section 2.1.1.1, mandatoriness, speed and superficiality in 2.1.1.2, ontogenetic determinism in 2.1.1.3, domain specificity in 2.1.1.4 and inaccessibility and encapsulation in section 2.1.1.5.

2.1.1 Features of Fodorian modules

2.1.1.1 Dissociabilty and localisability

A functionally dissociable system is one that's operations can be selectively impaired (i.e. damaged independently of the operations of other systems (Robbins 2015)). These selective deficits are often observed as a consequence of focal brain injury and are said to strongly evidence the claim that mental faculties are localised in biological tissue (Prinz 2006). Agnosia (a cognitive disturbance caused by neurological damage affecting a single, sensory modality) is a prime example of dissociability and localisability at work. Its first description was provided by Lissauer in 1890 who made the claim that focal brain lesions can impair either visual or auditory perception leaving the other sensory modalities intact (Lissauer 1890). This view is one that is shared by Mesulam (2000) too, who claims that the visual modality regions adjacent to V1 in the brain are responsible for the processing of the colour, form and motion of visual stimuli while the primary auditory cortex in the left hemisphere of the brain (A1) is responsible for auditory processing and that damage to each of these areas can result in selective impairment.

See Figure 3.1 for an illustration of the brain's functional zones (where 'AA' represents the auditory association cortex, 'ag' the angular gyrus, 'A1' the primary audition cortex, 'B' Broca's area, 'cg' the cingulate cortex, 'f' the fusiform gyrus, 'FEF' the frontal eye fields, 'ins' the insula, 'ipl' the inferior parietal lobule, 'it' the inferior temporal gyrus, 'MA' the motor association cortex, 'mpo' the medical parietooccipital area, 'mt' the middle temporal gyrus, 'M1' the primary motor area, 'of' the orbitofrontal region, 'pc' the prefrontal cortex, 'ph' the parahippocampal region, 'po' the parolfactory area, 'ps' the peristriate cortex, 'rs' the retrospenial area, 'SA' the somatosensory association cortex, 'sg' the supramarginal gyrus, 'spl' the superior parietal lobule, 'st' the superior temporal gyrus, 'S1' the primary somatosensory area, 'tp' the temporopolar cortex, 'VA' the visual association cortex, 'V1' the primary visual cortex and 'W' Wernicke's area):

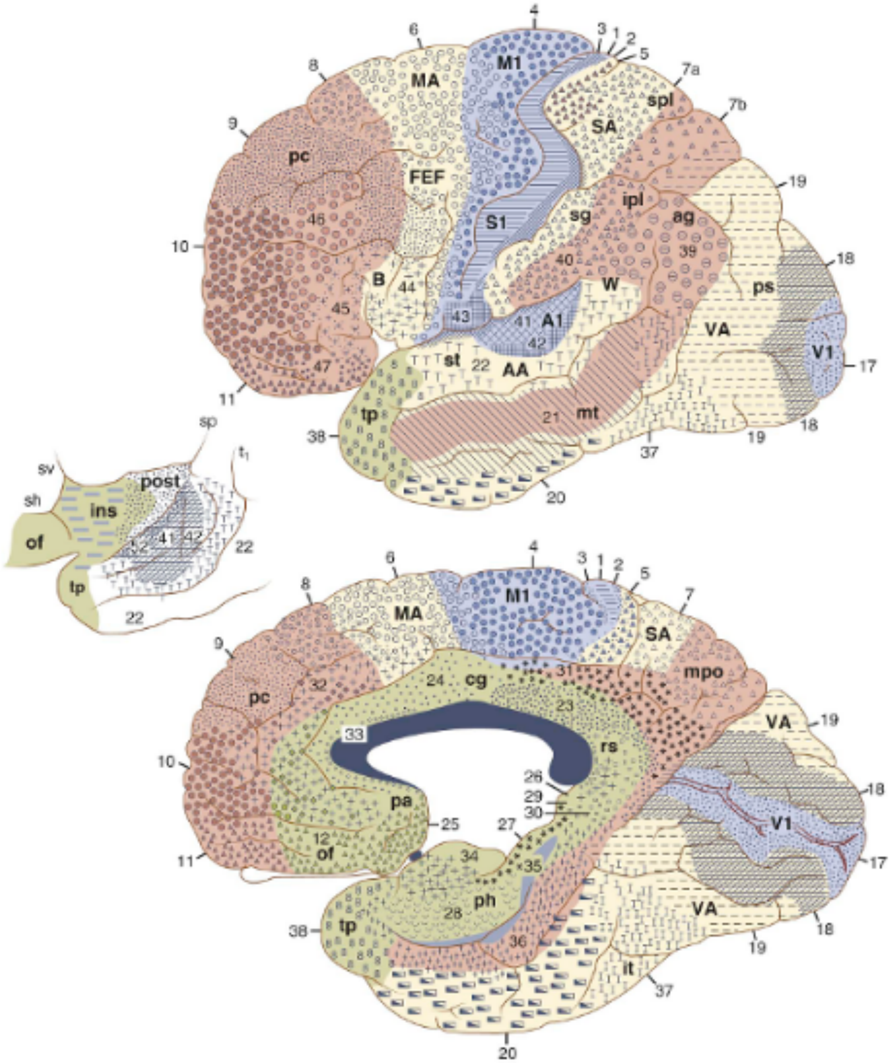

Figure 3.1 Functional zones in the brain (adapted from Daroff et al. 2012: 95)

Adjacent and anterior to the primary visual cortex in the posterior occipital lobe (V1) is the visual association cortex (VA). This region contains several areas including V4, which is specialised for colour recognition and can cause contralateral hemiachromatopsia (a loss of colour perception (or colour blindness)) when damaged (Mesulam 2000). Bilateral damage to the primary auditory cortex (A1) (which is the part of the cerebral cortex that's responsible for processing auditory information and plays a pivotal role in the auditory system (Pickles 2012)) can cause a type of hearing loss called central deafness, meanwhile (Mesulam 2000).

Clinical linguistic research has brought us equivalent cases of selective impairments from focal injury such as agrammatism (the loss of complex syntax), dyslexia (disordered reading and writing ability) and (something that's especially relevant to Chapter 5) anomia (a deficit in word retrieval). These three language disorders have been found in individuals that are otherwise cognitively normal (Damasio et al. 1996), which would suggest that the capacities lost in them are subserved by functionally dissociable mechanisms. Clinical linguistics has also brought us examples of localisable systems – those responsible for auditory production and comprehension most markedly. Broca's aphasia, for example, which affects auditory production but not auditory comprehension (Darley et al. 1975; Marshall et al. 1990; Friedmann 2006; Fogle 2008) is said to be caused by damage to Broca's area (see 'B' in Figure 3.1), the posterior-inferior frontal gyrus of the left cerebral hemisphere or the surrounding vicinity (Naeser and Hayward 1978; Kertesz et al. 1979; Damasio 1989; Kearns 2004; Keller et al. 2009). Wernicke's aphasia, on the other hand (which causes problems in the opposite direction in that it affects auditory comprehension but not auditory production (Albert et al. 1981)) is said to result from damage to Wernicke's area (see Figure 3.1's 'W'), the posterior-superior temporal gyrus of the left cerebral hemisphere (Naeser and Hayward 1978; Fitzgerald 1996; Kolb and Whishaw 2008). This is, of course, suggestive that the mental faculty responsible for language production lies in Broca's region and the one responsible for language comprehension in Wernicke's region, and that damage to those different domains can result in quite different deficits (although one should recall from Chapter 2 that functional and structural modules don't *always* map onto one another as neatly as this).

2.1.1.2 Mandatoriness, speed and superficiality

Something else that merits an explanation here is the mandatory quality that's assigned to Fodor's (1983) modules. At its core, mandatory operation has to do with whether a cognitive system is in any way controlled by consciousness. If a mechanism is uncontrolled by consciousness, which is to say that it is capable of both starting and stopping its operating without any mindful effort to do so, then it is what we call mandatory in its processing (Bargh and Chartrand 1999). Fodor's (1983) central point about mandatoriness, essentially, is that modules automatically and obligatorily process information; that the processing operations observable in input and output systems 'are mediated by automatic processes which are obligatorily applied' (Marslen-Wilson and Tyler 1981: 327). To illustrate, he gives an example from spoken word recognition pointing out that 'you can't help hearing an utterance of a sentence as an utterance of a sentence' (Fodor 1983: 52) and that 'you can't hear speech as noise even if you would prefer to' (Fodor 1983: 53). This, he says, is because the linguistic system's computations take place independently of will; they involuntarily respond to relevant stimuli and so (can be, and indeed have been) likened to reflexes, specified to apply whenever they can (Schwartz 1986).

Further evidence for the mandatoriness of input systems has been found in Stroop-type tasks (Stroop 1935) that require their subjects to name the colours in which written words are presented to them. When the written words are themselves the names of colours, response latencies and accuracies are affected by whether they are the same as or different from the colours in which they presented. Latencies are shorter, and accuracies are higher when these two things are one and the same, for example, participants more quickly and accurately identify a font colour as being pink when it used to scribe the word *pink* than the word *blue* and vice versa. This suggests that visual-word recognition accesses word meaning mandatorily (Coltheart et al. 1999). The same is true for visual-word recognition's access to phonology: naming has been found to be slower and less accurate when a target word is phonologically unrelated to the name of its font's colour than when it is (e.g. if the colour of a target word's font is blue, participants would more quickly identify the word *clue* than its synonym *hint* as there is more of a phonological similarity (compare [b l u:] vs. [k l u:] and [b l u:] vs. [h ɪ n t])) (Coltheart et al. 1999).

Mandatoriness and speed of processing are actually said, by Fodor (1983), to be positively correlated qualities. This is because in shadowing experiments (experiments where subjects repeat what they hear as quickly as they can) he observed that there was only a 250 msec lag on average between the stimuli the subjects were presented with and their responses to them. He related the speed of the systems' analyses to their mandatory means of operating, which he said suggested that the latter was the cause of the former. For Fodor (1983), then, if a system's operations are automatic, the natural consequence is that they must also be fast.

The third feature Fodor (1983) attributes to modular systems is that they produce, what he calls, 'shallow' outputs. The depth of an output is a function of two properties: how much computation is required to produce it (i.e. shallow means computationally cheap (Robbins 2015)) and how specific its informational content is (i.e. shallow means informationally general (Robbins 2015)) he says (Fodor 1983). These two properties are correlated too in that outputs with more specific content are typically expensive (i.e. take much effort) for a system to produce, whereas outputs with more general content can be produced inexpensively (i.e. don't take much effort). For example, it doesn't take the perceptual system much to process shallow concrete words (i.e. words that can be proven by appealing to the physical senses) such as *table* or *chair,* but it does to produce abstract terms (which do not have any physical referents because they aren't tangible) or highly theoretical concepts (from theoretical quantum physics, for example), which are too semantically dense to meet the shallowness criterion (Fodor 1983).

2.1.1.3 Innateness and ontogenetic determinism

Fodor (1980) also argues that modules are ontogenetically determined in that they develop predictably according to a universal maturational timeline in all healthy individuals (Cowie 1999). In a word, they're *innate*, he says. The most obvious example of an innate module of the mind for me to give here is language since extensive research into us humans' capacities for language has provided strong support for the nativist view that humans are born with a genetic predisposition to learn it. Even before the age of five, children can, without having had any formal instruction, comprehend and produce sentences that they have never heard before, and it was, in fact, these very capacities children have for language comprehension and production that led Chomsky (1965) to formulate the 'poverty of the stimulus' argument that served as the foundations for the nativist view he proposed in the 1960s.

In Chomsky's (1965, 1980) eyes, the reason that children can so easily master the complex operations of language is that they have an innate knowledge of the rules and principles that guide them in developing a grammar. In other words, language learning is facilitated by a tendency humans have towards certain structures of language. There exist for us a set of constraints including both formal universals (e.g. principles and parameters) and substantive universals (e.g. lexical categories and features) (Dabrowska 2015), that are hardwired into the brain at birth and manifest without being taught (Crain and Lillo-Martin 1999).

Just as babies naturally develop arms and not wings while they are in the womb, they learn to speak and not chirp when they are out of it, and language emerges on a fixed schedule (children reliable learn single words at around 12 months of age, come to combine them into telegraphic speech at around 18 months, acquire complex grammar at around 24 months and so on (Stromswold 1994)), Fodor (1983) argues. Please note, though, that although Fodor (1983) sees modules as being innate, he doesn't necessarily think that they evolved. I touched on this point in Chapter 2.

2.1.1.4 Domain specificity

A cognitive processor is said to be domain specific if it is only able to compute solutions to a restricted subset of the problems that the mind has to solve as a whole (Cam 1988). In other words, domain specificity restricts the class of information the processor is able to accept as inputs (Cam 1988). As Fodor (1983: 103) puts it, 'domain specificity has to do with the range of questions for which a device provides answers' (i.e. the range of inputs for which it computes analyses). The narrower the range of inputs a system can compute, the narrow the range of questions the system can answer and the narrower the range of those questions, the more domain specific the device (Carruthers 2006a; Samuels 2006).

There are results owing to researchers at Haskins Laboratories that strongly suggest that the perceptual systems responsible for the phonetic analysis of speech are domain specific. Mattingly and Liberman (1988), for example, claim that they are different from the systems responsible for the analysis of nonspeech, as experiments have shown that how a signal sounds to a listener depends on whether the acoustic context indicates that the signal is linguistic or not. The very same signal that is heard as the onset of a syllable when the context specifies that the stimulus is speech may be heard as a chirp when it is isolated from the speech stream. This certainly seemed to be the case in Mattingly and Liberman's (1988) study: when a brief synthesised resonance of changing centre frequency (like one of the two in 3.2 (a)) was presented to participants in isolation, a nonspeech chirp was heard, but when the resonance was the transition (i.e. first formant) of a third-formant trajectory in a three speechlike formant-pattern (like one of two in 3.2 (b)), listeners heard either the consonant-vowel (CV) syllable [da] or the CV syllable [ga] depending on the slope of the transition. The pattern with the falling transition was heard as [da] and the one with rising transition as [ga]:

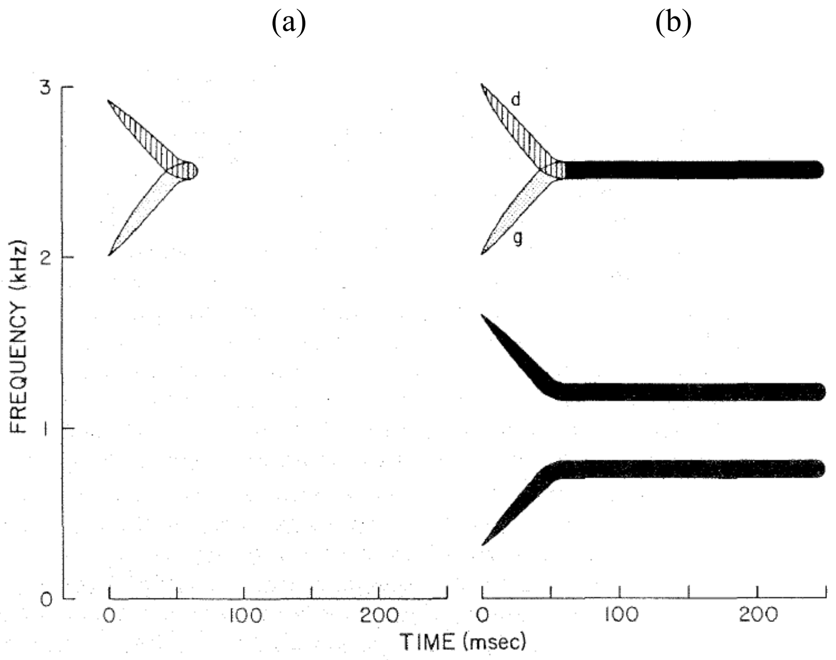

Figure 3.2 A schematic illustrating the domain specificity of speech perception where (a) illustrates isolated resonances and (b) those same resonances producing [d] and [g] in a speechlike context (adapted from Mattingly and Liberman 1988: 69)

Mattingly and Liberman (1990: 502) argue that 'when the resonance is presented in isolation, the auditory module for timbre interprets its center-frequency slope as a chirp, but when this same resonance is in the appropriate context, the phonetic module interprets its slope, together with other parts of the pattern, as a phonetic event'. The rather strong implication of this is that the computational systems that come into play in the perceptual analysis of speech are domain specific in that they are only able to operate on acoustic signals that appear to be part of a speech stream (Liberman et al. 1967; Fodor et al. 1974).

2.1.1.5 Inaccessibility and encapsulation

The final two properties on Fodor's (1983) list, limited central accessibility and informational encapsulation, are closely linked in that they both pertain to the character of information flow across systems, albeit in different directions: while limited central accessibility restricts the flow of information out of a system, informational encapsulation restricts the flow of information into it (Robbins 2015).

Let us look at limited central accessibility first. For Fodor (1983), a system is inaccessible if its inner workings are opaque to introspection – that is, if the intermediate-level representations it computes are inaccessible to consciousness. This, Prinz (2006) points out, is a characteristic that's seen in the peripheral systems, given that despite our ability to make use of the five (traditionally recognised) senses of seeing (opthalmoception), hearing (audioception), taste (gustaception), smell (olfaception), and touch (tactioception) every day, we have no introspective access to how our sensory systems operate. Likewise, although we have tacit knowledge of our native language (i.e. native speaker competence) and the ability to produce it, we do not understand the operations of each and every part of our linguistic system (Davenport and Hannahs 2010), note.

Informational encapsulation, on the other hand, concerns the information a cognitive mechanism has access to. It has been defined by Fodor (1983: 69) as 'the claim that the data that can bear on the confirmation of perceptual hypotheses includes, in the general case, considerably less than the organism may know'. An informationally encapsulated system is one that 'ha[s] access to only some of the information available to the mind as a whole' (Chien 1996: 1), that operates in isolation from any information that's stored beyond it, sensitive only to information stored within the mechanism itself, (in a proprietary database, say) and any input it receives (Fodor 1983; Prinz 2006; Robbins 2015). Being informationally encapsulated, modules neither consult one another, nor are they 'susceptible to influence from information from higher levels, levels at or above that at which they deliver outputs' (Currie and Sterenly 2000: 147) for example general memory in central cognition (Collins 2005). They simply operate quickly and automatically on what they take as input, and can do so precisely because their processes' databases are so well defined (Collins 2005) – this is a point I will come back to in section 2.2.

The encapsulation of perceptual systems 'is evidenced directly by the fact that perception does not change when it conflicts with belief, as in cases of visual illusion', Currie and Sterenly (2000: 148), argue. And that certainly seems to be the case. Consider this example from Fodor (1983: 66-67):

> When you move your head, or your eyes, the flow of images across the retina may be identical to what it would be were the head and the eyes to remain stationary while the scene moves. So: why don't we experience apparent motion when we move our eyes? Most psychologists now accept one or another version of the "corollary discharge" answer to this problem. According to this story, the neural centers which initiate head and eye motions communicate with the input analyser in charge of interpreting visual stimulations. Because the latter system knows what the former is up to, it is able to discount

> alterations in the retinal flow that are due to the motions of the receptive organs.
>
> Well, the point of interest for us is that the visual-motor system is informationally encapsulated. Witness the fact that, if you (gently) push your eyeball with your finger (as opposed to moving it in the usual way: by an exercise of the will), you *do* get apparent motion. Consider the moral: when you voluntarily move your eyeball with your finger, you certainly are possessed of the information that it's your eye (and not the visual scene) that is moving[, b]ut this explicit information, available to you for (e.g.) report, is *not* available to the analyzer in charge of the perceptual integration of your retinal stimulations. That system has access to corollary discharges from the motor center *and to no other information that you possess.*

Since a cognitive processor is one that has access to nothing more than the information stored within the local structures that subserve it, it is, of course, the *lack* of access to knowledge about what the finger is doing that demonstrates the encapsulation of the visual-motor system in this example. The visual mechanisms involved in this exercise are simply not designed to take any externally caused movement of the eyeball or any knowledge of that into account (Barrett 2005).

The Müller-Lyer (Day 1989) illusion (which also has to do with visual perception) (see Figure 3.3) is also said to support Fodor's (1983) claim that input systems are modular. In this illusion, two equal-lengthed parallel lines are flanked by arrows pointing inwardly in one case and outwardly in the other, but although the lines are of equal length, the one with outward-pointing arrows consistently appears shorter to those that perceive them. Even when people are made aware that the two lines are the same length, the lines still look as though they are different lengths to them; they cannot use their knowledge that they are not to alter their visual perception that they are. This, Fodor (1983) says, suggests that visual perceptive processes are encapsulated from module-external information (such as dimensions, in this case):

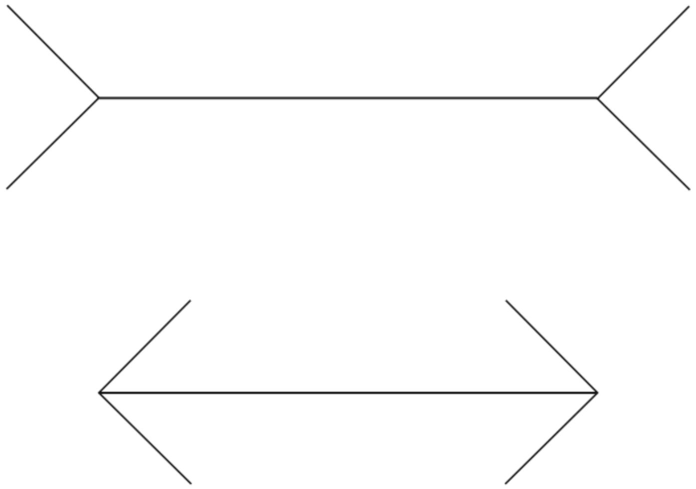

Figure 3.3 A Müller-Lyer illusion with typical central shaft and inward and outward fins

A similar effect might be observed if somebody asked one of their friends to remove something that was stuck in their eye, for it's likely that they would blink as their friend's fingers approached it in spite of trusting that their friend wouldn't hurt them (Chien 1996). If this effect was observed, it could count as strong evidence to suggest that the eyelid reflex is encapsulated, sensitive only to motions around the eye and not one's beliefs about other people (Chien 1996). It is for this reason that 'informational encapsulation' is sometimes used interchangeably with 'cognitive impenetrability': what one believes does not (and cannot) make a difference to how modules work.

Now although Fodor didn't distinguish the nine properties described above with respect to weight or priority in 1983, in later essays (e.g. Fodor 2000) he emphasised informational encapsulation at the exclusion of others, as the defining feature of modularity (Garfield 1987; Applebaum 1998).

2.2 What is and isn't modular for Fodor

We obviously know now from Chapter 2 (and the first section of this one) that Fodor (1983) wrote a bold, two-part thesis about the structure of the mind (Wilson 2005). His first claim is a positive one, that part of it (i.e. its input systems (those responsible for lower-level cognitive functions such as perception and language)) is modular while the second is negative, that central cognition (whose systems are responsible for higher-order cognitive functions such as belief fixation and practical reasoning) is not (Robbins 2015).

Fodor (1983) characterises some of the components of psychological systems by analogy to the organisation of computers (or, more specifically, Turing machines), which are, for Fodor (1983: 39), ‘as general [an example of a central processing unit] as any kind of computer can be’. Turing machines are, according to Fodor (1983: 39), informationally encapsulated devices in that ‘the sole determinants of their computations are the current machine state, the tape configuration, and the program [with] the rest of the world being quite irrelevant to the character of their performance’. Organisms ‘are forever exchanging information with their environments, and much of their psychological structure[s are] constituted of mechanisms which function to mediate such exchanges’ (Fodor 1983: 39), meanwhile, so if we are to model anything in cognitive psychology on Turing machines, it must not be the mind as a whole, but the input systems that are ‘embedded in a matrix of subsidiary systems [(i.e. transducers)] which affect their computations in ways that are responsive to the flow of environmental events [and function] to provide the [input systems] with information about the world’ (Fodor 1983: 39).

These subsidiary systems (or transducers) Fodor (1983) speaks of on page 39 are helpfully distinguished from the input systems and central processors in the trichotomous taxonomy he provides on page 42:

> Input systems function to get information into the central processors; specifically, they mediate between transducer outputs and central cognitive mechanisms by encoding the mental representations which provide domains for the operations of the latter. […] Whereas transducer outputs are most naturally interpreted as specifying the distribution of stimulations at the ‘surfaces’ (as it were) of the organism, the input systems deliver representations that are most naturally interpreted as characterizing the arrangement of *things in the world.* Input analyzers are thus inference-performance systems within the usual limitations of that metaphor. Specifically, the inferences at issue have as their ‘premises’ transduced representations of proximal stimulus configurations, and as their ‘conclusions’ representations of the character and distribution of distal objects

In other words, there are three different types of mental mechanism for Fodor (1983): transducers, input systems, and central systems. Transducers lie at the interface between the mind and the world producing symbolic mental representations as output from the physical, non-symbolic input they find externally (Cain 2013, 2016); that is, they convert the energy that impinges on the body’s sensory surfaces into something that’s computable by the input systems (Robbins 2015). The retina (i.e. the light-sensitive tissue that lines the back of the eye), for example, is a transducer that in response to being sensorily stimulated by light, produces symbolic output that represents the light's properties (e.g. its intensity, propagation direction, frequency or wavelength, to name a few (Buser and Imbert 1992)) (Cain 2013).

Input systems, on the other hand, are mechanisms that function to 'present[…] the world to thought' (Fodor 1983: 40) by taking the outputs of the sensory transducers as input and transforming them into representations of their distal causes as output by means of computation (Cain 2013). These output representations are then passed on to the central system which decides on these bases what decisions are to be made about the external world (Cain 2016).

As an example, let's consider what happens when one smells something sweet. First of all, sensory information is conveyed to the central nervous system (CNS) and perceived in four steps (as exemplified in Figure 3.4): (1) stimulation (where 'a physical stimulus impinges on a sensory neuron or an accessory structure' (Raven 2006: 1104)); (2) transduction (where 'the stimulus energy is used to produce electrochemical nerve impulses in the dendrites of the sensory neuron' (Raven 2006: 1004)); (3) transmission (where 'the axon of the sensory neuron conducts action potentials along an afferent pathway to the CNS' (Raven 2006: 1104)) and (4) interpretation (where 'the brain creates a sensory perception from the electrochemical events produced by afferent stimulation' (Raven 2006: 1104)). It is fair to say that we actually smell (as well as see, hear, touch and taste) with our brains as opposed to with our sensory organs then.

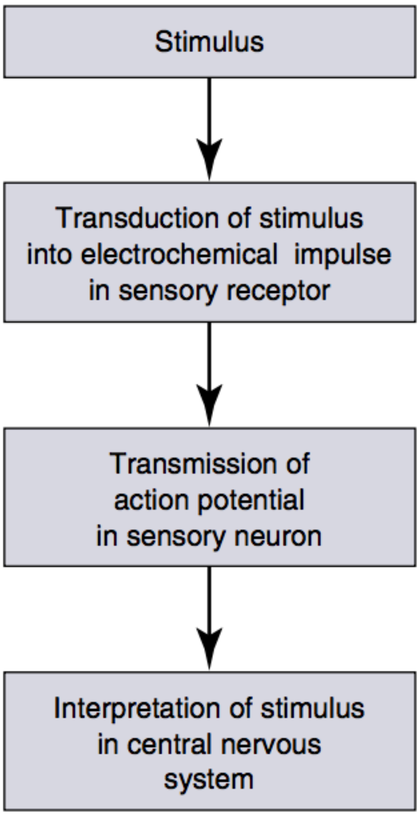

Figure 3.4 The path of sensory information (Raven 2006: 1104)

When somebody smells something sweet, the chemoreceptors or ciliated neurons located in the linings of their nasal passages transduce (i.e. respond to) chemical substances present in their environment (in particular Glycophore d = 2.6 compounds which are made up of two oxygen atoms that are separated by a diagonal distance of d = 2.6 Angstrom and attached to two carbon atoms in a cis- configuration (Fulton 2005)) in order to generate electrochemical nerve impulses as output. These are then taken as input by the neurons in the olfactory input system which transmit impulses through their axons to the brain for interpretation via the olfactory nerve:

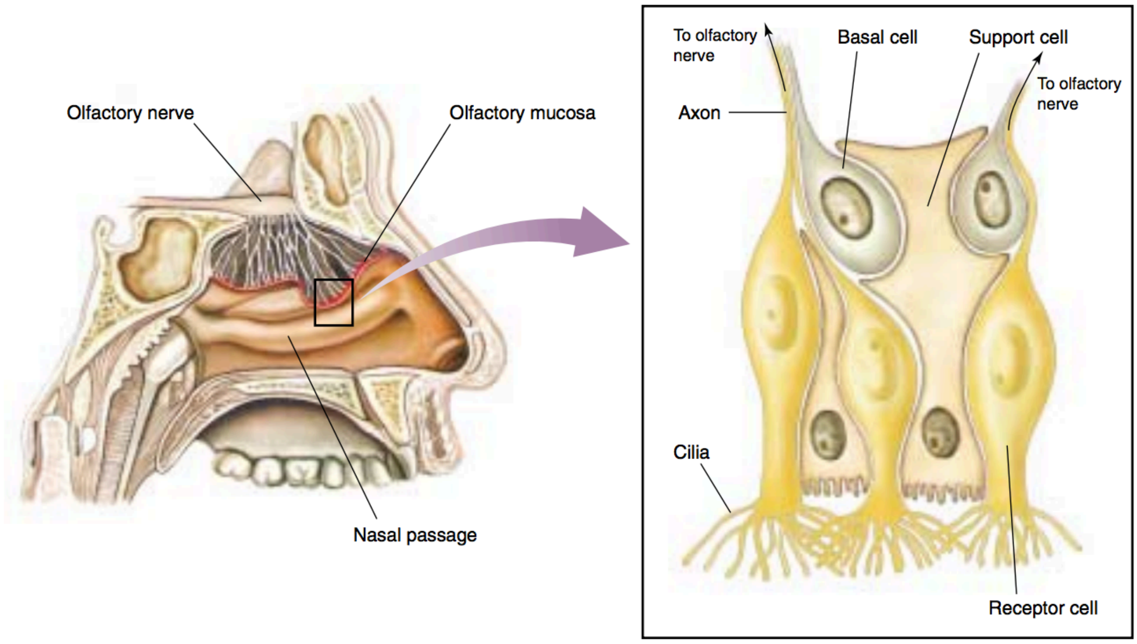

Figure 3.5 Smell (Raven 2006: 1110)

The central system might then use the information that's been made available to it about something smelling sweet together with one's memory of past events that something that smelled sweet tasted sweet too to fixate the belief that the same correlation will be found in this instance; that whatever they have in front of them will taste as sweet as it smells.

The olfactory system is only one of six input systems, however, for there are input systems that correspond to each of the other traditionally conceived senses as well (i.e. the visual system for seeing, the auditory system for hearing, the gustatory system for tasting and the somatosensory system for touching) as well as the one for language (Cain 2013) which functions to generate from the auditory and visual information associated with the spoken and signed modalities of language, respectively, their underlying semantic, syntactic and phonological structures. I will have more to say about this, peripheral modularity, that is, in section 2.2.1.

2.2.1 Peripheral modularity

Like transducers, input systems are reflexive and automatic, and like cognitive mechanisms, they are inferential and computational, Fodor (1983) claims (Applebaum 1998). They are also informationally encapsulated, he says, in that they operate to map characteristic inputs onto characteristic outputs using the informational resources stored in their proprietary databases only. This makes them modular by definition (Fodor 2000).

Because of the arbitrary constraints on input systems' access to information, they are said to be 'irrational', and because they operate blindly in the sense that they do so without being able to access all that the organism knows they are said to be 'dumb'. Irrational and dumb aren't actually bad things for modules to be though, it turns out, for it is precisely because of the restrictions of the data that's available to modules that they are able to process things as quickly and mandatorily as they do (Carruthers 2005). On pages 64 through 71 Fodor (1983) emphasises these points:

> [T]he point of the informational encapsulation of input systems […] is to restrict the number of *confirmation relations* that need to be estimated as to make perceptual identifications fast. (Fodor 1983: 71)
>
> The informational encapsulation of the input systems is […] the essence of the analogy between the input systems and the reflexes. (Fodor 1983: 71)
>
> Because [the input systems'] processes are automatic, you save computation (hence time) that would otherwise have to be devoted to deciding whether, and how, they ought to be performed. (Fodor 1983: 64)

Central systems, on the other hand, 'operate without antecedently established constraints on the information they are able to recruit in the course of their operations' (Applebaum 1998: 319) and are therefore rational, smart, but most importantly amodular (due to their lack of informational encapsulation), in Fodor's (1983, 2000) eyes.

2.2.2 Central nonmodularity

Fodor's (1983, 2003) argument for the nonmodularity of central systems has been broken down by Robbins (2015: 13) as follows:

1. Central systems are responsible for belief fixation.
2. Belief fixation is isotropic and Quinean.
3. Isotropic and Quinean processes cannot be carried out by informationally encapsulated systems.

Hence (from 2 and 3):

4. Belief fixation cannot be carried out by an informationally encapsulated system.

But:

5. Modular systems are informationally encapsulated.

Hence (from 4 and 5):

6. Belief fixation cannot be carried out be a modular system.

Hence (from 1 and 6):

7. Central systems are not modular.

Two terms call for explication in this argument (*isotropy* and *Quineanness*), and so I will do that here. Both have to do with the confirmation of scientific hypotheses (Wilson 2005). Isotropy, as defined by Fodor (1983: 105), refers to the epistemic interconnectedness of beliefs in that 'everything the scientist knows is, in principle, relevant to determining what else he ought to believe'. Carruthers (2003a: 76) gives the following example:

> As an illustration of the supposed holism of belief, consider an episode from the history of science. Shortly after the publication of *The Origin of Species* a leading physicist, Sir William Thompson, pointed out that Darwin couldn't just assume the long time-scale required for gradual evolution from small differences between individual organisms, because the rate of cooling of the sun meant that the Earth would have been too hot for life to survive at such early dates. Now we realize that the Victorian physicists had too high a value for the rate at which the sun is cooling down because they were unaware of radioactive effects. But at the time this was taken as a serious problem for Darwinian theory – and rightly so, in the scientific context of the day.

To say that scientific confirmation is Quinean, on the other hand, is to say that 'the degree of confirmation assigned to any given hypothesis is sensitive to properties of the entire belief system' (Fodor 1983: 107), that 'the shape of our whole science bears on the epistemic status of each scientific hypothesis' (Fodor 1983: 107). Both isotropy and Quineanness preclude the encapsulation (and therefore modularity) of central systems since a system's possession of those two features requires it to have 'potentially unlimited access to the contents of central memory' (Prinz 2006). Put in slightly different terms, isotropy and Quineanness are global properties, and since globality rules out encapsulation, so too do central processes (which means they cannot be modular) (Prinz 2006).

For many, Fodor's (1983) argument is a difficult one to resist, which is unsurprising, really, given its logicality. His main points are these: first, that there is a strong negative correlation between globality and encapsulation, second that there is a strong positive correlation between encapsulation and modularity, and from those two points a third, that there is, therefore, a strong negative correlation between *globality* and modularity (Robbins 2015). For Fodor (1983), the more global a process is, the less modular the system is that performs it (Robbins 2015).

There are three ways, then, that Fodor's (1983) conclusion can be countered: the first way is to deny that central processes are global, the second is to deny globality and encapsulation are strongly negatively correlated and the third to deny that encapsulation and modularity are strongly positively correlated (Prinz 2006). The third option (denying that modularity requires encapsulation) is the approach Carruthers (2006a) takes. In *The Architecture of the Mind* Carruthers (2006a) draws a distinction between two kinds of encapsulation: 'narrow-scope' encapsulation (which is seen in systems that cannot draw on information beyond their system boundaries during the course of their processing and corresponds to Fodor's (1983) use of the term encapsulation) and 'wide-scope' encapsulation (which is seen in systems that indeed do have access to exogenous information during the course of their processing, but cannot access everything all at once). This, wide-scope encapsulation, that is, is a much weaker sense of encapsulation than the one Fodor's (1983) proposed. As such, it allows for the central systems to be seen as modular enterprises after all.

3. Massive modularity

Researchers in the cognitive science community have gone a step further than Fodor (1983, 2000) by claiming that the mind is wholly, or at the very least massively, composed of modules (Cosmides and Tooby 1992, 1994; Tooby and Cosmides 1992; Sperber 1994, 1996, 2002; Pinker 1997; Gallistel 2000; Barrett 2006; Barrett and Kurzban 2006). There are a number of good reasons, then, for thinking that this is so, and so I will begin this section by shedding light on some of these for they will bring to bear on later discussion. In sections 3.1.1 through 3.1.3 I will look briefly at some of the developmental, pathological and experimental evidence for the mind having such an architecture before honing in on evolutionary psychologists' principal theoretical argument for massive modularity in section

3.1.4, which has to do with evolution. Section 3.2 will be devoted to the discussion of Carruthers (2006a) arguments for massive modularity (the argument from design in section 3.2.1, the argument from animals in section 3.2.2 and the argument from computational tractability in section 3.2.3) and section 3.3 his definition of a module, for it differs from Fodor's (1983, 2000) in quite a number of ways.

3.1 Evidence for modularity

3.1.1 Developmental evidence

A variety of arguments (that are themselves of varying strengths) have been put forward to support some form of (or at least, some facets of) a massively modular view of the mind (Carruthers 2003a). The first set (and indeed the set to be studied in this subsection) have to do with development.

Developmental psychologists now generally agree that cognitive development is a process that proceeds at different speeds in different domains (such as naïve physics, naïve psychology, and, for Atran (2002), naïve biology, for example) (Carruthers 2004). Children's competence in at least some of these domains is observable very early on in infancy (Carruthers 2003a; Caruthers 2004), in some cases as young as four months old (Carruthers 2004). Evidence for early competence in contact mechanics (and therefore naïve physics) has been found too, and so has evidence for early competence in social understanding (ergo naïve psychology) (Spelke 1994; Baillargeon 1995; Woodward 1998; Phillips et al. 2002).

Cognitive scientists have argued that children's abilities to know so much, so fast, with so little information available to them cannot be explained without deploying some sort of 'poverty of the stimulus' argument like Chomsky's that was put forward in support of the innateness of linguistic knowledge. According to Carruthers (1992, 2003a, 2004), Leslie (1994), Spelke (1994), Baron-Cohen (1995) and Dwyer (1999), among others, it is hard to see how children can acquire so much knowledge at such young ages with only general-learning mechanisms to help them. There must, therefore, be an innately channelled learning module for knowledge acquisition in each of the different domains they say, much like there is a learning module for the acquisition of linguistic knowledge.

3.1.2 Pathological evidence

Related arguments for some domains being dissociable from others come from psychopathological damage studies (Shallice 1998; Tager-Flusberg 1999). During development, theory of mind can be damaged while physical/spatial thinking is not, for example, and the reverse is possible too, Carruthers (2004) points out.

The first case, actually, is what we see in autism (a developmental condition in which people show a selective impairment in the naïve psychological domain in that they can find it difficult to understand the mental states of themselves and other people but can be of normal intelligence otherwise (Baron-Cohen 1995)). The second case, on the other hand, is something seen in children with Williams' syndrome (a developmental disorder in which people have difficulties in the domain of practical problem solving (which of course, implicates naïve physics) but not necessarily anywhere else (Karmiloff-Smith et al. 1995; Mervis et al. 1999)).

There has also been evidence to suggest that the naïve biological domain can be subject to dissociable damage, particularly so from stroke victims who have category-specific semantic impairments in the realm of animate vs. inanimate things (Warrington and McCarthy 1983; Warrington and Shallice 1984; Sartori and Job 1988; Job and Surian 1998; Atran 2002). Warrington and McCarthy (1983) and Warrington and Shallice (1984) were among the first people to report this: Warrington and McCarthy (1983) described a patient who had suffered a left hemisphere stroke and had impaired production and comprehension performance for non-living objects but not living ones while Warrington and Shallice (1984) described the opposite pattern of breakdown in four patients with the herpes simplex encephalitis virus.

3.1.3 Experimental evidence

There isn't as much experimental evidence for modularity of mind as there could be, for most of its proponents are theoretical, as opposed to empirical, researchers. What little evidence there is, however, is strongly suggestive that the mind is indeed modular (Carruthers 2004).

The first piece of evidence concerns the existence of a geometric module in rats. In 1986, Cheng trained rats to search for food in rectangular enclosures. On each trial, food was put in a particular corner, and 75 seconds after being shown the food, the disorientated rats were allowed to search for it. When there were no other cues, rats searched the correct corner and its geometrically equivalent corner (the one diagonally opposite). What was most interesting, however, is that the rats continued to confuse geometrically equivalent locations even in the presence of highly salient clues that could disambiguate the geometric information and that they would well recognise in other circumstances (such as different odours and colours). The rats' inability to integrate geometric information with information of other kinds led Cheng (1986) to suggest that the processing of local spatial geometry is modular, according to Fodor (1983)'s definition of the word, as to him the study suggested that geometric processing in rats was cognitively impenetrable (or informationally encapsulated, if you prefer).

Since then, similar paradigms of research have been conducted on humans (Cheng and Newcombe 2005). Hermer and Spelke (1994, 1996), for example, extended Cheng's (1986) work to human children and found that between the ages of two and four children behaved identically to rats when they were disorientated, splitting their choices evenly between the correct corner and the diagonally opposite corner of a rectangular room regardless of a nongeometric landmark (a blue wall) to relocate an object hidden in one of its four corners. Using the data from Hermer and Spelke (1994, 1996) (and indeed their own) as evidence, Hermer-Vazquez et al. (1999) went on to sketch out how they thought children might reorient. Until children acquire spatial language (at around six years of age), they will, like rats, reorient themselves on the basis of spatial representations within a geometric module of mind, they explain.

There is also evidence to suggest that Pavlovian (1955) associationist models of simple conditioning are 'subserved by a special-purpose computational system that is designed to predict varying temporal contingencies' (Carruthers 2004: 306). In these sorts of model, a behaviourally neutral stimulus called the conditioned stimulus is repeatedly paired with a motivating stimulus called the unconditioned stimulus until the subject responds to the former in a manner that demonstrates their anticipation of the latter (Gallistel and Gibbon 2001). In Pavlov's experiments which investigated the conditioning and learning by association, recall, a dog that was exposed to the ring of a bell (the conditioned stimulus) whenever it was fed (the unconditioned stimulus) over and over again, learnt to associate the sound of the bell with the expectation that food would come and would salivate upon hearing it.

Gallistel and Gibbon's (2001) argument for there being an information processing computational module that subserves associationist conditioning is as follows: neither the delay between stimuli and reinforcements nor the ratio of reinforced to unreinforced presentations of conditioned stimuli have been found to affect rates of acquisition. And, while neither of these things can be understood from a perspective that sees learning as the mere building of associative connections (i.e. Hebbian synapses), they can be easily explained within a computational model which assumes that what animals really do is estimate likelihoods and calculate rates of return (Gallistel and Gibbon 2001).

3.1.4 Evidence from evolutionary psychologists

The fourth and final set of arguments (to be discussed in section 3.1, at least) for massive modularity are evolutionary in nature. According to evolutionary psychologists such as Tooby and Cosmides (1995), Fiddick et al. (2000) and Cosmides and Tooby (2001), biological systems (which they take to include the mind as well) characteristically evolve by becoming more modular in response to evolutionary pressures from their environment, by 'bolting on' additional structures that are specialised to perform particular tasks and solve certain problems (Carruthers 2004; Carruthers 2006a). The anti-modular 'general-purpose computer' model of the mind cannot be correct, in their eyes, for no such computer could evolve in the way that the mind seems to.

There is indeed evidence to suggest that this is the case: computational simulations by Kashtan and Alon (2005) and Kashtan et al. (2007), for example, have shown that networks evolve modularity and evolvability in environments that are subject to change, while evolution in environments that are unchanging produces nonmodular networks that are comparatively slower to adapt (Kashtan and Alon 2005; Kashtan et al. 2007). Follow up studies undertaken by Parter et al. (2007) further support this thesis that it is an organism's changing environment that generates its modular structure. They found that the modularity of bacterial metabolic networks is positively correlated with the frequency and degree to which their environments change.

The argument is summarised by Tooby and Cosmides (1995: xiii-xv) as follows:

> just as the body contains distinct morphological adaptations that evolved to perform distinct physiological functions, the mind must contain distinct "mental organs," which evolved to solve a Vast Number of adaptive problems that required distinct behavioral solutions (Symons 1992, p. 142). Since the behavioral solution to any one of these problems wouldn't have transferred to any of the other problems […] each adaptive problem would have selected for its own specialized cognitive adaptation. Thus, "our cognitive architecture resembles a confederation of hundreds of functionally dedicated computers (often called modules

In their eyes, when we need a new skill, a new system simply evolves to subserve that. I will come back to this point in sections 3.2.1 and 3.2.2, as this has to do with Carruthers' arguments for massive modularity.

3.2 Carruthers' arguments for massive modularity

In his 2006 publication *The Architecture of the Mind*, Carruthers offers a clear and comprehensive defence of the idea that the human mind is massively modular. He begins by providing a definition of modularity that's appropriate to the thesis that central cognition is modular too, for unlike Fodorian (1983) modules that can be characterised by nine features, Carrutherian (2006a) modules are more simply than that function-specific automatic processing systems which are independent of one another and whose internal operations are largely inaccessible to the rest of cognition. The result is a notion of modularity that is dramatically different from Fodor's (1983) – it is much closer to the use of notion in the biological research domain and even closer to its use by researchers in computing (a point I will return to in section 3.3). In the book's first three chapters Carruthers (2006a) put forward three arguments for massive modularity (the argument from design, the argument from animals and the argument from computational tractability). Each of those will be addressed in turn here.

3.2.1 The argument from design

3.2.1.1 Biological modules

The first (and arguably the most developed) of Carruthers' (2006a) arguments for massive modularity (the argument from design) follows the lead of Simon (1962), who said that systems are optimally designed if they are modular, for the modular organisation of complex systems means that they are:

> constructed hierarchically out of dissociable sub-systems (each of which is made up of yet further sub-systems), in such a way that the whole assembly can be built up gradually, adding sub-system to subsystem; where the properties of sub-systems can be varied independently of one another; and in such a way that the functionality of the whole is buffered, to some extent, from changes of damage occurring to the parts (Carruthers 2006a: 12)

He told the tale of two watchmakers, Tempus and Hora, to illustrate this point. Tempus and Hora both made watches from myriad parts and were interrupted frequently by phone calls as they worked, Simon (1962) said. Hora's designs were no less complex than Tempus' were, but while Hora designed his watches as decomposable systems made up of modules, Tempus did not, and so each and every time Tempus was interrupted by the telephone and had to set aside his work to answer it, he had to come back and reassemble the whole watch from scratch (Simon 1962). By contrast, Hora designed small stable subassemblies (or micro-assemblies) that could be fashioned into larger stable subassemblies, and so when he was interrupted by the telephone, only the last unfinished subassembly had to be reassembled; his earlier work, meanwhile, was of course preserved (Simon 1962).

What made Tempus' watch design so unstable in the end was not the number of parts it had but the interdependencies among them (Langlois 1999). This is because in a nondecomposable system (like Tempus'), the successful operation of one part depends on the successful operations of all others, meaning that when a system is missing parts, it is no longer functional (Langlois 1999). In a decomposable system (like Hora's), on the other hand, the proper working of a part depends more on the parts within its subassembly than the parts outside of it, which means that it may well be able to operate as it should even if some of the system's other parts are missing (Langlois 1999).

The modularity of technological systems then, by limiting the scope of interactions between elements of tasks, can shorten their designs' developmental times, as it eliminates the need for pre-assembly operations Wilhem (1997) points out. It can also reduce their cost of production (Garud and Kumaraswamy 1995; Sanchez and Mahoney 1996; Muffatto 1999; Cusumano and Nobeoka 1998), economise their scale and scope (Pine 1993; Friedland 1994; Garud and Kumaraswamy 1995; Sanchez 1999) and increase their number of compatible suppliers (Langlois 1992, 2000; Langlois and Robertson 1992; Garud and Kumaraswamy 1993; Morris and Ferguson 1993; Reed 1996; Baldwin and Clark 1997; Sanderson and Uzumeri 1997). The main thing that artificial systems (like technological ones) have in common with natural systems (like biological ones), though, is that their modularity allows their structures to evolve more quickly and easily than they would have been able to had their architecture not been modular. Just like technological systems have to be modularised in order that they can evolve through new parts being added and old ones subtracted, debugged, improved or updated without running the risk of errors being introduced elsewhere (Carruthers 2006a), modularity in complex biological systems is essential for their biological evolution through natural selection (Wagner and Altenberg 1996; Bolker 2000; Raff and Raff 2000; Wimsatt and Schank 2004; Carruthers 2006a; Futuyma 2009; Epinosa-Soto and Wagner 2010 (see also, Carruthers (2006a: 21-22)):

> The basic reason why biological systems are organized hierarchically in modular fashion is a constraint of evolvability. Evolution needs to be able to add new functions without disrupting those that already exist; and it needs to be able to tinker with the operations of a given functional sub-system – either debugging it, or altering its processing in responses to changes in external circumstances – without affecting the functionality of the remainder.

Simon's (1962) argument is really a one from *design,* then, whether the designer is a human engineer (as in the case of technological systems) or natural selection (as in the case of biological ones) (Carruthers 2006a).

3.2.1.2 Is the mind a biological module?

Carruthers (2006a: 13) argues that a belief in massive modularity is 'well nigh ubiquitous across the biological sciences' and that while not everyone uses the word 'modularity', others are used to denote the same concept (Nijhout (1991) uses 'autonomy', Maynard-Smith and Szathmáry (1995) and Kirschner and Gerhart (1998) use 'compartmentalisation', Larson and Losos (1996) use 'individualisation' and West-Eberhard (1996) uses 'sub-unit organisation'). There is indeed a great deal of evidence from the field of biology, suggesting that complex biological systems are modular. This has found to be true for many levels of the organism such as genes, cells, cellular assemblies, whole organs, organ assemblies, the organism itself and even multi-organism units (Seeley 1995; West-Eberhard 2003; Schlosser and Wagner 2004; Callebaut and Rasskin-Gutman 2005). Consider the evolution of orchids, for example, who have adapted through natural selection to promote reproduction (Schiestl et al. 2000).

Orchids reproduce through pseudocopulatory pollination (so-called because the behaviours of the individuals involved mimic copulation even though there is no actual sexual activity involved (Schiestl et al. 2000)). The pollinator sees the flower as a potential mate and attempts to engage in coitus. In doing so, its body makes contact with the viscidium of the flower, and pollinium is pulled from the anther, which is connected to the viscidium by something called the caudicle (Schiestl et al. 2000). The pollinator then leaves and approaches another flower of the same species to copulate, depositing the pollinium to the stigma of the second flower and thereby completing the pollination process (Schiestl et al. 2000).

In order to increase its chances of pollination, the orchid may mimic a potential female mate for the pollinator visually (van der Pijl and Dodson 1966). A prime example of this can be seen in the hammer orchid (*Drakaea*), which is highly modified in that its labellum resembles a female *Thynnid* wasp in both shape and colour to lure in potential partners to pick up and deposit its pollen to another *Drakaea* (Groom and Lamont 2015).

Orchids are also known to exploit the attraction of male insects to female sex pheromones by emitting a scent that mimics that of a potential mate (Tan and Nishida 2000). The early spider orchid (*Ophrys sphegodes*), for example, which is pollinated by *Adrena nigroaenaea* males, secretes chemicals that smell similar to the sex pheromones of a female *Andrena nigroaenaea* from glands called osmophores in order to attract them (Shiestl et al. 2000).

Note that in each of the cases presented here, only particular parts of organisms undergo adaptations: in *Artemia salina*, it is the osmoregulation system, in *Drakaea* it is the labellum and in *Ophrys sphegodes,* it is the osmophores. This is because of the organisms' modular organisations: the modularity of organisms allows evolution to occur as it allows the functions of units to be altered without altering the functioning of other units (Riedl 1978; Gilbert et al. 1996; Raff 1996).

This is also the case in animals as well as plans. Nobody supposes that there could be a single general-purpose sensory organ for fulfilling all of the five sensory functions. On the contrary what we find are five distinct organs that have been shaped by natural selection to fulfil each of the five sensory functions – the eyes for seeing, the ears for hearing, the nose for smelling, the tongue for tasting and the skin for touching (Carruthers 2004). By the same token, nobody expects to find a single general-purpose organ that fulfils both the functions of the heart and lungs or the cardiovascular system and the respiratory system. Rather, the cardiovascular system is made up of the heart and blood vessels (arteries, capillaries and veins) and the respiratory system the nose, mouth, pharynx, larynx, trachea, bronchi and lungs (Carruthers 2004). The same, then, should surely be true of the mind, Carruthers argues, if that is a biological system too: one 'should expect there to be one distinct subsystem for each reliably recurring function that human minds are called upon to perform' (Carruthers 2006b: 10) – especially seeing as the senses, which like language are subserved by the input systems, are thought of as being modular.

3.2.1.3 Carruthers' argument from design summarised

In short, Carruthers (2006a) argues that the mind is an evolved biological system and as such, must, therefore, obey the same principles of design that other biological systems do. His argument is neatly summarised on page 25 as follows, and is closely related to his second argument from animals, which will be the focus of section 3.2.2):

> (1) Biological systems are optimally designed systems, constructed incrementally.
> (2) Such systems, when complex, need to have massively modular organization.
> (3) The human mind is a biological system, and is complex.
> (4) So the human mind will be massively modular in its organization.

3.2.2 The argument from animals

The computational theory of mind has been the principal position in the field of philosophy on how the mind works for the past five plus decades. The first suggestion that something like the Turing machine might model the mind well was made by McCulloch and Pitts in 1943, and by the 1960s Turing computation came to be central to cognitive science (Rescorla 2017). The classical computational theory of mind has two basic tenets. The first is that the mind is a representational system and the second is that mental representations are processed computationally (Sterenly 1990). I consider these both here.

Now, the problem most people have with the computational theory of mind is that, because it has the word 'computational' in its title, they are inclined to think of the mind as a computer – but that's not what the theory intends (Rescorla 2017). Rather, what the computational theory of mind proposes, is for the mind to be thought of as a computational *system.* Describing the mind as a computer strongly suggests that it is susceptible to programming which neither minds (nor most Turing-style computational systems, for that matter) are (Rescorla 2017). As a result, a number of critics (e.g. Churchland et al. 1990) have falsely objected to the computational theory of mind by virtue of their thinking that the mind is not like a programmable, general-purpose computer. In reality, though, nobody is saying it is.

With that being said, some of the most widely accepted articulations of the computational theory of mind are due to Fodor (1975, 1987, 2000) (see also Fodor and Pylyshyn 1988). One of the main contributions he made was that the mind has an inner medium that carries out computations, and that compositionality, productivity and systematicity of thought could only be explained if that inner medium has a structure of language which has those three properties. According to the language of thought hypothesis, thought and thinking are done in a mental language where words express concepts and sentences propositional attitudes (Murat 2015).

The language of thought hypothesis as it is understood today can be characterised as a combination of the following three theses (Murat 2015: 2-3):

A. Representational Theory of Mind (RTM) (cf. Field 1978:37, Fodor 1987:17):
    1. Representational Theory of Thought: For each propositional attitude *A*, there is a unique and distinct (i.e. dedicated) psychological relation *R*, and for all propositions *P* and subjects *S*, *S A*s that *P* if and only if there is a mental representation #*P*# such that
        a. *S* bears *R* to #*P*#, and
        b. #*P*# means that *P*.
    2. Representational Theory of Thinking: Mental processes, thinking in particular, consists of causal sequences of tokenings of mental representations.

B. Mental representations, which, as per (A1), constitute the direct "objects" of propositional attitudes, belong to a representational or symbolic *system* which is such that (cf. Fodor and Pylyshyn 1988:12–3)
    1. representations of the system have a combinatorial syntax and semantics: structurally complex (molecular) representations are systematically built up out of structurally simple (atomic) constituents, and the semantic content of a molecular representation is a function of the semantic content of its atomic constituents together with its syntactic/formal structure, *and*
    2. the operations on representations (constituting, as per (A2), the domain of mental processes, thinking) are causally sensitive to the syntactic/formal structure of representations defined by this combinatorial syntax.

C. Functionalist Materialism. Mental representations so characterized are, at some suitable level, functionally characterizable entities that are (possibly, multiply) realized by the physical properties of the subject having propositional attitudes (if the subject is an organism, then the realizing properties are presumably the neurophysiological properties of the brain).

What is most interesting about the language of thought hypothesis however, as Carruthers (2006a) points out, is that it has important implications for animal cognition. According to Fodor (1975, 1987, 2000), language of thought is not dependent on any natural language, which leaves open the possibility that non-linguistic creatures (i.e. animals) can think as well (Carruthers 2006a).

Recall from 3.2.1 (i.e. Carruthers' (2006a) argument from design) that the modularity of cognitive capacities constitutes an evolutionary feature than enhances the adaptability of organisms so that they can cope more efficiently with their environments. In his second argument for massive modularity (the one which comes from animals), Carruthers (2006a) develops this point further, claiming that non-human animal minds are also massively modular and that the characteristic was conserved during the evolutionary transition from the animal mind to the human mind as valuable characteristics often are.

Unlike the argument from design, the argument from animals isn't summarised in Carruthers (2006a), though it has been helpfully broken down by Wilson (2008: 278) like so:

(1) Animal minds are massively modular.
(2) Human minds are incremental extensions of animal minds.
(3) So human minds are massively modular.

3.2.2.1 Types of module

3.2.2.1.1 Learning modules

Following Gallistel (1990, 2000) and Tooby and Cosmides (1992; 1995), Carruthers begins his argument from animals with a 'reflection on the differing task demands of the very different learning challenges that people and animals must face, as well as the demands of generating appropriate fitness-enhancing intrinsic desires' (Carruthers 2006a: 29). It is one task to work out the sun's azimuth angle (which, with other measures such as its zenith angle) defines its position in the sky at any given time of day (Seinfield and Spyros 2006; Sukhatme 2008; Duffie and Beckman 2013) and use it to find your way from point A to point B, but it is quite another to follow the Polaris star north, say, and turn 90 degrees clockwise to go east, another 90 to go south, and a further 90 to head west, Carruthers (2006a) says. These are both learning problems animals can solve, but they require quite different learning mechanisms to do so (Carruthers 2006a).

To reinforce that argument, Carruthers (2006a) widens his focus on navigation to a number of other learning problems such as vision, speech recognition, mind-reading, cheater detection and complex skill acquisition. Each of these learning problems pose computational challenges that are distinct from the computational challenges of other learning problems, he points out, and '[s]o for each problem, we should postulate the existence of a distinct learning mechanism, whose internal processes are computationally specialized in the way required to solve the task', he says (Carruthers 2006a: 29). Carruthers (2006a: 29) finds it 'very hard to believe that there could be any sort of *general* learning mechanism that could perform all of these different roles', and he is not alone in this view. Gallistel (2000), Gallistel and Gibbon (2001) and Gallistel et al. (2001) have all argued forcefully, for example, that there is *no such thing.*

3.2.2.1.2 Desire modules

Next, Carruthers (2006a: 30) defines learning as a process that 'issues in true beliefs, or beliefs that are close enough to the truth to support [the] inclusive fitness' of an individual. Desires, he says, therefore aren't learned in *that* sense of the word, but need to be formed in ways that will support an individual's inclusive fitness still. Carruthers points out that while some desires are 'instrumental' in that they are 'derived from ultimate goals together with beliefs about the means that would be sufficient for realizing [them]' (Carruthers 2006a: 30), not all acquired desires are formed that way. Rather, 'evolutionary psychology postulates a rich network of systems for generating new desires in the light of input from the environment and background beliefs' (Carruthers 2006a: 30).

Much of these desires, Carruthers (2006a) says, are, therefore, 'ultimate' in the sense that they are 'produced by inferences taking place in systems dedicated to creating desires of that sort' as opposed to being 'produced by reasoning backwards from the [need] to fulfil some other desire' (Carruthers 2006a: 30). A desire to have sex with someone, for example, isn't always produced by reasoning that such an act is likely to fulfil an evolutionary goal of reproduction, he argues. Rather, it can be generated by some sort of system (module) that has evolved for that purpose, he says (Carruthers 2006a).

3.2.2.1.3 Other modules

What emerges from Carruthers' (2006a) thoughts in Chapter 1 then is a picture of the mind being made up of a host of systems specialised for learning, and another host of systems that have been designed to generate fitness-enhancing desires, but although this looks like *much* of the mind is modular, it does not yet follow that the mind is composed *exclusively* of modules. Being well aware of this, Carruthers (2006a) goes on to argue in Chapter 2 that not only do animal minds consist of sets of learning modules and desire-generating modules, the systems charged with belief-generation and emotion-generation, and the selection, organisation and control of action are decomposable as well. As such, there is nothing left of the mind that could be considered nonmodular, for all of its primary functions (perception, belief, desire, practical reasoning and motor-control) are subserved by modules.

Carruthers (2006a) devotes all but one paragraph of Chapter 2 to defending the first premise of his argument from animals (that animal minds are massively modular) (Wilson 2008).[1] The final remaining one, however, begins as follows, and is taken by Wilson (2008) to be the reasoning behind premises (2) and (3) (that human minds are incremental extensions of animal minds and that human minds are massively modular, respectively):

> Given that animal minds are organized along massively modular lines, then normal biological reasoning should lead us to expect that massively modular architecture will be preserved in the minds of members of *Homo sapiens*, too. (Carruthers 2006a: 149)

For Carruthers (2006a) it makes sense from an evolutionary perspective that if animal minds are massively modular, human minds will be as well. Fitness-enhancing biological structures are generally preserved in the evolutionary transition from one species to another, so provided it is appropriate to extend biological and evolutionary principles to minds, he says, we should expect the mind to be made up of the same modular systems that are present in apes.

3.2.3 The argument from computational tractability

Carruthers' (2006a) third and final argument in support of massive modularity is the one from computational tractability, which is derived in part from the earlier Fodorian (1983, 2000) tractability argument that Carruthers (2006a: 44) has helpfully summarised as follows:

(1) The mind is computationally realized.
(2) All computational mental processes must be suitably *tractable.*
(3) Only processes that are informationally encapsulated are suitably tractable.

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[1] The chapter also locates those modules within a basic perception/belief/desire/practical reason/motor-control architecture, but I will say no more about that for it goes well beyond the scope of this thesis.

(4) So the mind must consist entirely of encapsulated computational systems.

Carruthers' (2006a) argument begins like Fodor's (1983, 2000) with the relatively uncontroversial claim, in the cognitive sciences, that cognitive systems are realised computationally (Gottschling 2009). If this is correct, then their computations must be suitably tractable, he (again, like Fodor (1983, 2000)) claims, which means they must be (at least in principle) able to be carried out within finite time (Gottschling 2009). The tractability of computations requires them, therefore, to be frugal in two respects: first in that the algorithms used in their computation must be suitably simple; and second in that they use in their computation only a limited amount of information (Gottschling 2009).

'[I]n order to be tractable, computations need to be encapsulated' (Carruthers 2006a: 51) says, 'for only encapsulated processes can be appropriately frugal in the informational and computational resources that they require' (Carruthers 2006a: 51). This looks a lot like something Fodor (1983, 2000) would have said at first glance, but while that may be, his definition of encapsulation here is quite different from Fodor's (1983, 2000) it turns out, as section 3.2.3.1 explores.

3.2.3.1 Narrow-scope vs. wide-scope encapsulation

On pages 57 through 59 Carruthers (2006a) draws a distinction between narrow-scope and wide-scope encapsulation, using a sketch from Atran (2002) of a practical-reasoning systems which takes as its initial input whatever is currently the strongest desire for P and conducts a search through long-term memory for beliefs of the form $Q \rightarrow P$. If it finds one, a belief of the form $Q \rightarrow P$, that is, it checks its database to see whether Q is something for which a motor-scheme exists (i.e. whether Q is something that can be done in the here and now). If so, it goes ahead and does that, but if not, it conducts another search, this time for beliefs of the form $R \rightarrow Q$ and so on. The system also has a couple of simple stopping rules, Carruthers (2006a: 58) claims, e.g.: '[i]f it […] go[es] more than n conditionals deep without success, or if it […] search[es] for the right sort of conditional belief without finding one for more than some specified time t, then it stops and moves on to the next strongest desire', making it frugal 'both in the information that it uses, and in the complexity of its algorithms' – but does that count as it being encapsulated?

Well, not in the way that informational encapsulation is traditionally understood, Carruthers (2006a) stresses, for here […] the practical-reasoning system can search within the total set of the organism's beliefs, using structure-sensitive search rules' (Carruthers 2006a: 58), and those beliefs would not affect a cognitively impenetrable system. There does seem to be *a* sense in which the system's encapsulated though, he points out. And so he draws a distinction between narrow-scope and wide-scope encapsulation, where narrowly-scoped encapsulated systems are those that cannot be affected by *all* that's beyond them, and widely-scoped encapsulated systems those that can be affected by *some* but can't be by most. See below for an explanation:

> Put as neutrally as possible, it can be said that the idea of an encapsulated system is the notion of a system whose internal operations *can't* be affected by *most or all* of the information held elsewhere in the mind. But there is a scope ambiguity here.[2] We can have the modal term 'can't' take narrow scope with respect to the quantifier, or we can have it take wide scope. In its narrow-scope form, an encapsulated system would be this: concerning most of the information held in the mind, the system in question *can't* be affected by *that* information in the course of its processing. Call this 'narrow-scope encapsulation'. In its wide-scope form, on the other hand, an encapsulated system would be this: the system is such that it *can't* be affected by *most* of the information held in the mind in the course of its processing. Call this 'wide-scope encapsulation'. (Carruthers 2006a: 58)

Since wide-scope encapsulation of information is enough for a system to be frugal (and therefore tractable) as demonstrated by the above example, Carruthers (2006a) argues, premises (3) and (4) must be revised for the argument from computational tractability to be an accurate one. Narrow-scope encapsulation (i.e. encapsulation as it is traditionally understood) is not necessary, he says, and so the following is proposed instead (Robbins 2015: 20):

(1) The mind is computationally realized.
(2) All computational mental processes must be tractable.
(3) Only processes that are at least weakly (i.e., wide-scope) encapsulated are tractable.
(4) So the mind must consist entirely of at least weakly encapsulated systems.
(5) So the mind is massively modular.

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[2] 'Modal terms like 'can' and 'can't' have wide scope if they govern the whole sentence in which they occur; they have narrow scope if they govern only a part. Compare: 'I can't kill everyone' (wide scope; equivalent to, 'It is impossible that I kill everyone') with; 'Everyone is such that I can't kill them' (narrow scope). The latter is equivalent to, 'I can't kill anyone.'' (Carruthers 2006a: 59)

The claim that modules must be encapsulated in Fodor's narrow-scoped sense was criticised by a number of researchers (e.g. Marlen-Wilson and Tyler 1987; Churchland 1988) before Carruthers drew a distinction between narrow- and wide-scope encapsulation in 2006a, but one of its least complementary critiques is actually due to Prinz (2006) who argues that the linguistic system rarely (if ever, in fact) satisfies this requirement. Prinz (2006) argues that top-down effects (effects of the high-level central systems) on linguistic processing could count as strong evidence that the input systems aren't encapsulated. He draws special attention to the phoneme restoration effect in this argument (which is a perceptual phenomenon in which, under certain conditions, sounds that are missing from the speech signal can be auditorily hallucinated by the brains of and therefore 'heard' by listeners). This can be so strong that listeners don't even pick up that there were actually any phonemes missing to begin with, they simply 'fill-in' the acoustic gap with a phoneme determined by the sentence's semantic interpretation (Warren and Warren 1970; Samuel 1987; Kashino 2006). If subjects hear *The _eel is on the axel,* for example, they experience a /w/ sound in the gap, and if they hear *The _eel is on the orange,* they experience a /p/ sound, Prinz (2006) points out.

Something similar is done for the perceptual systems by Churchland (1989), who argues that although Fodor (1983) found evidence to suggest the lack of power knowledge has over perception in the Müller-Lyer illusion (Day 1989), the well-known duck-rabbit figure (see Figure 3.6) shows the opposite, that there are 'a wide range of elements central to visual perception […] which are cognitively penetrable' (Churchland 1989: 261):

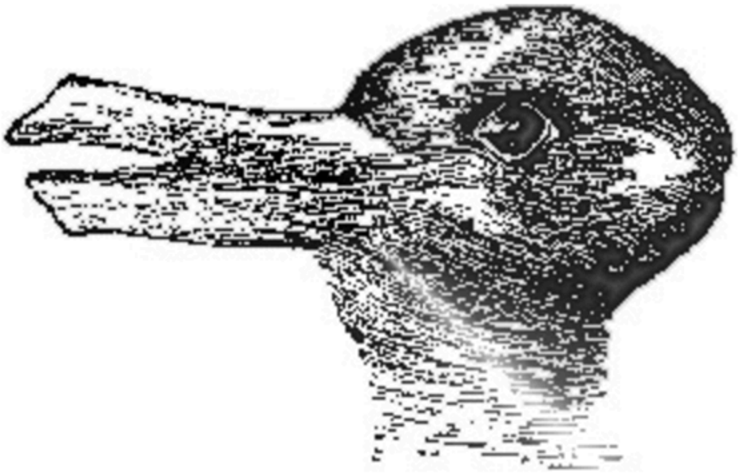

Figure 3.6 The duck-rabbit figure (Jastrow 1899: 312)

Unlike in the Müller-Lyer illusion (Day 1989) where a person’s knowledge that the two lines in it are of the same lengths cannot change the fact that they are seen to be of different ones, knowledge that the duck-rabbit figure is ambiguous and can be seen in one way or the other (i.e. either as a duck or as a rabbit) *can indeed* alter peoples’ perceptions of the figure – which would suggest that the visual system has access to knowledge after all.

3.3 Carrutherian modules

There are two cross-cutting spectra of opinion about how much of the mind is modular, then – Fodor’s (1983, 2000) modest opinion and evolutionary psychologists’ massive one (e.g. Smith and Tsimpli 1995; Tooby and Cosmides 1995; Segal 1996; Fiddick et al. 2000; Carruthers and Chamberlain 2000; Cosmides and Tooby 2001; Carruthers 2006a) – and the two camps’ definitions of modularity clearly cannot be the same. According to Fodor (1983), whose argument was designed to apply to the input and output systems only, modules are domain specific and innately specified processing systems that have their own proprietary transducers and that deliver shallow (i.e. non-conceptual) outputs (Carruthers 2003a). They are also held to be ‘mandatory in their operation, swift in their processing, isolated and inaccessible to the rest of cognition, associated with particular neural structures, liable to specific and characteristic patterns of breakdown, and develop according to a paced and distinctively-arranged sequence of growth’ (Carruthers 2003a: 68).

Those that argue for a more modular account of cognition, however, have had to weaken Fodor’s (1983) definition somewhat so that it can be applied to central systems as well (Smith and Tsimpli 1995; Segal 1996; Carruthers and Chamberlain 2000; Carruthers 2006a). In *The Architecture of the Mind,* for example, Carruthers (2006a: 12) lists which of Fodor’s features would most likely have to go for his thesis of massive modularity to stand up:

> [I]f a thesis of mental modularity is to be remotely plausible, then by ‘module’ we cannot mean ‘Fodor-module’. In particular, the properties of having proprietary transducers, shallow outputs, fast processing, significant innateness or innate channeling, and encapsulation will very likely have to be struck out. This leaves us with the idea that modules might be isolable function-specific processing systems […] which are domain specific (in the content [i.e. Fodorian] sense), whose operations aren’t subject to the will, which are associated with specific neural structures (albeit sometimes spatially dispersed ones), and whose internal operations may be inaccessible to the remainder of cognition.

Of the original set of descriptors for Fodorian modules, then, Carruthers strikes out four, leaving him with only domain specificity, dissociability, automaticity, neural localisability, and central inaccessibility. Informational encapsulation, notice, is absent from this list.

4. Concluding remarks

In what follows, I consider the extent to which the phonological processor can be considered modular in Fodor's modest (1983, 2000) sense of the word (in Chapter 4) and modest in Carruthers (2006a) massive sense of it (in Chapter 5). For Fodor (2000) informational encapsulation is the defining feature of modularity, but for Carruthers (2006a) it is not as necessary as another feature – that feature being domain specificity. Carruthers (2006a) places greater emphasis on modules of mind having to be domain specific than he does informationally encapsulated because, to his mind (recall from the argument from design in section 3.2.1 and from animals in 3.2.2), modules only exist to subserve a certain function, and so for frugality purposes (recall from his argument from computational tractability in 3.2.3), they should only be able to operate on information that's relevant to their specialism. A module with a function specific to a domain is *ipso facto* only concerned with computing the kinds of input that have to do with that domain in Carruthers (2006a) eyes – and I am inclined to agree.

Chapter 4: A Modestly Modular Approach to Phonology

1. Introduction

Formalism and functionalism are two diametrically opposed approaches to linguistics (Newmeyer 2010). This functionalist/formalist distinction is particularly apparent in phonology given its position within the architecture of the language faculty between the mental and the physical (Hannahs and Bosch 2018). Phonetics and phonology are traditionally thought of as being two different things. According to formal linguists, phonology is concerned with the connections between the internal grammar and the external sound system (that is, phonetics) (Hannahs and Bosch 2018)[3]. In recent years, however, some phonologists have come to conflate the two. This has led to a number of unconventional conclusions about phonology (and indeed the language faculty in which it resides) (Hannahs and Bosch 2018).

As Hannahs and Bosch (2018) rightly point out, usage-based phonological analyses owing to the likes of Bybee (2003, 2010) refer to facts about speech as it is used in the real world to inform their thinking about language, and corpus-based work (as in that of Durand et al. (2014), for example), does the same. Articulatory phonologists (e.g. Browman and Goldstein 1986) meanwhile, argue that the phonological system is organised in accordance with the ways in which speech is articulated, and even optimality theorists (such as Hayes et al. (2004) – who are more formal in their approach than Bybee (2003, 2010), Durand et al. (2014) and Browman and Goldstein (1986) put together) argue for the phonetic grounding of optimality constraints (Hannahs and Bosch 2018).

This blurring of the distinction between phonetics and phonology has gone as far as to lead some scholars to assert that phonetics and phonology are actually one and the same thing (e.g. Johnson 2007) and others still further that phonology plays no part in the grammar at all (Burton-Roberts 2011). Even those who maintain that there's a distinction between phonetics and phonology are in disagreement about where and how the distinction should be drawn. For

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[3] I should probably point out that when I say phonology here, I'm talking about spoken language phonology in particular (hence my reference to the sound system) despite the fact that it's my belief that the only difference between spoken language and signed language is that the former is produced by the mouth and perceived by the ears, and the latter is produced by the hands and perceived by the eyes, and so underlying, their phonological structures should be the same.

some (e.g. Archangeli and Pulleyblank 1994; Hayes et al. 2004) phonology is distinct from phonetics but is ‘phonetically grounded’, and for others (e.g. Blaho 2008; Hale and Reiss 2008; Iosad 2013, 2017) the symbols the phonological processor computes are actually abstract because phonology is independent of phonetics or ‘substance-free’, as it is so-called.

It is against the phonetically grounded/substance-free debate that this chapter has been written, as the substance-free approach to phonology has interesting things to say about Fodor’s (1983) informationally encapsulated definition of modularity. A Fodorian modular phonology must be substance-free, and the same is true in the opposite direction: if phonology is phonetically-grounded, then it can’t be modestly modular. It’s no wonder that we haven’t reached a clear conclusion about whether phonology is modular or not then, for many theories of phonology are still (incorrectly, I argue) grounded in phonetics.

Personally, I take a position on the substance-free side of the fence. This is, importantly, for reasons that *don’t have to* do with modularity though, which will be discussed in due course along (roughly) the lines of Blaho (2008) and Iosad (2013, 2017). Spontaneous phonologisation (a phonological process described by Fruehwald in 2013 and 2016) also leads me to this line of thinking, and so this is explored also at the end of the chapter.

2. Substance-free phonology

Phonetically grounded phonologists (e.g. Archangeli and Pulleyblank (1994), and those who published papers in Hayes et al. (2004): i.e. Crosswhite (2001), Hayes and Steriade (2004), Wright (2004), Jun (2004), Kaun (2004), Blevins and Garrett (2004), Dresher and Zhang (2004), Flemming (2004) and Gordon (2004)), blur the distinction between phonetics and phonology in their claim that phonological processes are the result of articulatory and acoustic factors (Blaho 2008). They argue that articulatory and acoustic knowledge is encoded in phonology in the form of constraints, requiring phonological representations to be characterised in accordance with information important to production and perception (Blaho 2004).

It’s easy to see why people may adopt an approach to phonology such as this one given the Jakobonian legacy of substantive markedness and the universal features described in classic generative phonology of course (Jakobson et al. 1952; Chomsky and Halle 1968). Even OT, which is a relatively recent phonological framework, assumes the phonetic grounding of some of its phonological constraints (Prince and Smolensky 1993). A number of other researchers, however, are of the opposing position that phonology is actually autonomous of phonetics; that both phonological representations and the processes that compute them are devoid of phonetic influence (Dresher et al. 1994; Hale and Reiss 2000a, b; Hume 2003; Avery and Rice 2004; Blevins 2004; Blevins and Garrett 2004; Mielke 2004, 2005; Hale et al. 2007; Morén 2007a, b, c; Blaho 2008; Hale and Reiss 2008; Iosad 2013, 2017). This argument appears to have good grounds.

There are five schools of thought when it comes to substance-free phonology, and they're listed here as follows, from the most substance-free to the least substance-free:

- The Concordia school of thought (Hale and Reiss 2000a, b; 2003, 2008; Hale et al. 2007)
- The Toronto school of thought (Avery and Rice 1989; Rice and Avery 1991; Piggott 1992; Rice 1993; Dresher et al. 1994; Avery 1996; Dresher 2001, 2003)
- Element Theory (Harris 1990, 1994, 2005, 2006; Harris and Lindsey 1995)
- The Parallel Structures Model (Morén 2003a, b; 2006, Morén 2007a, b, c)
- Radically Substance-free Phonology (Odden 2006, 2013; Blaho 2008, Youssef 2010)

For a detailed overview of these many varying schools of thought, one could consult Blaho (2008), but for this thesis, it should suffice to say that there are indeed five, and that, although they differ in their degrees of substance-freeness, they are all alike in that they reject a one-to-one correspondence between phonetics and phonology.

In the sub-section that follows (section 2.1), I will offer some assumptions about substance-free phonology, before summarising some arguments that were offered in the theses of Blaho (2008) and Iosad (2013) to support the substance-free position (including gradual phonologisation) in sub-section 2.2. I'll then present a couple of analyses of /ay/ raising in Philadelphian English which I believe to evidence a substance-free phonology: Fruehwald's (2013) (in 2.3.1) who assumes that /ay/ raising in Philadelphia is due to spontaneous phonologisation, and the phonological analysis that I myself subscribe to, that of Bermudez-Otero (2017), who claims pre-fortis clipping is what causes /ay/ to raise (in 2.3.2). In section 3 some concluding remarks are made, mainly that there is plenty pointing to phonology being substance-free, and therefore an informationally encapsulated module of the mind.

2.1 Some assumptions about substance-free phonology

According to Blaho (2008: 2), the three major tenets of substance-free phonology are as follows:

(1) Phonology refers to the *symbolic computational system* governing the *significant*, i.e., the non-meaningful level of linguistic competence. Phonology is taken to be *universal* – common to all (natural human) languages and all modalities –, and *innate*. Phonological knowledge is part of [Universal Grammar(]UG[)], but phonetics is not.

(2) Phonological primes are substance-free, in that their phonetic interpretation is invisible to phonology, and thus does not play a role in phonological computation.

(3) Markedness and typological tendencies (in the sense of Greenberg (1957, 1978)) are not part of phonological competence, but rather an epiphenomenon of how extra-phonological systems such as perception and articulation work.

A number of arguments have been made in favour of the above, and so I consider those next in section 2.2.

2.2 Some existing arguments for a substance-free phonology

2.2.1 Explanatory adequacy

An existing argument from Hale et al. (2007) (that was described by Blaho (2008)) for a substance-free phonology has to do with explanatory adequacy. As Hale et al. (2007: 662) point out, the set of attested languages (that is, the number of languages (living or dead) we have evidence to prove existed) is not equal to the set of statable languages (that is, the number of languages that a model of phonology can predict). Rather, the relationship looks a little something like this (Blaho 2008: 7):

(1) attested $\subset$ attestable $\subset$ humanly computable $\subset$ statable

Hale et al. (2007: 663) describe the pattern in the following way:

> First, the set of attested languages is a subset of the set of attestable languages (where 'attestable' includes all linguistic systems which could develop diachronically from existing conditions—e.g., all dialects of English or Chinese or any other language in 400 years, or 4000 years, etc.). In addition, the set of attestable languages is a subset (those which can evolve from current conditions) of the set of humanly computable languages. (In our opinion, the human phonological computation system can compute a featural change operation such as /p/ → [a] / _ d but it is of vanishingly small probability that such a rule could arise from any plausible chain of diachronic changes.) Finally, the set of humanly computable languages is itself a subset of formally statable systems (which could include what we take to be humanly impossible linguistic processes such as /V/ → [V:] in prime numbered syllables). The key point here is that the set of diachronically impossible human languages is not equivalent to the set of computationally impossible human languages.

The argument that follows from this view of language, Blaho (2008) reasons, is that 'it is preferable for a model of phonology to have as few assumptions as possible, *even at the expense of overgenerating*' (Blaho 2008: 8). For example, given three groups of sounds A, B and C, if there was evidence of languages with A and B, B and C, and A, B and C, and yet there wasn't any of languages with A and C, then most phonologists would be inclined to argue that UG has a rule prohibiting systems consisting of A and C, right? Well, according to Blaho (2008), not necessarily. We can't always assume that what we observe, has a phonological reason behind it, she stresses.

It might be that a pattern observed has an extra-phonological explanation having to do with production or perception, for a start. To take Blaho's (2008) trivial example, the fact that there have been no sounds produced with the larynx and lips as the articulators, for instance, needn't be encoded in the phonology as some sort of feature co-occurrence restriction, because it can already be explained by the anatomy of speech organs, in that the larynx and lips are simply too far apart for this to be a possible pattern.

Principles of UG of the type 'features can combine freely, except for A and B' or even worse 'all sounds are possible except for this one and that one because the articulators are too far apart' are therefore not necessary in Blaho's (2008) opinion, and nor are they in mine. As Blaho (2008) puts it: explanatory adequacy shouldn't be sacrificed for the sake of observational or descriptive adequacy. And so anything that can be explained outside of phonology should be.

2.2.2 Multiple modalities

One of the most obvious arguments for the autonomy of representations, though, to my mind, comes from the existence of languages that aren't spoken. A key property of grounded phonology, (Blaho 2008) points out, is that constraints refer to aspects of spoken language only, even though there are many more modalities than that. Linguistics has long established that signed languages are natural human languages in the way that spoken ones are (Stokoe 1960; Klima and Bellugi 1979; Senghas et al. 2004); there are a number of autonomous and mutually unintelligible systems of communication used in deaf communities around the world (MacSweeney et al. 2008). Now, to my mind, the only way in which spoken languages and signed languages differ is in their methods of production and perception surely (in that spoken languages are produced by the mouth and perceived by the ears, whereas signed languages are produced by the hands and perceived by the eyes) and so underlyingly, if phonological knowledge is as universal to humans as Chomsky and Halle (1968) say it is, then their linguistic structures should be the same.

As Blaho (2008) says, it is not easy to see how a phonetically grounded model of phonology could account for signed language, for this isn't a modality in which (oral/nasal) articulatory production and acoustic perception plays a part. If it is the case that the constraints of phonology *are* phonetically grounded, then UG must have at least two sets, surely: one for spoken language phonology, one for signed language phonology (and possibly others if it turns out that those modalities have different phonetics than do the spoken and signed ones). Per Blaho's (2008) (and indeed my own) understanding, the simplest explanation is that innate phonology is free of phonetic information concerning production and perception, and the mapping from underlying forms to their surface realisations is something that's acquired during language learning instead at an interface between phonology and phonetics. In other words, a substance-free phonology would be less of a cognitive load than a phonetically grounded one would, and therefore the one most likely to play a part in a human mind that is bound by the constraints of memory.

2.2.3 Emergence

2.2.3.1 Emergent features

A fundamental question that's been asked by generative phonologists concerns how phonological patterns of the world's languages are characterised. In generative phonological theory, it's argued that universal (and therefore innate) building blocks of speech sounds called distinctive features are responsible for this – and these characterisations have to do with phonetics (e.g. [voice] for which segments can be + or – depending on whether they're produced with the vocal chords vibrating or not, respectively, and [nasal] for which segments can be + if they are produced with the velum lowered which allows air to flow through both the nose and the mouth or – if there is velic closure, that is if they are produced with the velum raised allowing air to flow through the mouth only).

Segments (and natural classes of them) are typically interpreted in the generative phonology literature as bundles of these phonetically defined feature sets that match the ways in which they are articulated but, although the central idea behind distinctive feature theory (that contrasts between phonemes can more elegantly be described in terms of properties of segments as opposed to treating segments as alphabetic atoms) makes sense, in many of the features being defined by auditory and articulatory terms distinctive feature theory is clearly not supportive of the suggestion that phonology is substance-free.

Distinctive feature theory, which draws on the work of Saussure (1966), Trubetzkoy (1969) and Jakobson et al. (1952) dates back as far as Jakobson (1942) but received its most sophisticated defence at the hands of Chomsky and Halle in 1968 in *The Sound Pattern of English* (SPE). Chomsky and Halle (1968) recast the acoustically defined features as articulatory ones so that all distinctive features had to do with articulation. In more recent years weaker versions of distinctive feature theory (owing to Kiparsky (1985, 1995), Steriade (1987, 1995), Archangeli (1988), Archangeli and Pulleyblank (1994), Hale et al. (2007) and Hale and Reiss (2008)) have come about too, but in spite of their weakening of the others' theses, they wholeheartedly maintain that there exists a set of universal and innate features that specify segments (Iosad 2013).

More recently still, scholars have shown, however, that there isn't necessarily a *need* for features to be innate, for they are inducted over the input by the language learner during acquisition – that is, they are emergent from the ways in which the productive and perceptive systems work (Blaho 2008). In *The Emergence of Distinctive Features*, for example, Mielke (2008) set out to evaluate how well distinctive feature theories could account for the range of phonologically active classes in a cross-linguistic database. He compared the success of three theories (Jakobson et al.'s (1952) Preliminaries to Speech Analysis (PSA), Chomsky and Halle's (1968) SPE and Clements and Hume's (1995) Unified Feature Theory (UFT)) against a database consisting of 6,007 classes of sounds described in 628 language varieties, to see how phonologically active classes (that is, sets of segments that act as targets or triggers in alternations) lined up with the classes that those featural theories predict. It turned out that 'no single theory [was] able to characterize more than 71 percent of the classes, and over 24 percent [were] not characterizable in any of the theories' (Mielke 2008: 3); the theory that fared the best was that of Chomsky and Halle (1968) – but even that was only able to cover 4,313 of the 6,077 classes, Mielke (2008) said.

Off the back of these results, Mielke (2008) argued that the segment classes predicted by phonological features in the literature aren't actually representative of those that are active extra-grammatically. Thus, a theory in which language learners observe categorical distributions in surface forms and map them onto underlying phonological categories is one that would explain the natural and unnatural class patterns found in the data.

To his mind, then, humans are not born with a limited set of universal features, they are born with the ability to create them using the data they're exposed to during the course of language acquisition. The same is said by Hale and Reiss (2000) about a phonological pattern that was originally depicted in terms of teleological constraints by Beckman (1997), and so I'll consider this in subsection 2.2.3.2.

2.2.3.2 Emergent patterns

According to Hale and Reiss (2000b), there is no need for UG to prescribe that which can be procured from the physiological. They argue that that pattern described by Beckman (1997) isn't one that is innately imposed but in actuality arises through emergence, and so I summarise what they (Beckman (1997) and Hale and Reiss (2000b), that is) had to say about that in what follows.[4]

In *Positional Faithfulness, Positional Neutralisation and Shona Vowel Harmony*, Beckman (1997: 1) argues that:

> The distribution of the feature [high] in Shona verbs is a prototypical example of positional neutralisation accompanied by vowel harmony.[…] In languages which exhibit positional neutralisation of vowel contrasts, one or more vowels (generally, the most marked

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[4] Hale and Reiss (2000a, b) also reject that *r*-insertion in Massachusetts English (McCarthy 1993) and opacity effects in Hebrew spirantisation (1996) should appeal to teleological constraints, but given the limitations on space in this thesis, I've elected to explain the most illustrative of the examples only. For an overview of the other two, please feel free to refer back to the original text.

> members of the vowel inventory) may occur distinctively in only a small subset of the structural positions available in the language. Outside of these positions, the marked vowels may surface only if they harmonise with a similar vowel in the privileged position.

The mid vowels /e/ and /o/ in Shona verbs then, she says, 'are contrastive only in root-initial syllables [and may appear] in subsequent syllables only when preceded by a mid vowel in root-initial position' (Beckman 1997: 1). 'Low vowels neither trigger nor propagate height harmony; only [+high] *i* and *u* may follow a low vowel, even if the root-initial vowel is mid' (Beckman 1997: 1). 'Harmony fails to apply between a root-initial mid front *e* and a subsequent round vowel'[, and t]hus, rather than a height-harmonic string of *e ... o*, we find disharmonic *e ... u*. Harmony applies regularly when the initial vowel is round *o*', meanwhile (Beckman 1997: 2).

Beckman (1997) goes on to present a full analysis of Shona height harmony, proposing that the phonological properties of initial and non-initial syllables (described above) arise due to a dispersion of a couple of IDENT constraints:

(1) IDENT-σ_1(hi)
A segment in the root-initial syllable in the output and its correspondent in the input must have identical values for the feature [high].

(2) IDENT(hi)
Correspondent segments in output and input have identical values for the feature [high].

(1) and (2) together allow faithfulness to the feature [high] to be maintained in some contexts but not others, she says, as the context-sensitive constraint IDENT-σ_1(hi) in (1) ranks above *HIGH (a markedness constraint that is violated by the presence of high vowels), which itself ranks above the general constraint IDENT(hi) in (2):

(3) IDENT-σ_1(hi) >> *HIGH >> IDENT(hi)

More specifically, Beckman (1997: 8) argues that in Shona:

> the ranking of *IDENT-σ_1(hi)* above the vowel-height markedness constraints (see below) permits the full range of height contrasts to

> occur in initial syllables, and further renders these syllables impervious to height harmony. By contrast, the ranking of the context-free constraint *IDENT(hi)* below the markedness constraints renders noninitial syllables incapable of licensing marked vowels and further, susceptible to height harmony.

Hale and Reiss (2000b), on the other hand, argue that many markedness patterns are actually emergent – including the one explored by Beckman (1997). Consider the alternative account they offer (Hale and Reiss 2000b: 159-160):

> Imagine a child exposed to a language L_1 that allows high vowels in all syllables—initial, medial, and final. Imagine further that L_1 has initial stress and that stress is realized as relatively increased duration and intensity. Given this scenario, it would not be surprising to find that a child constructing L_2 on the basis of output from L_1 consistently fails to acquire a contrast between mid and high vowels in relatively short, quiet syllables (those that are noninitial and thus unstressed), but succeeds in acquiring it in initial syllables, which are stressed and thus longer and louder. This mapping from L_1 to L_2 is an example of "sound change"—in particular, […] a "conditioned merger"[.]
>
> On the other hand, it is highly implausible that a child would consistently fail to correctly analyze the mid/high contrast in longer, louder (stressed) syllables, yet successfully analyze the contrast in relatively short, quiet syllables.
>
> We see therefore that the existence of positional faithfulness phenomena can be understood as merely reflecting the nature of the learning situation[and is therefore not a reflection of any grammatical principle].
>
> If the acoustic cues of a given contrast in the target language are correctly analysed by the acquirer in a context where they are relatively weak, they will also be analysed correctly in a context where they are relatively strong.

They suggest instead that the markedness pattern is an emergent property, rather than an innate one. '"Positional faithfulness" is due' Hale and Reiss (2000b: 160) claim, 'not to the nature of phonology [itself], but to the "sifting effect" of acquisition on the incidental, arbitrary nature of the phonetic substance associated with phonological symbols'.

Beckman's claim that positional faithfulness constraints should be encoded in the grammar is therefore rejected by Hale and Reiss (2000b) as, for them, if something can be explained by something external to phonology (as is the case here, because 'those [effects] observed by Beckman already have a coherent extragrammatical account within acquisition theory' (Hale and Reiss 200b: 160)), then it need not and should not be fixed into it in the form of teleological constraints. There is no reason why UG should stipulate that faithfulness in less prominent positions is outranked by faithfulness in more prominent positions if the same pattern emerges through perception, they say. '[B]uilding positional faithfulness into a theory of universal phonology is a misuse, or abuse, of phonetic substance in theory construction' according to Hale and Reiss (200b: 160), and Blaho (2008: 4) agrees. As she puts it: 'it would be superfluous to duplicate this 'knowledge' and build it into our model of phonology'.

The conclusion Hale and Reiss (2000b: 162) draw from the above example (and their reanalyses of *r*-insertion in Massachusetts English (McCarthy 1993) and opacity effects in Hebrew spirantisation (McCarthy 1996), for that matter) is that:

> the best way to gain an understanding of the computational system of phonology is to assume that the substance of phonological entities is *never* relevant to how they are treated by the computational system, except in *arbitrary, stipulative ways*. What this means is that many of the so-called *phonological universals* (often discussed over the rubric of markedness) are in fact epiphenomena deriving from the interaction of extragrammatical factors like acoustic salience and the nature of language change. […] We propose extending the Saussurean notion of the arbitrary nature of linguistic signs to the treatment of phonological representations by the phonological computational system. Phonology is not and should not be grounded in phonetics since the facts that phonetic grounding is meant to explain can be derived without reference to *phonology*.

I think it's safe to say that Hale and Reiss' (2000b) argument for a phonology that is substance-free (due to their demonstration that the markedness tendency seen in Shona verbs can be explained on phonetic grounds) is a convincing one – duplicating what's going on outside of the grammar inside it is a waste of time and space, surely, and so would be avoided by a computational system at all costs.

Recall Carruthers' (2006a) argument from computational tractability in Chapter 3. The mind, including the grammar and the component parts of it, are computationally realised, he says, and all computational processes must be suitably tractable so that they can be carried out within finite time (Carruthers 2006a: 44):

(1) The mind is computationally realized.

(2) All computational processes must be suitably tractable.

In computational phonology, the field that approaches the study of sound patterns from a computational perspective, problems are classified into two types: tractable problems and intractable problems (Heinz 2011a, b). According to Heinz (2011a: 9):

> Problems are tractable iff there is a algorithm which implements this function in fewer than f(n) steps where f is a polynomial function and n is the length of the input (this input is the problem instance in some string-based representation). Decidable problems which cannot be so bounded are intractable.

He also stresses, though, that the variables of *restrictiveness* and *complexity* affect computational tractability as well. The more restrictive a computational system, he says, the less complex it is. And the less complex a computational system, the more tractable it is.

Now, OT has been criticised by some (e.g. Idsardi 2006; Vaux 2008) for its computational complexity as it is, Iosad (2013: 47) points out. Idsardi (2006) and Vaux (2008) have each criticised OT for being too complex, and therefore not tractable enough. Of course, the smaller the set of constraints that exist in phonology, the more restrictive the phonological computational system would be. The more restrictive it would be, the less complex it would be, and the less complex it would be, the more tractable would be.

I'm inclined to argue, then, that if it is the case that phonology is a computational system, and it's imperative that computational systems are tractable, then it wouldn't express patterns with phonological constraints that are already expressed by phonetics, because that isn't being very restrictive at all. A computational phonology would not make more work for itself by handling something grammatically that's already being handled extra-grammatically if it's aim was to be tractable, for that would be the opposite of restrictive (or as Carruthers 2006a puts it, frugal) – that would be extravagant, lavish, wasteful. The fewer constraints a phonological computational system has the better. For the more constraints it has, the less restrictive → more complex → less tractable it is.

John Ohala has similarly argued that markedness tendencies (for similar reasons) should be explained phonetically as opposed to phonologically. For an extensive exploration, one should see Ohala (1998). Various work on learnability owing to Boersma et al. (2003), Escudero and Boersma (2003), Boersma (2006, 2007), Apoussidou (2006) and Boersma and Hamann (2007) strongly supports Hale and Reiss' (2000) claim too; their studies showed that rankings can be derived from phonetic data during language learning as well (Blaho 2008).

2.2.4 Diachrony

A fourth and final existing argument for substance-free phonology to be considered in this thesis concerns diachrony. Although diachronic change is not (usually) assumed to play a part in synchronic linguistic competence, there is no denying the fact that it is related to the synchronic system's functioning in some way, as Iosad (2013) says, and so I consider it here. Classical generative phonologists (Chomsky and Halle 1968), diachronic generative phonologists (Kiparsky 1965, 1968; King 1969), natural phonologists (Stampe 1972; Donegan and Stampe 1979), natural generative phonologists (Vennemann 1972a, b, 1974; Hooper 1976), phonologists interested in variation and sound change in progress (Labov 1971; Labov et al. 1972), those concerned with phonetic explanations of phonetic patterning and sound change (Ohala 1974, 1981; Thurgood and Javkin 1975; Hombert et al. 1979), and researchers working on intrinsic and extrinsic variations in speech (Wang and Fillmore 1961; Chen 1970; Mohr 1971) have been at great debate about the relation between synchrony/diachrony and phonetics/phonology, and for many (dating as far back as Baudouin de Courtenay (1972), actually) 'the original cause of the emergence of alternants is always purely anthropophonics' Baudouin de Courtenay (1972: 184); they believe that phonological structure is derived from phonetic substance.

Hyman (1972, 1976), a phonologist who considers himself to sit somewhere in between generative phonology and the structural phonology of the Prague school of thought (e.g. Trubetzkoy 1939; Martinet 1960) is concerned, in particular, with a process he calls phonologisation, which refers to the change of a phonetic property into a phonological one. It is thought by some phonologists (e.g. Iosad 2013) that the theory of phonologisation counts as evidence for a substance-free phonology, and I can certainly see why.

Spanning the years of 1972 to 1976, Hyman gave a number of definitions for phonologisation, including, but not limited to, these ones:

> A universal phonetic tendency is said to become 'phonologized' when language-specific reference must be made to it, as in a phonological rule (Hyman 1972: 170).
>
> *phonologization*, whereby a phonetic process becomes phonological […] (Hyman 1975: 171).
>
> […] what begins as an intrinsic byproduct of something, predicted by universal phonetic principles, ends up unpredictable, and hence, extrinsic. (Hyman 1976: 408).

But what *exactly* would phonologisation look like, one might wonder? In *Phonological Involvement in Phonetic Change,* Fruehwald (2013) paints a picture.

2.2.5 Gradual phonologisation

According to Fruehwald (2013), two things have to do with phonologisation: a phonetic process called coarticulation and a phonological one called differentiation. To exemplify these processes (and how they play a part in phonologisation), he brings evidence from a hypothetical sound change to a vowel /V/, which appears in two totally different segmental contexts /__ x/ and /__ y/. I summarise his explanation in the exposition that follows.

Fruehwald (2013) presents the distributions of [Vx] (in red) and [Vy] (in blue) in F1 x F2 vowel space as in Figure 4.1, where F1 stands for the first formant of vowel space (which is inversely related to vowel height) and F2 the second formant (which has to do with the degree of backness of a vowel), according to Ladefoged (2006). A formant, Ladefoged (2006) says, is the concentration of acoustic energy that surrounds a particular frequency in a speech wave. Each formant corresponds to a certain resonance in the vocal tract, and we can distinguish one vowel from another by the differences they have between these overtones (e.g. we could distinguish /i/ and /u/ by their F2 values (backness) seeing as /i/ is a high front vowel and /u/ a high back one, and /i/ from /æ/ in terms of their F1 values (height) given that /i/ is a high front, and /æ/ a low front, vowel).

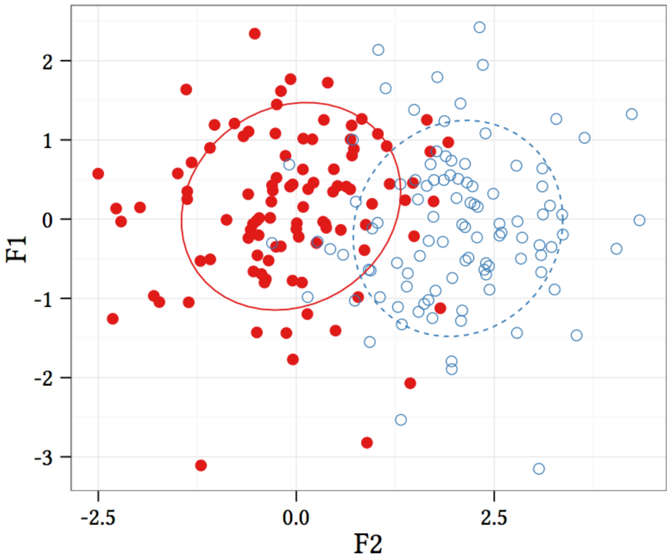

Figure 4.1 Distribution of contextual variants ([Vx] and [Vy]) of a hypothetical vowel (Freuhwald 2013: 61)

As you can see, Figure 4.1 clearly demonstrates the difference in [Vx] and [Vy]'s articulation – [Vy] is produced further back than [Vx]. However, there are a couple of ways in which [Vx] and [Vy] could have been generated, Fruehwald explains. On the one hand, it could have been through phonological differentiation in that /y/ could have spread some feature *f* (say, [+ back]) onto /V/, creating a featurally distinct and therefore phonologically different allophone of /V/:

(1) $V \rightarrow V_f$ / __ y

In this case, [V] (in red) and [V_f] (in blue) would have independent targets for phonetic implementation (as is depicted in Figure 4.2):

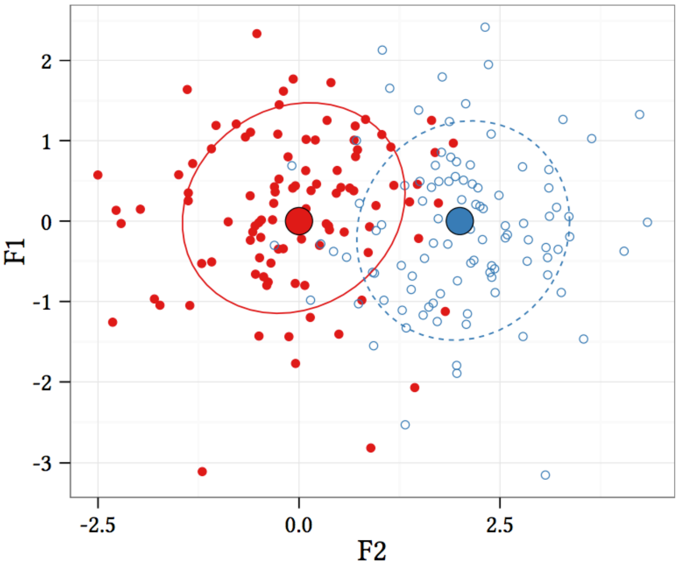

Figure 4.2 Independent targets of phonetic implementation produced by phonological differentiation (Fruehwald 2013: 62)

On the other hand, however, it might have been the case that phonology wasn't involved at all and rather, that there was only one mapping to the phonetic target [Vx] (from its underlying representation /V/), and [Vy] was produced due to a coarticulatory pressure being placed on [Vx] by [y], pulling its production back. In this case, [Vx] and [Vy] don't have independent targets for phonetic implementation:

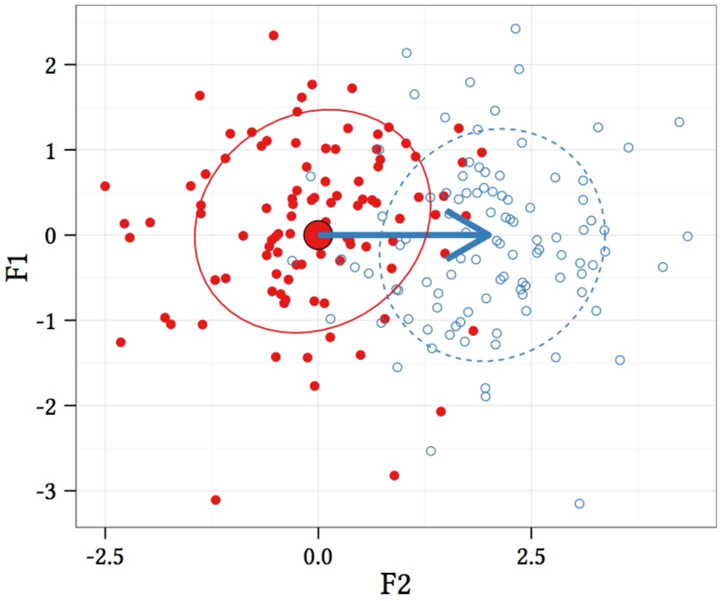

Figure 4.3 The effect of coarticulation on shifting productions from intended targets (Fruehwald 2013: 63)

Now, it should be clear from Fruehwald's (2013) example, at this point, why phonetic coarticulation could be reanalysed as phonological differentiation – and that's what phonologisation is. Before they undergo diachronic change, you see, the ways in which [V] and [Vx], and [V_f] and [Vy] are realised in F1 x F2 vowel space look more or less the same. When [V] and [V_f] and [Vx] and [Vy] change diachronically, however, a difference can be observed.

Firstly, in the case of phonological differentiation [V] and [V_f] have independent targets of phonetic implementation, so it is possible for them to have separate diachronic trajectories as in Figure 4.4. In the case of phonetic coarticulation, however, the realisation of [Vy] is yoked to that of [Vx]. What this means, is that the *diachronic trajectory* of [Vy] is yoked to that of [Vx] too, and so the same thing has to happen to each of them (as in Figure 4.5):

Compare the two:

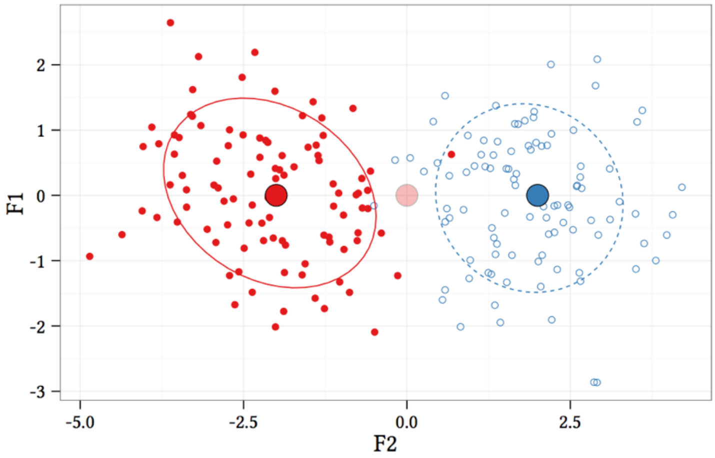

Figure 4.4 The interaction of phonological feature spreading and diachronic phonetic change (Fruehwald 2013: 64)

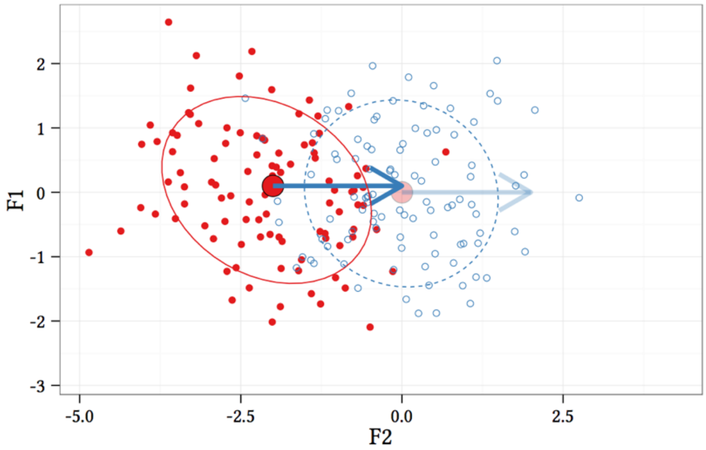

Figure 4.5 The interaction of phonetic coarticulation and diachronic phonetic change (Fruehwald 2013: 64)

Figure 4.4, Fruehwald (2013) explains, illustrates the interaction between phonological feature spreading and diachronic change. The data represents an independent shift of [V] frontwards along F2; [V_f] does not move. In Figure 4.5, which illustrates phonetic coarticulation's interaction with diachronic phonetic change, [Vx] shifts frontwards along F2, and so too does [Vy] given that it's yoked to [Vx], it is explained by Fruehwald (2013). This is interesting because phonologisation is, in fact, the reanalysis of a difference between two segments being due to phonetic coarticulation as being due to a phonological process with featurally distinct objects, Fruehwald (2013) says and, through diachronic change data, we can pinpoint precisely when that phonologisation takes place.

The effects of the reanalysis of phonetic coarticulation as phonological differentiation are illustrated by Fruehwald (2013: 68) with the following figure:

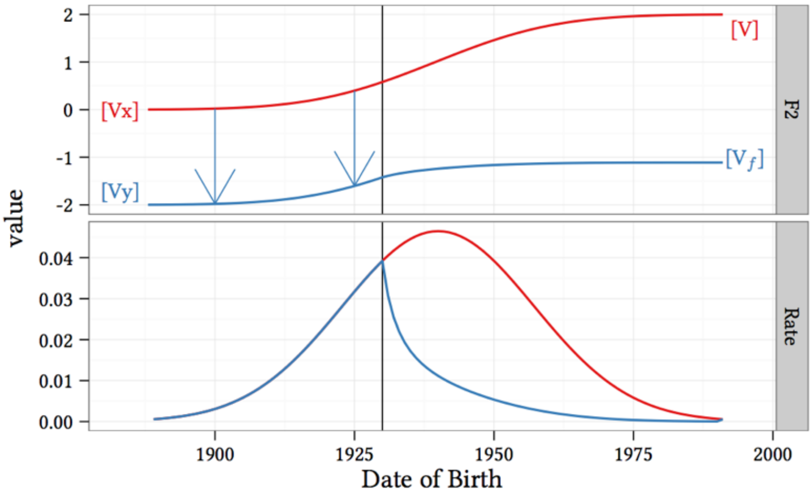

Figure 4.6 The reanalysis of phonetic coarticulation as phonological feature spreading, and its effect on the rate of phonetic change

In the top graph in Figure 4.6, you can see that [Vx] and [Vy] undergo the same change until roughly 1930, which means that up until that point the difference between the two must have been due to phonetic coarticulation (the trajectory of [Vy] was yoked to [Vx], which meant that they each underwent the same change). At around the year of 1930, however, you can see that the former ([Vx], now [V]) continues on along its previous path being affected by the change, whereas the latter ([Vy], now [V_f] does its own thing because affected it is no longer. This is because, at this point, the phonetic effect was reanalysed as being phonological, and so the process entered the grammar creating two featurally distinct allophones [V] and [V_f] which had independent targets of phonetic implementation, and therefore independent trajectories of change.

For representative examples of real phonologisation at play one might want to read Barnes' (2006) work on vowel neutralisation or Kingston's (2007) on tonogenesis – but for now, it should suffice to say that phonologisation could also count as evidence for a substance-free phonology, for the difference between phonological and phonetic patterns establishes the existence of two different domains of grammar (Iosad 2013).

2.3 Alternative arguments for a substance-free phonology

2.3.1 Spontaneous phonologisation

Now, with all that about gradual phonologisation being said in section 2.2.5, I should probably point out that if gradual phonologisation could count as evidence for a substance-free phonology, then Fruehwald's (2013, 2016) argument *against* gradual phonologisation in favour of one *for* spontaneous phonologisation in Philadelphian English could count as evidence that's even better. If his analysis of the phonological pattern is proven to be correct, then it is even more challenging to the view that there isn't a qualitative difference between phonetic and phonological representation and computation than those of the phonologists arguing for gradual phonologisation are. In his dataset, Fruehwald (2013, 2016) found evidence to suggest that the phonological processes which differentiate allophones enter the grammar at the very onset of sound changes, rather than partway through the change as late-stage reanalyses. This means that they cannot simply be the codifications of phonetic effects, he says, because, at the outset of the changes, there were no phonetic effects to be codified.

Canadian /ay/ raising – wherein the diphthong /ay/ surfaces as /ʌy/ before voiceless consonants has been a topic of linguistic interest since the 1940s. A number of researchers have written about raising thereafter, in Canadian varieties of English and other ones. It was this that was the focus of Chapter 5 of Fruehwald's doctoral dissertation which was published in 2013, and also a later paper *The Early Influence of Phonology on a Phonetic Change* which appeared in *Language* three years later in 2016. He analyses /ay/ raising in longitudinal data extracted from the Philadelphia Neighborhood Corpus (Labov and Rosenfelder 2011) which spans the speech of people born between 1889 and 1998, and demonstrates that in the data all /ay/ diphthongs before underlyingly voiceless consonants pattern together, regardless of whether or not their trigger (the proceeding plosive) is voiced on the surface or not.

Fruehwald (2013) found, more specifically, that pre-voiceless /ay/ raising applied before flapped /t/ and /d/ in the same way it did before un-flapped /t/ and /d/, which suggests, he said, that the neogrammarian phonetic change targeted an underlying *phonological* category as opposed to a surface phonetic one. Fruehwald's (2013) findings are clearly really relevant to questions about the contested relationship between phonetics and phonology, then, and so it's important that they're looked at.

Fruehwald (2013: 111) begins his argument by providing the following ordered rule analysis, to show where and how /ay/ raising occurs in Philadelphian English:

Input	***Writer*** ɹaitəɹ	***rider*** ɹaidəɹ
RAISING	ɹʌitəɹ	-
FLAPPING	ɹʌiɾəɹ	ɹaiɾəɹ
Output	ɹʌiɾəɹ	ɹaiɾəɹ

Table 4.1 Opaque interaction between /ay/ raising and flapping

As you'll see from the table above, /ay/ raising applies opaquely with respect to flapping, Fruehwald (2013, 2016) argues. Whilst words like *write* which have voiceless codas are produced with the raised variant of the vowel /ʌy/, words like *ride* which have voiced ones are produced with the variant that is not raised (i.e. [ay]). /ay/ raising also occurs before flapped /t/, but not flapped /d/. So a word like *writer* in which the /t/ is flapped will surface with the higher diphthong, whereas for a word like *rider* with a flapped /d/, /ay/ raising will not apply.

It therefore looks like raising is phonological, he says. That it is triggered before a flapped /t/ which is voiced on the surface, only because the /t/ is voiceless underlyingly. He argues that it is not how the trigger is realised phonetically that is relevant to this sound change, but rather, the voicing of the underlying representation.

Some phonetic arguments have been made for /ay/ raising, however, (e.g. those of Joos (1942) and Chambers (1973) and Moreton and Thomas (2007)), and so Fruehwald (2013) carefully considers those explanations, in order to see whether the way in which /ay/ patterns in the Philadelphian data set can be attributed to phonetic pressures after all. Pre-voiceless vowel shortening is the phonetic precursor for pre-voiceless /ay/ raising that's most commonly argued for in the literature, and so he began with that one.

According to Joos (1942) and Chambers (1973), /ay/ diphthongs are long gestures that are shortened before voiceless segments and so in order to shorten the diphthong, they argue, a speaker may well raise the nucleus of it to reduce gesture length. To test Joos' (1942) and Chambers' (1973) hypotheses that /ay/ raising is due to duration, Fruehwald (2013) assessed the median duration (in msecs) of /ay/ before flapped /t/ and surface /t/ and flapped /d/ and surface /d/ after putting them in order of shortest to longest (as in Table 4.2):

Segment	Context	Median duration (msecs)
/t/	Flapping	111
/t/	Surface	144
/d/	Flapping	156
/d/	Surface	237

Table 4.2 Median /ay/ durations by context (adapted from Fruehwald 2013: 117)

As the table shows, /ay/ before flapped /t/, surface /t/ and flapped /d/ form a set of shorter distributions, and /ay/ before surface /d/ forms another, longer one. If Joos (1942) and Chambers (1973) were right in what they were saying, that /ay/ raising is phonetically conditioned by diphthong duration, then this would predict flapped /t/, surface /t/ and flapped /d/ all participate in raising, Fruehwald (2013) points out. However, since /ay/ raising only occurred before flapped and surface /t/s in the Philadelphian data, it is evident that their thesis is incorrect, he says.

A second phonetic conditioning hypothesis comes from Moreton and Thomas (2007). They, like Fruehwald (2013), make an argument against Joos (1942) and Chambers' (1973) prevoiceless shortening account, pointing out that in dialects in which /ay/ is monophthongised, the monophthongisation is actually *least* advanced before voiceless segments, meaning it's at its longest in this context and therefore at its least likely to raise as a result of being shortened. They hypothesise alternatively that the glide is peripheralised in pre-voiceless contexts, a hypothesis which cleverly captures, Fruehwald (2013: 116) says, 'both the coarticulatory pressure to raise the nucleus towards the glide, and the resistance to monophthongization'.

Fruehwald (2013) considers this phonetic conditioning explanation for /ay/ raising too, but since /ay/ doesn't raise before /r/ in Philadelphia, he has to do so with data derived from Rosenfelder (2005), who studied /ay/ raising in Victoria, British Columbia. The ways in which surface /t/ and /d/ and flapped /t/ in Rosenfelder's (2005) data are given in Figure 4.7 below:

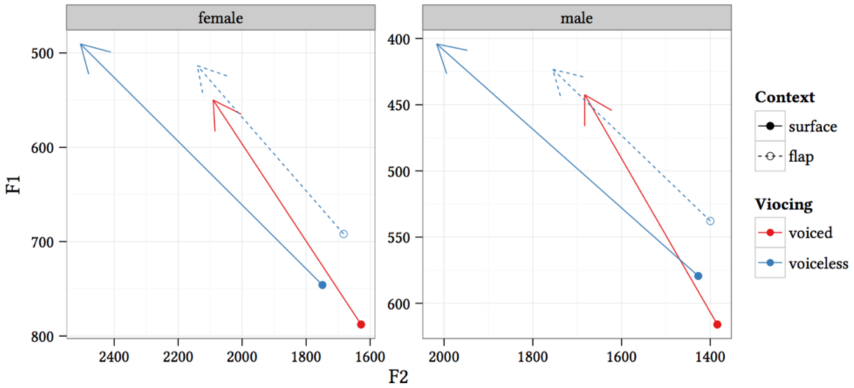

Figure 4.7 Nucleus to glide trajectories in Victoria, British Columbia (Fruehwald 2013: 199)

Now, whilst it's quite clear from the figure that glide targets are more peripheralised before the voiceless obstruents than the voiceless one to a degree that isn't in proportion to the heights of the nuclei they proceed (which would seem to support the glide peripheralisation hypothesis) in Victoria, British Columbia, it also predicts something quite different from what we see in the Philadelphian data, Fruehwald (2013) figures.

If flapped /d/ was less peripheralised than surface /d/ (in the same way that flapped /t/ is less peripheralised than surface /t/ and as the peripheralisation hypothesis would predict) and flapped /d/, therefore, had a more similar glide target to surface /d/ and flapped /t/ than surface /t/, Fruehwald (2013) points out, then the glide peripheralisation hypothesis would predict that only surface /t/ (the most peripheralised of all) would undergo raising. We can see from Figure 4.8 though that this is not the case – raising applies to both variants of /t/ (Fruehwald 2013: 121):

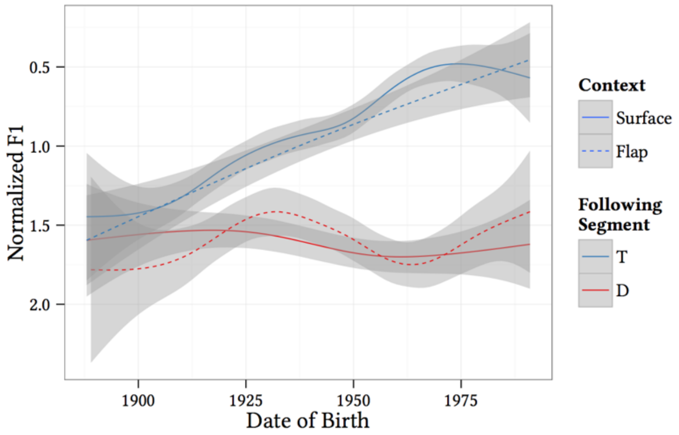

Figure 4.8 /ay/ height by date of birth and context

The trajectories in Figure 4.8 (which plots /ay/ height by date of birth and contexts (before surface and flapped /t/ and surface and flapped /d/), as you can see, match neither the predictions of the duration precursor hypothesis (which predicts that flapped /t/, surface /t/ and flapped /d/ all participate in raising) nor the glide peripheralisation hypothesis (that only surface /t/ does), then, for n this dataset 'the height of /ay/ appears to pattern according to the underlying phonological voicing of the following segment, with {surface /t/, flapped /t/} patterning together, and {surface /d/, flapped /d/} patterning together', with surface and flapped /t/ undergoing raising, and surface and flapped /d/ not undergoing it (Fruehwald 2013: 120).

The fact that /ay/ raising applies to underlyingly voiceless /t/ and /ɾ/, despite their surface realisations, Fruehwald (2013) argues, points to this being a phonologically conditioned change, not a phonetically conditioned one. This is, of course, a qualitative conclusion, however, and so he ran his data through Stan (a probabilistic programming software) to see whether this argument was one that could be supported by statistics.

Fruehwald (2013) plotted the estimated F1 trajectories for /ay/ in each context faceted by from (surface or flapped) together with 95% highest density posterior (HPD) intervals (i.e. the coloured bands in the graph) which specified a probability of 95% that the true value for each context lay within the coloured band associated with it. The potential scale reduction factor ($\hat{R}$) is less than 1 for this dataset Fruehwald (2013) says, which suggests that the model adequately depicts the data being analysed:

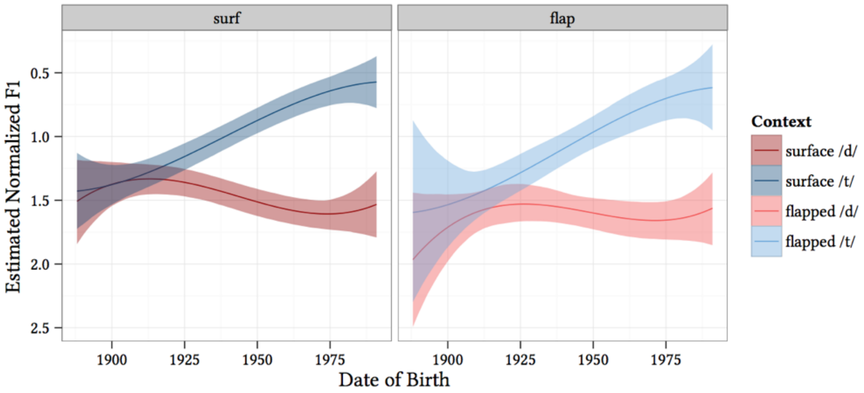

Figure 4.9 Model estimates of /ay/ height, faceted by surface vs. flapped realisations

Unlike the hypothetical example of gradual phonologisation in which [Vx] and [Vy] underwent a change together due to a phonetic effect until around the time of 1930, at which point the change was reanalysed as being phonological, and two featurally distinct allophones [V] and [V_f] were formed and underwent change separately, phonologisation was observed at the outset with ay-raising, Fruehwald (2013) argues.

You can indeed see quite clearly from Figure 4.9 that surface /t/ and flapped /t/, and surface /d/ and flapped /d/ have always had independent trajectories. And, in Figure 4.10, which plots the same trajectories but faceted by the underlying stop to see whether it is this, specifically, that affects naming, you can see why Fruehwald came to this conclusion even more clearly. /ay/ before /t/ (as both a surface form, and as the underlying form of flap)'s estimated normalised F1 value lowers over time (indicating that it raises in height), whereas /d/ (in both its surface and flapped underlying forms) does not:

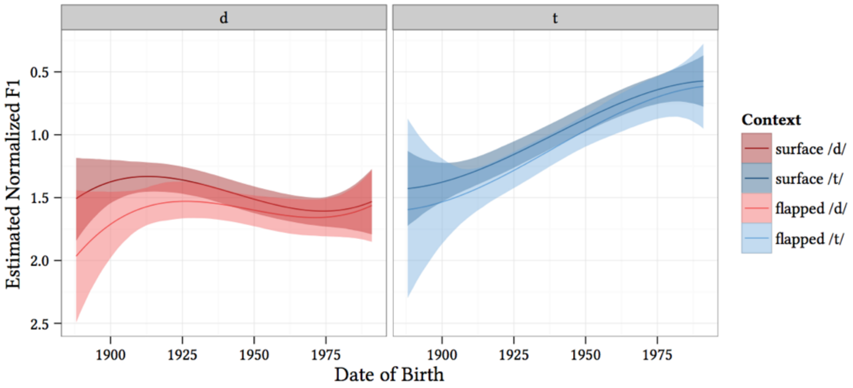

Figure 4.10 Model estimates of /ay/ height, faceted by /t/ vs. /d/

The fact that /ay/ before /t/ and /ay/ before /d/ follow different trajectories from the get-go, suggests, Fruehwald (2013) says, that the change didn't bring to bear on phonetics during the change at all, and so phonological factors have greater explanatory power than phonetic ones in the case of /ay/ raising. He suggests that the phonological differentiation of /ay/ as high or low is due to a grammatical process that entered the phonology before the change came about. These results, of course, pose a challenge for the commonly accepted view that phonologisation (the process by which a phonetic pattern becomes a phonological one, recall) occurs gradually and, as such, has important implications for theories of phonology – including the one of interest to this thesis that is the substance-free framework.

Fruehwald's (2013) account of /ay/ raising is clearly incompatible with theses in which the boundary between phonetics and phonology is blurred like phonetically grounded phonology ones, because the change, to his mind at least, had nothing to do with phonetics. It is not incompatible with the substance-free phonology thesis though of course, as according to this one phonological knowledge is not simply the codification of phonetic patterns, but rather something more abstract. As such, I think it's safe to say that Fruehwald's (2013) work can count as strong evidence against the former, and for the latter.

Note though that most of this discussion thus far has been about what Fruehwald (2013) has said *didn't* happen to /ay/. Evidence from his 2013 dissertation demonstrated that /ay/ raising was likely neither (a) phonetically conditioned by diphthong duration as Joos (1942) and Chambers (1973) hypothesise, and nor was it (b) phonetically conditioned by glide peripheralization (which was what Moreton and Thomas's (2007)) offered as an explanation. Rather, Fruehwald (2013) argues, /ay/ raising is a *phonologically* conditioned change.

He explains that if two variants of a speech sound are to diverge in their phonetics over time, then that must be because they are treated as quantitatively different categories (that is, as different allophones) by speakers from the moment that they begin to diverge. He argues that the split of /ay/ into two allophonic categories is *not* the reanalysis of a phonetic change (as would be the case if this was phonetic coarticulation being reanalysed as phonological differentiation). Rather, that the longer term phonetic change is only possible precisely *because* /ay/ split into two allophones before the phonetic change began. This split is what allowed for their phonetic targets to diverge phonetically according to the voicing specification of the following segment, flapped /t/ or flapped /d/, he says, with one undergoing a gradual (phonetic) change in height (before flapped /t/) and the other remaining low (before flapped /d/).

If this is the case though, then the next question that arises is this one: how did those two categorically different variants of /ay/ come to exist in the first place to be conditioned by flapping? An answer is provided by Bermudez-Otero (2017) and so I discuss that here.

2.3.2 The enhancement-of-clipping hypothesis

An alternative (although related) phonological analysis of the pattern (that I'm more inclined to concur with because the analysis can explain /ay/ raising in a number of other varieties of English including Canadian and Scottish[5]) is the one offered by Bermudez-Otero (2017). Although different from Fruehwald's (2013, 2016), this argument is still consistent with a modular architecture of the mind in which phonology free of phonetic substance in that there are actually *two* different types of process involved in /ay/ raising – pre-fortis clipping and flapping which are phonological, and the raising of the vowel itself, which is phonetic.

Pre-fortis clipping, Bermudez-Otero (2017) argues, is a categorical phonological process that applies at the *stem-level* of phonology, and therefore underapplies when the voiceless segment that conditions it belongs to a suffix at the word-level. Since flapping occurs across word boundaries (e.g. 'sit in the park' 'si[ɾ] in the park' (Kaisse and Shaw 1985)), this must be a post-word level process (Kiparsky 1985; Kaisse and Shaw 1985) he says, occurring at the *phrase-level* of phonology. He argues furthermore, that since phrase-level processes occur after stem-level ones, flapping applies after clipping.

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[5] The analysis offered by Bermudez-Otero explains not just /ay/ raising in Philadelphian English, but /ay/ raising in Ontario, Canada (see Chambers (1973, 1989, 2006); Chambers and Hardwick (1986); Thomas (1991) Rosenfelder (2007) and in Scotland (see Aitken (1981), Agutter (1988), McMahon (1991)) – for a more nuanced discussion of how prefortis clipping affects /ay/ raising in these varieties of English, one should consult Bermudez-Otero (2017).

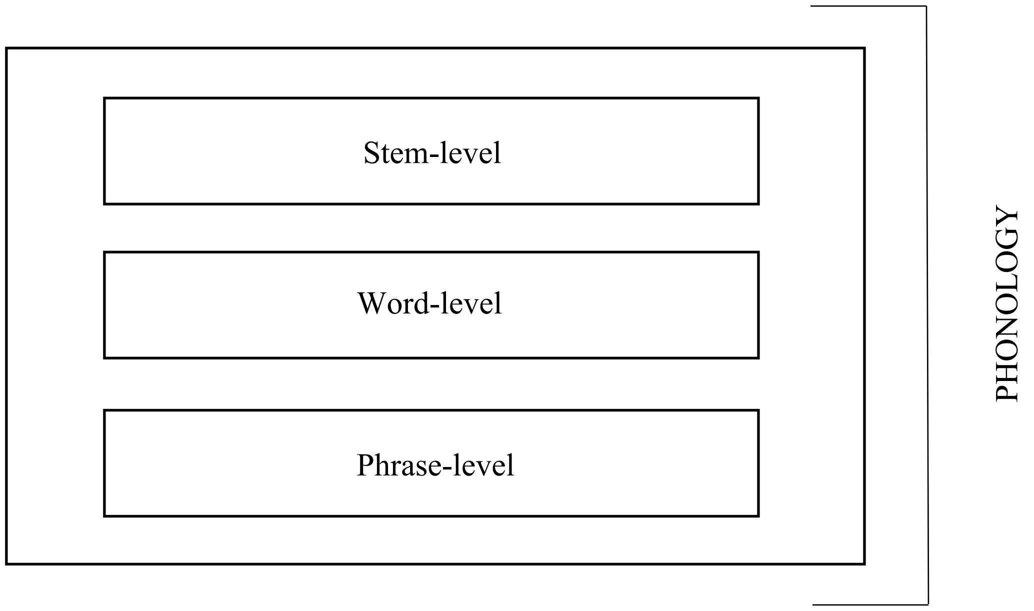

Figure 4.11 The stem-level, word-level and phrase-levels of phonology (adapted from Bermudez-Otero and Trousdale 2012: 700)

Raising, Bermudez-Otero (2017) argues, is a phonetic process that applies opaquely with respect to *clipping* (not flapping as Fruehwald (2013, 2016) believes to be the case). It is the clipped allophone only which undergoes the /ay/ raising change under his proposal, and therefore the rule for raising is:

not aɪ → ʌi / __[-voice]
but ăɪ → ʌ̆i

As is exemplified by the following set of data:

		rider	*writer*	*idle*	*title*
Stem level	(clipping)	ɹaɪd	ɹăɪt	aɪdəl	tăɪtəl
Word level		ɹaɪdəɹ	ɹăɪtəɹ	aɪdəl	tăɪtəl
Phrase level	(flapping)	ɹaɪɾəɹ	ɹăɪɾəɹ	aɪɾəl	tăɪɾəl
Phonetics	(raising)	ɹaɪɾəɹ	ɹʌ̆iɾəɹ	aɪɾəl	tʌ̆iɾəl

Table 4.3 Derivations in early 20th-century Philadelphia (adapted from Bermudez-Otero 2017: 8)

Now, whilst this weakens Fruehwald's (2013) argument for this sound change being 'phonologisation' that occurs spontaneously as opposed to gradually, it importantly *preserves* his core argument that a phonetic change operated over surface phonological representations. Bermudez-Othero (2017) proposes that a pre-existing phonological process (pre-fortis clipping) created two different allophones of /ay/, [ai] and [ăi], before /ay/ began to raise phonetically. The phonetic change that resulted in the raising of pre-voiceless /ay/, he says, targeted the clipped allophone ([ăi]) only.

This analysis makes more sense, to my mind, given that pre-fortis clipping and /ay/-raising are observed elsewhere in identical environments – consider, for example, the distribution of /ay/ raising in Scottish English (Bermudez-Otero 2017: 13-14):

In Scottish English, /aɪ/, among other vowels, is…

- clipped before all consonants other than voiced continuants,
  e.g. *sign, side, life, sight*
- unclipped elsewhere
  e.g. *sigh, dive*

If it's the case that raising targets only clipped tokens of /aɪ/, it would follow that in Scottish English, raising would only target tokens of /aɪ/ that have been clipped by the Scottish Vowel Length Rule (SVLR) (and would do so even when those tokens are followed by a voiced consonant).

Evidence for this has indeed been found by (Scobbie et al. 1990: 241):

• [ʌ̆i]	clipped by the SVLR	and so raised:	*sign, side, life, sight*
• [aɪ]	unclipped by the SVLR	and so unraised:	*lie, alive*

Bermudez-Otero (2017) then, like Fruehwald (2013, 2016), importantly draws a distinction between phonological processes (clipping and flapping, in this instance) and a phonetic process (that is, raising) in his account of the diachronic change in Philadelphian English. This analysis (unlike analyses of the change which attribute it to phonetic conditioning) is, of course, consistent with a modular feedforward architecture of the grammar in which phonology precedes phonetics.

This analysis importantly demonstrates a difference between changes that occur in a phonological module (in which clipping and flapping apply) and a phonetic one (where raising applies). This is supportive of the substance-free framework of phonology, which separates phonology from phonetic substance.

3. Concluding remarks

As you can see, there is a wealth of evidence pointing towards phonology being substance-free. In section 2.2 I explored some existing arguments for a substance-free phonology, including explanatory adequacy in 2.2.1, multiple modalities in 2.2.2, emergence in 2.2.3, diachrony in 2.2.4 and gradual phonologisation in 2.2.5.

After that, I examined the extent to which Fruehwald's (2013) account of /ay/ raising as being due to spontaneous phonologisation, not gradual phonologisation, could count as even stronger evidence for a substance-free phonology and concluded that, if correct, it could. Since phonetic change for the variants within that vowel category were best explained in terms of phonological allophony than in phonetic disposition, it is easier to see how this could be the case if phonology wasn't grounded in phonetics than if it was. The Philadelphian data was suggestive that phonology can bring about change without making any reference at all to phonetics, and more specifically that the divergent pattern of change is best attributed to categorical allophones that were created by the phonology at the outset of it, which is of course what a substance-free phonology thesis would predict.

However, as I point out in the later subsections of this Chapter I myself actually subscribe to an alternative analysis of the pattern, the one owing to Bermudez-Otero (2017), which although different from Fruehwald's (2013) is still supportive of a substance-free view of phonology. Bermudez-Otero (2017) also argues for separate phonological (clipping and flapping) and phonetic (raising) processes in his analysis, which is very much in alignment with a substance-free framework of phonology which separates phonological processes from phonetic ones, with phonological processes occurring inside the phonology, and phonetic processes outside of it.

Chapter 5: A Massively Modular Approach to Phonology

1. Introduction

The chapter examines the extent to which phonology can be considered domain specific and therefore a module of the mind by massive modularists' (in particular Carruthers 2006a's) standards. To do so it considers the effects semantically- and phonologically-targeted treatments have on phonological anomia (a disorder of naming that has to do with damage to the phonological level of language processing) under the rationale that if it is the case that the phonological processor is a domain specific module of the mind, it must be able to operate only on input relevant to its function, that is, phonological information. If semantically-targeted treatments were found to facilitate word retrieval in people with phonological anomia, then this could count as evidence that the phonological processor is ammodular, for it would demonstrate an ability of phonology to operate on semantic information (i.e. input that is *irrelevant* to its function), I argue.

An understanding of the complex cognitive system that's involved in spoken word production is a prerequisite for a proper understanding of the disorders of naming that are observed in aphasia, Raymer (2005) rightly points out, as they are actually caused by damage that disrupts that system's activity. Many models of spoken word productions' details have been subject to great debate, but in spite of this, there is a general agreement among researchers that there are two stages (semantic and phonological) involved in the word retrieval process (Caramazza et al. 2000; Herbert 2004), though, as I point out in section 2.4.1 (2.4.1.1 specifically) syntax is involved as well. Researchers also generally agree that subsequent to these stages exist post-lexical processes in which speech production is planned (Raymer 2005).

The current cognitive approach to the assessment of and intervention into naming disorders in aphasia locates language breakdown in normal models of language processing at one, some or all of these levels, and so the chapter will begin with a detailed discussion of those models. I also present my preferred model (one that isn't currently referenced in the clinical literature, that of Jackendoff (2002)). This model, in my opinion, better explains how and where syntax plays a part, where the lexicon (and the lexical entries) that make it up is stored, and how modules are connected to and communicate with one another. There are number of problems with the models of word retrieval referenced by clinicians, and Jackendoff's (2002) solves them all, I point out. I also, importantly, explain how word retrieval can be handled by

Jackendoff's (2002) model of the language faculty. It is not just a model of competence, but of performance too, I point out. And in my eyes, is the most superior of all argued for so far.

I begin by outlining leading theories and areas of agreement and disagreement among the theorists each model is due to, before assessing the adequacy of their abilities to account for the word retrieval errors we see in normal (in section 2) and aphasic (in section 3) patients. In section 4 though (which is the largest one of all) I turn my attention to anomia therapy, and both qualitatively and quantitatively consider what the results of 43 studies conducted between the years of 1980 and 2002 have to say about the domain specificity and modularity of phonology, which is that it is neither domain specific nor modular. My results show that phonology can operate on semantic information, in that semantic therapy is able to treat phonological anomia. Section 5 concludes.

2. Models of spoken word production

Cognitive models of word retrieval paint a picture of how the processes involved in it are organised, and as a result, are clinically relevant in that they provide a framework for the formulation of testable hypotheses about language behaviour (Laine and Martin 2006). Data that has been used to test these hypotheses include the speech errors that are produced by normal speakers (Fromkin 1971) and people with aphasia (Dell et al. 1997), as well as analyses of tip of the tongue states in normals and aphasics (Brown and McNeill 1966), as section 2.2.1.1 and (briefly) 2.2.4.1 explore. The evidence from each of these sources has made a significant contribution to the development of cognitive models; without speech error and tip of the tongue data, we wouldn't know anywhere near as much about spoken word production as we do today.

Current cognitive models of word retrieval have their origins in the neuroanatomical models of language breakdown in aphasia that were proposed by 19th century neurologists (Laine and Martin 2006). In 1825, for example, Jean-Baptiste Bouillon distinguished between two types of language production impairments: one that affected ones' underlying knowledge of words, and one that affected the planning and execution of the speech movements that were required to produce them (Laine and Martin 2006). A similar distinction was drawn by Jacques Lordat in 1843, and by 1865 the third frontal convolution had been identified by Broca as being the site of expressive language (i.e. Broca's area). By 1874 Wernicke had linked language comprehension to the left superior temporal lobe's posterior two-thirds (i.e. Wernicke's area) (Laine and Martin 2006).

Broca's (1865) and Wernicke's (1874) investigations into neuroanatomical sites of language acted as the foundations on which the more detailed models of the mental operations that underlie language production were built. In fact, it was Wernicke's (1874) work together with that of Lichtheim (1885) that led to the first fully-fledged model of the language system: the Wernicke-Lichtheim model (which was, rather self-explanatorily, named after the pair). The Wernicke-Lichtheim model had interesting applications to aphasiology for two reasons: on the one hand, because it identified the component parts of the language processor (centres for auditory images (A), motor images (M) and concept elaboration (C) as well as auditory perceptual (a) and motor encoding (m)), and because it described different profiles of aphasia in terms of disruption (see the slashes with numbers) to those components or to the spaces between them, they said (Laine and Martin 2006).

A lesion at site (A) in Wernicke-Lichteim's model, which is Wernicke's region, would result in Wernicke's aphasia whereas if there were a lesion at site (M) (which is Broca's region), Broca's aphasia would result. This is because A, is where auditory word images are processed (Laine and Martin 2006) (the hence comprehension problems) and Wernicke's aphasia), and (M) where motor images are processed (Laine and Martin 2006) (hence Broca's aphasia which causes problems with production:

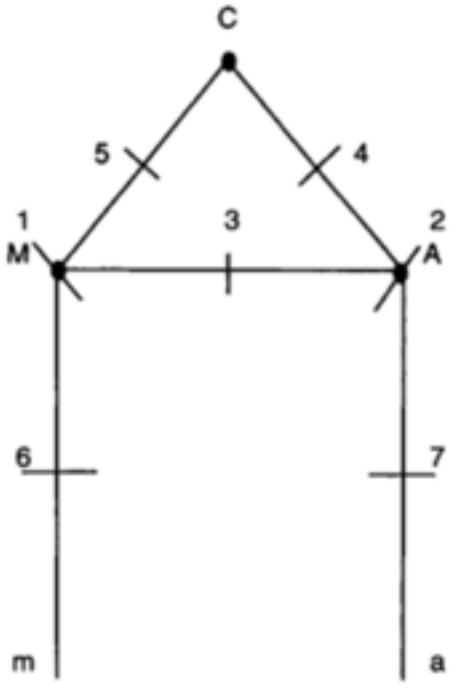

Figure 5.1 A depiction of the Wernicke-Lichtheim model of language processing and the different aphasia profiles accounted by the model (Laine and Martin 2006: 5)

Now, although the Wernicke-Lichtheim model isn't what we would consider particularly descriptive today it was compared to other models in its time, and it was enough so that its assumptions led to the creation of the classical taxonomies of aphasic syndromes that were used in the assessment of and intervention into aphasia in the 60s and 70s (Geschwind 1965; Benson 1979).

Wernicke and Lichtheim's brain-behaviour approach (that is, their belief that language components have neuroanatomical correlates) was being cared for less and less by the 1980s, however. At his point researchers were beginning to develop more functional, as opposed to structural, approaches to the modelling of the language system (Morton and Patterson 1980; Dell 1986; Kay and Ellis 1987; Howard and Franklin 1988). Spoken word retrieval was mostly modelled, by this point, by 'box and arrow diagrams' (e.g. Morton (1970, 1979), Butterworth (1989) (and even Levelt et al. 1999)) which owed their instantiations to analogies with computers. They made use of boxes to illustrate stores of representations (e.g. semantic/phonological/morphological) and arrows the processes between them (e.g. which took the output of one representational store's route as input to the next) (Herbert 2004; Laine and Martin 2006). These functional, box and arrow models have proved useful clinically speaking as diagnostic frameworks in the field of aphasiology (as we'll soon see), for their detailed descriptions of the language processor's components provided clinicians with clear profiles of their patients' spared and impaired processes that enabled them to more precisely pinpoint the language impairments that were underlying the patients' language disorders, and what aspects of language in particular (e.g. semantic/phonological) should be targeted to treat them (Laine and Martin 2006). The functional approach to modelling also provided a vocabulary with which to describe disorders of language in terms of behaviourist phenomena. This is just as important an advance now as it was back then (even if interest in behavioural-neuroanatomical correlates has been brought back by innovations in neuroimaging (Laine and Martin 2006).

A renewed interest in brain imaging studies has led some to question how specified these 'box and arrow' models actually are, and *connectionist* computational models have been developed (through applications of the advances in the computer simulation technology of mental phenomena) to take their place (e.g. Shattuck-Hufnagel 1979; Stemberger 1985; Dell 1986, 1989). You see, whereas box and arrow models group representations together in one or more boxes, connectionist models specify even further than that, representing individual items as nodes (Herbert 2004). These nodes are connected both within *and between* hierarchical levels; and although some still assume that language is processed temporally in one level after the other, others argue that processing can occur in one level at the same time that processing is occurring in others (Herbert 2004). I explore each of those types of models in section 2.2.

However, the relationship between models of spoken word production and the diagnosis and treatment of language disorders is, of course, a co-dependent one. Whilst models of language production can be used to diagnose and treat disorders of language, systematic case studies of language disorders can be (and indeed have been) used to test how well models of language production can account from them. Aphasic speech error data has been used in particular for this – but so too has the speech error data of normal speakers and data from tip of the tongue states in both normal and aphasic speakers as I said earlier – and so this section and the next one will devote discussion to that.

I begin this section with a description of how words are thought to be represented in and retrieved from the mental lexicon by Morton (1970, 1979) in 2.1.1, before pointing out the problems with that approach. I draw on speech error and tip of the tongue data in order to do so, and I have drawn a distinction between Morton's (1970, 1979) model (which just has one step) and those of others (which are made up of two steps) in section 2.2.

The first two-step model I consider (in 2.2.2) is that of Butterworth's (1989) semantic lexicon model (which is similar to Morton's (1970, 1979) model in many ways, except that it incorporates an explicit level of lexical-semantic processing (the semantic lexicon) which the logogen model does not). I then map out Levelt et al.'s (1999) WEAVER++ model (in 2.2.3) before contrasting the pair of feed-forward activation models (Butterworth (1989) and Levelt et al.'s (1999), that is) with Stemberger (1985) and Dell's (1986) interactive activation models in 2.2.5. In section 2.3 I discuss the process by which the phonological information that is retrieved from stored lexical representations is converted into a form that can be used by the articulators for speech, and in doing so cover two well-known slot-and-filler models Shattuck-Hufnagel (1979) and Dell's (1989) interactive accounts of phonological encoding which describe how segments are slotted into syllabic frames in sections 2.3.1 and 2.3.2, respectively.

Some concluding remarks are then made about the adequacy of these models. I point out the questions these models leave unanswered, and how a model like Jackendoff's (2002) is able provide what we're looking for. In particular, I criticise the current models for not explaining the part syntax plays in language processing, and how connections are made and communication is handled between the semantic, syntactic and phonological (what Jackendoff (2002) calls *integrative*) processors. I also highlight how one could and should view the phonology-phonetics/phonetics-phonology *interface* module(s), as well as where the lexicon is and what it does, in particular how it operates in tandem with the rest of the (*integrative and interface*) modules. A more detailed discussion of anomia begins in section 3.

2.1 One-step models

One of the most representative examples of one-step models of spoken word production (and therefore the only one I'll consider in this chapter) is that of Morton (1970, 1979).

2.1.1 Morton's (1970, 1979) logogen model

In Morton's (1970, 1979) logogen model lexical items are represented as logogens, which are said to be counting devices that are put in place 'whenever there is an input of an attribute that matches one of those attributes specified within the defining set for that logogen' (Nickels 1997: 11). In speech production these logogens accumulate conceptual-semantic information from the cognitive system (which is depicted at the top of the diagram) and deliver it to be expressed by the phonological output lexicon (i.e. the first, rectangular box in Figure 5.2), he claims:

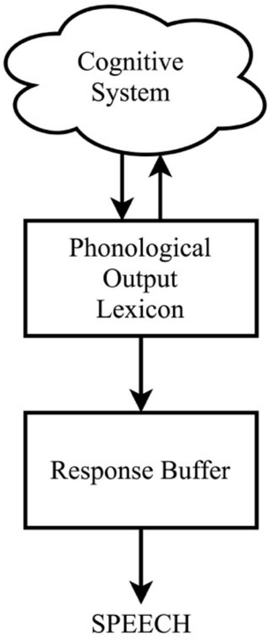

Figure 5.2 The logogen model of spoken word production (Nickels 1997: 11)

Each logogen is assigned a threshold, and when the count of the accumulated conceptual-information exceeds that threshold the logogen that was assigned it ‘fires’, sending a phonological code to the response buffer (i.e. the second, rectangular box) which results in the production of speech.

The single step involved in this process (taking information from the cognitive system and turning it into speech) is what the term ‘one-step’ in one-step models refers to; despite there being two boxes (the phonological output lexicon and the response buffer) there is only one (phonological) process necessary to get from the cognitive system to speech. Now, when one-step models are used in clinical work, many researchers believe that the diagnosis and treatment of aphasia being offered by clinicians using them is unsatisfactory, for it relies on an inadequate interpretation of the problem at hand. To their minds, there are actually two levels of representation (and therefore, two steps that need to be taken in order to retrieve words). As such, many two-step models of spoken word production have been proposed in more recent years (as section 2.2 explores).

2.2 Two-step models

Two-step models postulate two levels of language processing: a semantic one and a phonological one (with semantic representations of lexical items being accessed at the semantic level of language processing and their respective phonological representations from the phonological level of language processing at which instructions for realisations phonetically are given). Evidence for the existence of these levels has been found in the speech of both normal and aphasic speakers. I'll cover evidence from normal speakers in section 2.2.1.1.1 of this chapter, and evidence from aphasic speakers in 2.2.1.1.2.

2.2.1 Semantic and phonological levels

2.2.1.1 Evidence for independent semantic and phonological levels

2.2.1.1.1 Evidence from normal speakers

Speech errors or 'slips of the tongue' are by no means an infrequent occurrence in the everyday speech of normal speakers (Nickels 1997). They are not thought to occur randomly, though. Rather, systematically so, and so over the years, they've been extensively exploited for the insights they can provide about the production of spoken words (Nickels 1997).

2.2.1.1.1.1 Speech errors

Shattuck-Hufnagel (1979, 1987), Stemberger (1985), (Dell 1986, 1989) and (Butterworth 1989), for example, use speech error data as support for the theoretical models of spoken word production they've proposed in recent years that argue for two levels of representation in lexical access as opposed to just the single level of representation that's argued for in Morton's (1970, 1979) one-step logogen theory. The argument for two levels of lexical representation (one organised by meaning and the other organised by sound), they say, is based on the independence of semantics and phonology in errors (as evidenced by targets that are substituted with semantically related but not phonologically related words and those that are substituted with phonologically related words but not those that are related semantically) such as those in the following examples that were put forward by Fromkin (1971: 46):

(a) Semantically related: I like to - hate to get up in the morning
the oral - written part of the exam

(b) Phonologically related: bottle (target: bottom) of page five
while the present - pressure indicates

Within a model that has just a single level of representation, semantic errors such as *like* for *hate* and *oral* for *written* can be easily accounted for by the logogen model, Nickels (1997: 16) argues:

> As the lexicon (comprising output logogens) accumulates semantic information from the cognitive system, a range of semantically related items will be activated at one time and random noise within the system (e.g. temporary lowering of a threshold due to recent firing of a logogen) may result in a non target semantically related to the logogen firing.

But it is less of an easy feat trying to explain the occurrence of phonologically related real word errors (e.g. *bottle* for *bottom* and *present* for *pressure*) within Morton's (1970, 1979) model, for logogens are said to accumulate conceptual-*semantic* information, and so processing problems will result in errors that are semantically related only, she points out Nickels (1997). Once the logogen delivers the conceptual-semantic information from the cognitive system to the phonological output lexicon to be expressed errors might well occur in encoding, Nickels (1997) says, but this is what we call a phonemic process as opposed to a phonological one (I'll come back to this point properly in section 3) and so the result will be random phonemic errors that result in non-words (or real-words but just by chance) as opposed to errors which involve the switching out of the target word for something that is phonologically similar (Nickels 1997).

2.2.1.1.1.2 Tip of the tongue states

Models with single levels of representations and therefore 'all or nothing' retrieval of lexical items also have difficulty in accounting for tip of the tongue states in normal speakers. In a tip of the tongue state a speaker 'knows' the word that they want to produce, but can't access its form from memory. They might be able to evidence their knowledge of the word by saying something about it (e.g. that it begins with this or that sound or that it is similar in meaning to this word or that one), but when it comes to accessing its information in its entirety they're - for lack of a better word or phrase here – stumped. Tip of the tongue states can shed further light then on the inner workings of the speech production system in that they capture it in a state of interruption, as opposed to impairment, either because of something we call blocking or something else we call incomplete activation (Herbert 2004) (which is a point I'll come back to in due course).

Brown and McNeill (1966) were among the first to study the kind and quantity of information subjects have available to them in tip of the tongue states. They induced these states in subjects by reading them the definitions of low frequency English words (e.g. 'a navigational instrument used in measuring angular distances, especially the altitude of the sun, moon, and stars at sea' for *sextant*) and asking them to identify what it was that they were describing.

Subjects who didn't recall the target word in this particular example immediately but felt that they knew the word was in there somewhere provided two kinds of information to Brown and McNeill (1966) while they searched for it: semantic and phonological. They were also asked to make Brown and McNeill aware of any other words that came to mind in their attempt to access the target one and these could be divided into two types as well: those that were similar in meaning (e.g. *astrolabe*, *compass*, *divider*, *protractor*) and those that were similar in sound (e.g. *secant*, *sextet* and *sexton*).

Brown and McNeill (1966) pointed out that the nearer subjects were to the successful recall of the target words, the more accurate the information was that they possessed. This insight led Brown (1991) to formulate two hypotheses about the cause of tip of the tongue states (recall): on the one hand, he said that they could be due to blocking, and on the other that they could be due to incomplete activation. The blocking hypothesis suggests that similar sounding words to the target are activated as well as the target and in turn obstruct its retrieval while the incomplete activation hypothesis suggests that tip of the tongue states arise when a target fails to reach the threshold level of activation that's necessary for its retrieval (Brown 1991).

A series of experiments designed to test the two hypotheses owing to Brown (1991) was performed by Jones (1989) and Jones and Langford (1987). The studies used the same methodology as did Brown and McNeill (1966), but modified it slightly to accompany each definition they provided to subjects with a phonologically related, semantically related, both phonologically and semantically related or both phonologically and semantically unrelated prime (e.g. when presented with the definition 'medieval forerunner of chemistry' to induce a tip of the tongue state for the target word *alchemy*, the phonologically related prime *axial* was presented as well). Jones (1989) found tip of the tongue states to be more common when the target and prime were phonologically related or both phonologically and semantically related than semantically related or both phonologically and semantically unrelated, which led them to conclude that the results were more consistent with Brown's (1991) blocking hypothesis than they were with his incomplete activation one.

There were a number of problems with this study, however, in particular in that the prime stimuli differed in difficulty (read: some were high frequency words and some were low ones) and subjects weren't provided with their definitions so if their meanings weren't already known the efficacy of the semantic primes couldn't be properly measured. Due to this, Meyer and Bock (1992) carried out two further experiments following Jones (1989) and Jones and Langford's (1987) methodologies (but made sure to present primes' definitions as well to avoid differences in difficulty contributing to any faciliatory effects found).

Results revealed that words were retrieved more often when their definitions were followed by phonologically, semantically or phonologically and semantically related words than they were when they were followed by phonologically and semantically unrelated ones. They didn't find any evidence to suggest that the provision of semantically or phonologically related information reduced target words' accessibility either (which the blocking hypothesis would of course predict) and so Meyer and Bock (1992) suggested that the partial activation hypothesis looked more likely than the blocking one for this set of data.

When considering the kinds of responses produced by participants in tip of the tongue states the logogen model fails once again then, Nickels (1997) stresses. Just as was the case with speech error data, although the logogen model can easily explain the production of words that are semantically related to the target it can't account for phonologically related responses (Nickels 1997). Furthermore, the availability of partial phonological knowledge in tip of the tongue states is completely incompatible with a threshold model like the logogen one, for in the logogen model (remember) the logogen either reaches an activation threshold at which point the full phonological form is made available, or it remains below the threshold for activation on the phonological form, and no information is retrieved (Nickels 1997). There is no point at which, in the logogen model, partial information is made available, and so those circumstances described in tip of the tongue states cannot be adequately accounted for by it (Nickels 1997).

2.2.1.1.2 Evidence from aphasic speakers

Two cases from the aphasiology literature (JCU (from Howard and Orchard-Lisle 1984) and EST (from Kay and Ellis 1987)) have been cited over and over again to support the assertion that there are two independent semantic and phonological levels of language processing (e.g. Lesser 1989) as well. Whilst JCU made semantic errors but not phonological ones in their speech, EST made phonological errors but not semantic, which researchers have taken to be indicative that there are two processing levels – one semantic and one phonological – that can be impaired independently of each other. I come back to cover these case studies (among a number of others) in more detail in section 3 (sections 3.1 and 3.2, to be exact), but for now, in section 2, I think it should suffice to say that they are thought to have deficits in language processing at two different levels: JCU a deficit at the semantic one (hence the semantic but not phonological errors) and EST a deficit at the phonological one (hence the phonological errors, but not semantic).

Models that incorporate two levels of lexical representation can easily account for speech errors and the availability of information in tip of the tongue states in normals and aphasics however, and so I provide a summary of some of the most widely discussed models made up of two levels of representation here: Butterworth's (1989) semantic lexicon model (in section 2.2.2) and Levelt et al.'s (1999) WEAVER ++ model (in 2.2.3), as well as Stemberger's (1985) and Dell's (1986) interactive activation models in section 2.2.5.1. In 2.3.1 and 2.3.2, respectively, I discuss Shattuck-Hufnagel (1979) and Dell's (1989) interactive activation accounts of phonological encoding, before arguing for a model that looks more along the lines of Jackendoff's (2002) one and turning my attention to anomia and anomia therapy in sections 3 and 4.

2.2.2 Butterworth's (1989) semantic lexicon model

In 1989, Butterworth developed a model with two levels of lexical representation (lexical-semantics and a phonological lexicon) at which lexical access occurs temporally. First, lexical-semantics (which is a transcoding device that takes as input a semantic code from the cognitive system and outputs a phonological address) is accessed. After that, the phonological output lexicon is (which is another transcoding device that takes the phonological address (recall, the output from lexical-semantics) and outputs the phonological form). Access is strictly top-down for Butterworth (1989), (which remember means information flows downward only and can't be fed back from the lower levels up to the higher ones (except if they're the results of checking procedures (hence the two-way arrows in Figure 5.3), but that's not really relevant to this thesis)). You can see this in the following figure:

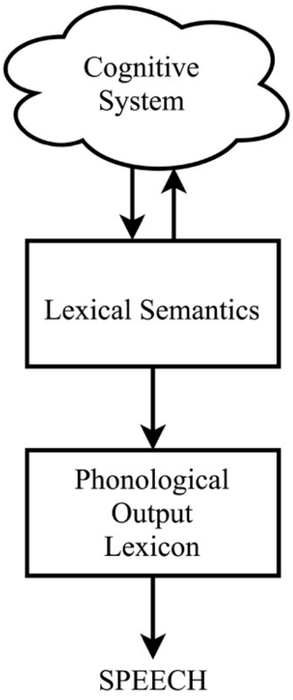

Figure 5.3 The proposed stages of spoken word production: Butterworth's (1989) semantic lexicon model (from Nickels 1997: 21)

Butterworth's (1989) semantic lexicon is made up of pairs of feature sets that associate semantic properties (those that comprise the criterion for the semantic search for a word in lexical-semantics, that is) with phonological ones in the phonological output lexicon (e.g. such as the number of syllables a word has, which of these are stressed and the segments that make up the syllables' onsets, nuclei and codas). Within the phonological output lexicon there exists pairs of addresses (output from the semantic lexicon) and strings of phonemes for the formation of words, and so unlike Morton's (1970, 1979) model of spoken word production, Butterworth's (1989) can easily explain the independence of semantically related and phonologically related spontaneous speech errors, it's pointed out.

Semantically related errors may occur for one of two reasons, Butterworth (1989) argues: (a) because of an error in generating a semantic search criterion, or (b) due to an error in matching a correctly generated search criterion with its partner representation in lexical-semantics (Nickels 1997). Phonological errors, by contrast, occur for just one reason only, when there is a failure to match a correctly retrieved (from lexical-semantics) phonological address with its partner in the phonological output lexicon and a near neighbour, instead of the target, is activated by accident (Nickels 1997).

Once again, unlike Morton's (1970, 1979) model, Butterworth's (1989) can give us a straightforward explanation of how partial phonological information is available to subjects in tip of the tongue states (Nickels 1997). Having accurately accessed a phonological address from the semantic lexicon, speakers might well be unable to retrieve the item that's at that address in the phonological output lexicon (Nickels 1997). If this is the case, the speaker might at that point produce no response, or use the address itself (that is, the one they retrieved from the semantic lexicon) to provide information about the target (that's in the phonological output lexicon) (Nickels 1997).

Addresses bear a relationship to the things that reside there in that similar addresses point to similar sounding words. The speaker could, therefore, retrieve a phonological neighbour (a word that phonologically resembles the target) or they could make use of the information that's in the phonological address to make 'guesses' at the target (relating in phonologically related words and nonwords) (Nickels 1997), e.g. if looking for the target word *anemone* a speaker might well say *enemy* (if the phonological address points to a similar sounding word), *anometer* (an otherwise phonologically similar real word) or *anenome* (a nonword) if they make use of the phonological information initial phoneme [æ], and four syllables but switch out the placement of the phonemes [n] and [m]. They might also even just report the phonological information they're aware of itself instead of trying to do anything with it (e.g. the word I'm looking for begins with 'a' and has four 'beats'); this is something we see all the time, in aphasics especially.

2.2.3 Levelt et al.'s (1999) WEAVER++ model

Now, whilst researchers are in general in agreement that there exist independent semantic and phonological levels of representation in spoken word production, there has been disagreement as to whether there exists or doesn't exist an intermediate level of representation (known as the lemma level) between them. Kempen and Huijbers (1983) were the first to propose the lemma level in models of spoken word production. The lemma, they said, is a semantically and syntactically specified representation that mediates between conceptual-semantic and phonological representations, and so a level depicting its processing belongs between them. Levelt et al. (1999) argue, however, that the lemma mediates between not conceptual-semantic but *lexical*-semantic representations and phonological representations, and that it is at the lemma level that information about an item's syntax is made available. Now, whether the lemma mediates between conceptual-semantic representations and phonological representations or lexical-semantic representations and phonological representations to both Kempen and Huijbers (1983) and Levelt et al. (1999) this much, at least, is clear: there are two stages to lexicalisation (one from the semantic to the syntactic, and one from the syntactic to the phonological).

According to Levelt et al. (1999), there are three major steps that underlie speaking (conceptualisation, formulation and articulation). After perceiving an object-form (for argument's sake, let's say an apple), the concept is identified by the person perceiving it, and a message (which tells the formulation processor what that is) is sent by the conceptualisation processor. The formulation processor would then take the messages and access the appropriate word (i.e. the noun *apple*) in the mental lexicon to express it. It'd then encode syntactic and morpho-phonological structures for the result, and the result would be a phonetic plan (i.e. an articulatory program) for the utterance (Roelofs 2000). Finally, the articulators would execute the articulatory program, and the result of that would be speech (Roelofs 2000), as is exemplified by Roelofs (2000) in Figure 5.4:

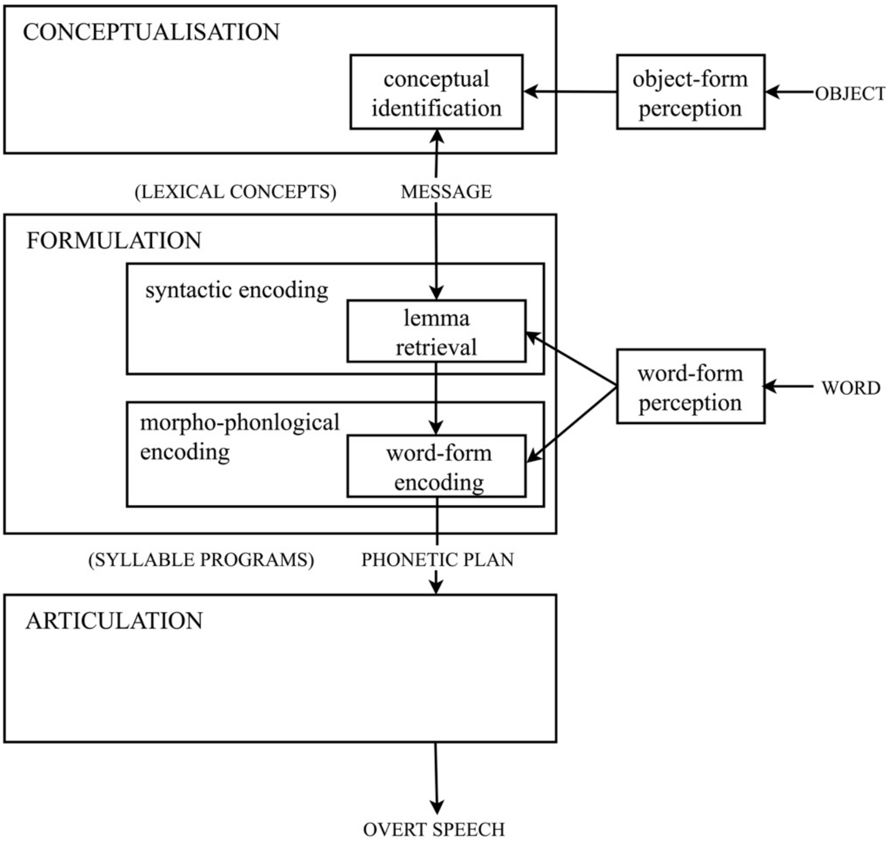

Figure 5.4 The types of process involved in speaking according to WEAVER++ (Roelofs 2000: 73)

Lexical access for Levelt et al. (1999), then, consists of lemma retrieval and word-form encoding processes which are part of the formulation stages of syntactic encoding and morpho-phonological encoding, respectively (Roelofs 2000). In lemma retrieval, lexical concepts are used to retrieve lemmas (which are, again, representations of the syntactic properties of words) from memory, and lemma retrieval makes these properties available for syntactic encoding processes that contain slots for the specification of abstract morpho-syntactic parameters (such as mood, tense, person and number) (Roelofs 2000). In word-form encoding, on the other hand, lemmas and their parameter values are used to recover the appropriate morpho-phonological properties from memory (Roelofs 2000). The purpose of this is to construct phonetic plans (Roelofs 2000).

2.2.4 Evidence that the lemma (syntax) is independent of phonology

A number of studies have sought to prove the independence of syntactic (i.e. lemma level) information from phonological (i.e. lexeme level) information to evidence that word production processes take two steps. If a lemma level exists, syntax will be made available before phonology, people say, and evidence from tip of the tongue studies (described in 2.2.4.1) and neuropsychological studies (2.2.4.2) have suggested that this might indeed be the case.

2.2.4.1 Tip of the tongue studies

Evidence to support the independence of syntax and phonology has been found in tip of the tongue studies involving normal subjects (Caramazza and Miozzo 1997; Vigliocco et al. 1997). In both Caramazza and Miozzo's (1997) and Vigliocco et al.'s (1997) studies subjects were provided with definitions of words which elicited tip of the tongue states and asked questions about the items that they weren't able to name to gauge their knowledge of the words' syntactic (e.g. grammatical gender) and phonological (e.g. the first and last sound and the number of syllables) properties. The studies showed that subjects in a tip of the tongue state were able to report the grammatical gender of words they were unable to name, whether their phonological properties were available or not. The results have of course been interpreted as support for the existence of an intermediate level of language processing between the semantic and the phonological at which syntactic information is available, for syntactic features were known to the speakers even when the full phonological form wasn't.

2.2.4.2 Neuropsychological studies

Further evidence to support the independence of syntax and phonology has been found in neuropsychological studies (e.g. Henaff Gonon et al. 1989; Badecker et al. 1995; Vigliocco et al. 1999). In the earliest (Henaff Gonon et al.'s (1989)) study the case of a French anomic subject, GM was reported. GM presented with fluent speech, but marked word finding problems were evident. While he was able to correctly identify the grammatical gender of the words of the 13/14 test items he was unable to find, though, identification of information relevant to the words' phonological forms was less accurate, which suggested to Hennaff Gonon et al. (1989) that syntactic and phonological information were made available to the patient at two different points in time.

Badecker et al. (1995), meanwhile, report the case of Dante, a patient who presented with anomia as a result of a meningoencephalitis. According to Badecker et al. (1995) when presented with pictures he'd been unable to name in a picture naming experiment, Dante was able to identify the grammatical gender of 106 of the 111 items, but was unable to provide any phonological information about the words at all (not their first sound, not their last sound and not their length). The authors interpreted the results of the study as evidence in support of a two-step level of spoken word production: Dante had access to the lemma level (at which syntactic information was made available), but access to phonology after that was absent altogether.

More recent research (owing to Vigliocco et al. 1999) came to the same conclusion. Vigliocco et al. (1999) described the case of MS who was able to identify whether items were count or mass nouns despite not being able to find the items' phonological forms to produce them during a picture naming task. They too attributed their data to syntactic and phonological information being represented at separate, syntactic and phonological levels.

2.2.5 Activation within two-step models

Not all models that have two levels of language processing have two levels of lexical access, however, and as Nickels (1997) says, it's important we maintain that distinction. Activation can be one of two things: feed-forward or interactive. As its name suggests, in the former activation moves level by level throughout the system, whereas in the latter activation can move forward and backwards. In models with discrete activation processing, there is, therefore, no temporal overlap between stages, but in models with interactive activation processing, there is.

Both one-step models (e.g. Morton's (1970, 1979) logogen model) and some two-step models (e.g. Butterworth 1989, 1992; Levelt 1992; Levelt et al. 1999) assume discrete activation processing, that is that processing has to finish at one level before it can start at the next. Activation in these models feeds forward, one stage at a time, with the output from one level being the input to the level that follows it (Morgart 2015). Dell's (1986) two-step model (and subsequent versions of it (e.g. Dell et al. 1997; Dell 1988, 1989)), on the other hand, differ from Butterworth's (1989, 1992), Levelt's (1992) and Levelt et al.'s (1999) two-step models, for in Dell's (1986, 1988, 1989) and Dell et al.'s (1997) models temporal overlap in processing between adjacent stages is assumed. This, interactive activation, that is, is something that's also assumed by Stemberger (1985) and Shattuck-Hufnagel (1979), as we'll see in sections 2.2.5.1 and 2.3.1, respectively.

2.2.5.1 Stemberger (1985) and Dell's (1986) interactive activation models

Both Stemberger (1985) and Dell (1986) have developed models in which there are two levels of representation that can be likened to those in the models of Butterworth (1989) and Levelt et al. (1999), but, as I've said above, whereas in Butterworth's (1989) and Levelt et al.'s (1999) models activation occurs at each level one after the other, the mechanism by which lexical items are accessed in Stemberger (1985) and Dell's (1986) models involves interactive activation.

Stemberger (1985) and Dell's (1986) models have units at a lexical level (called that in Stemberger's (1985) but the word/morpheme model in Dell's (1986)) which are similar to lemma levels in that they correspond to spoken words and a phoneme level (called that in both Stemberger's and Dell's models) at which phonemes are supposedly stored. Dell's (1986) model has three other levels (a syllable level and a rime level above the phoneme level and a feature level below it), but he omits these from later descriptions and depictions of the model (such as Dell (1989)), and so I won't say too much about them here.

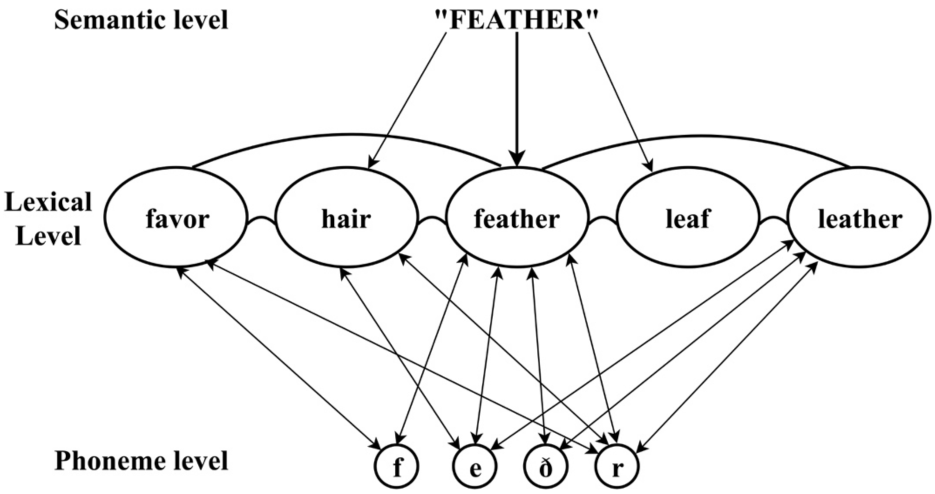

Figure 5.5 Stemberger's (1985) interactive activation model (adapted from Stemberger 1985)

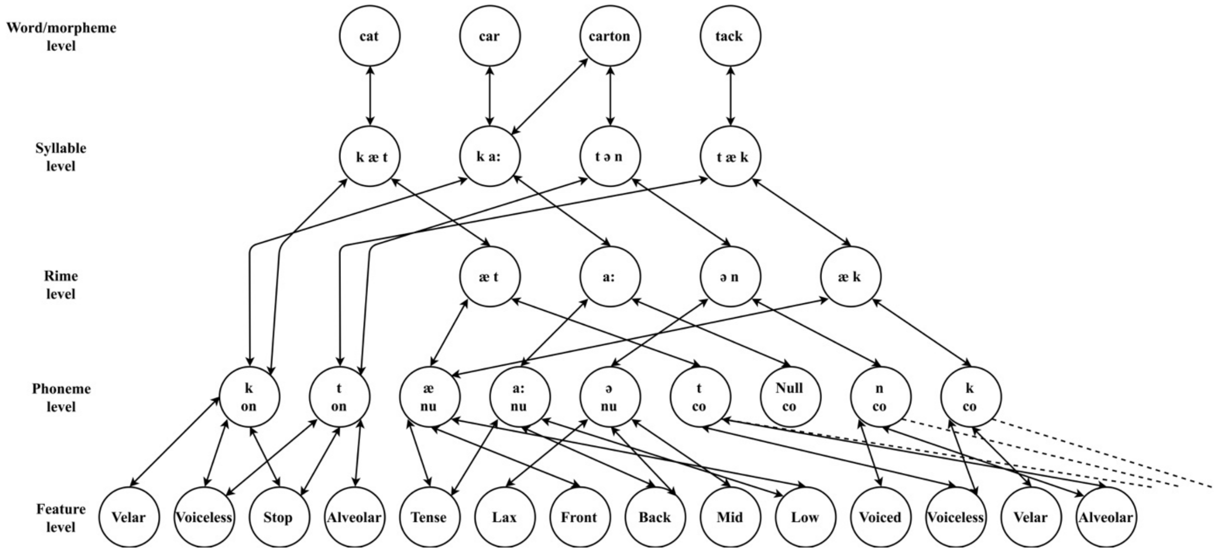

Figure 5.6 Dell's (1986) interactive activation model (adapted from Dell 1986). Word/morpheme level nodes are initially activated from semantic representations that aren't in the diagram. At the phoneme level on = onset, nu = nucleus and co = coda

In both Stemberger (1985) and Dell's (1986) models activated units (or nodes) send some of their activation to every other unit/node they are connected to. More highly activated nodes have larger effects on other nodes, of course, whereas less highly activated nodes have smaller effects. Differences in activation at one level are reflected at the other levels, and activation from lower levels feeds back to higher levels of the system (e.g. activated phoneme nodes will spread activation to words that are made up of those particular phonemes, meaning that non-target words can become partially activated if they are phonemically similar to (that is, are made up of similar phonemes to) the target).

These interactive two-step models can, therefore, account for the semantic and phonological errors produced in tip of the tongue states in a similar way to Butterworth's (1989) non-interactive two-step model, then. Conceptual-semantic information activates units at the lexical level (in Stemberger (1985)) and the word/morpheme level (in Dell (1986)) that represent that information, and errors arise because of the systematic spread of activation to non-target units and them being mistaken for targets as a result. This can also be the case at the phoneme level; consider how easy it would be for *feather* to be mistaken for *leather* in Figure 5.5, which phonologically differs from the target in its first phoneme only.

Now, although Stemberger (1985) and Dell's (1986) models have phoneme levels and Dell's (1986) has feature, rime and syllable levels), what they don't do is specify how phonological encoding is accomplished. Separate models of phonological encoding have been proposed, however (by Shattuck-Hufnagel (1979) and Dell (1989)), and so I'll consider them next.

2.3 Phonological encoding

In the previous sections, I've discussed some of the mechanisms in models that can lead to the retrieval of a word's phonological form. After that, though, 'the phonological form has to be translated into an articulatory programme for controlling the speech musculature' (Nickels 1997: 45). This process is what we call phonological encoding.

There are a number of models of phonological encoding out there, and almost all of them incorporate some sort of 'slot-and-filler' mechanism by which the sounds of words (the fillers) have to be inserted into their position in the word frame (that is, their slot). It is this process that is the source of many phonological speech errors, Nickels (1997) points out, and so this section begins by classifying the speech errors we see that are phonologically related to their targets. It moves on, then, to a detailed description of the factors that have even found to constrain their occurrence (an account that closely follows that of Shattuck-Hufnagel (1979), which is the paper that motivated the making of her model). I go on to describe Shattuck-Hufnagel's (1979) model of phonological encoding after that and discuss, as she does, how the various error types might arise in it. Next, I make a move to describe the ways in which Dell's (1986, 1989) interactive activation model is similar to Shattuck-Hufnagel's (1979) slot-and-filler model, and how, like Shattuck-Hufnagel's (1979), it can account for phonological encoding errors.

2.3.1 Shattuck-Hufnagel's (1979) interactive activation account of phonological encoding

2.3.1.1 How speech errors are classified

In her 1979 publication *Speech Errors as Evidence for a Serial-order Mechanism in Sentence Production* Shattuck-Hufnagel made use of the MITCU (Massachusetts Institute of Technology Cornell University) corpus of speech errors to develop a scan-copier model of phonological encoding. Out of the 6000 speech errors collected over six years in the corpus Shattuck-Hufnagel (1979) found five types (substitution, exchange, shift, addition and omission). She exemplifies these on page 299 as follows (Shattuck-Hufnagel 1979: 299):

1. Substitution: A target segment is replaced by an intrusion segment, which may or may not have an apparent source within the utterance, e.g.
   (a) It's a shallower test - chest, but broad
   (b) Anymay, I think (anyway)

2. Exchange: Two target segments change places in the target sequence, each serving as the other's intrusion segment, e.g.
    (a) emeny (enemy)
    (b) It's past fassing - fast passing by

3. Shift: A target segment disappears from its appropriate location and appears at another location in the target sequence, e.g.
    (a) State-lowned-_and owned-land (state-owned-land)
    (b) in a b_ack blo - black box

4. Addition: An extra segment is added to the target sequence; this intrusion may or may not have an apparent source within the utterance, e.g.
    (a) either the plublicity would be bad (publicity)
    (b) they bring abrout - about a

5. Omission: A target segment is dropped from the target sequence' there may or may not be a similar sequence elsewhere in the utterance, e.g.
    (a) the d_ug -the drugs
    (b) piano sonata _umber ten (number)

Shattuck-Hufnagel (1979) also went on to categorise these five types of speech error along the dimension of something she called the direction of influence (i.e. wherein the sentence the source of error was). When the source was later in the sentence, Shattuck-Hufnagel (1979) classified the error as anticipatory (e.g. the addition in 4(a)), and when the source was earlier she classified the error as being perseveratory (e.g. the addition in 4(b)). Like the additions in 4(a) and (b) omissions and substitutions can be either anticipatory or perseveratory, Shattuck-Hufnagel (1979) said, though it isn't always the case that additions, omissions or substitutions have to have an identifiable source, she stresses (and then exemplifies in 1(a) and (b) and 5 (a) and (b)). Shifts can likewise be anticipatory or perseveratory, she says, but unlike additions, omissions and substitutions, these errors do indeed need to have an identifiable source. Exchanges, lastly, were classified as being neither anticipatory nor perseveratory. This is of course because in exchanges there is more than one source and so the direction of influence goes both ways. Take the *emeny* for *enemy* example from (2a), for instance. In this, both the /n/ and the /m/ have a part to play and can be described as the source, in that they switch places.

2.3.1.2 How speech errors arise

In light of these errors, Shattuck-Hufnagel (1979) developed a model of three parts which has been described, super succinctly, by Nickels (1997: 53):

1. a dual representation consisting of serially ordered slots and an equal number of independently represented target segments, at least at the word level and at the sound level; [and]
2. a scan-copier that selects the appropriate segment from the set of two monitors: a check-off monitor, which marks or deletes segments as they are copied into their target slots, and an error monitor, which detects and deletes or otherwise edits error-like sequences in a planned utterance.

At the word level, ordered frameworks of word slots are generated and corresponding sets of target items retrieved from the lexicon to be stored (short-term) in a buffer. The scan-copier scans the target sets in the buffer for the ones that belong to the first of the ordered slots, and once those morphemes are found, they're copied into their positions. At this point, there is a check-off monitor that either marks the used morphemes as used (or deletes them from their sets if they're no longer needed) and the scan-copier moves onto the next slot, to repeat the process until all slots have been filled.

At the phoneme level, the same process is found but this time with phonemes as opposed to morphemes ones and ordered frameworks of, of course, phoneme-sized slots rather than ones that are morpheme-sized. Words are entered into a short-term storage buffer as they are retrieved, once again, with basic sets (such as initial, medial and final phonemes) lined up to be slot into their positions in the syllables of the target lexical items that are derived from rules of syllable structure and stress patterns. The scan-copier scans the buffer for what it needs each time (e.g. it scans the initial/final consonantal sets for the onsets/codas and medial sets for vowel phonemes for nuclei (or indeed for liquid or glide phonemes which can also act as nuclei)).

According to Shattuck-Hufnagel (1979), each of the five errors described above can be accounted for as being due to some sort of problem with this segment selection process. These too have been helpfully summarised by Nickels (1997: 54) as follows:

1. Exchanges arise from misselection by the scan-copier (but intact check-off, as described earlier).

2. Anticipatory substitutions arise from miss election and then failed check-off, perseveratory substitutions from failed check-off and then misselection. “No source” substitutions are explained as incompletely or incorrectly transferred to the slot location, or an extra word found its way erroneously into the set and was miss elected (e.g. as in ‘Freudian” slips.
3. Additions involve miss election and failed check-off, where the miss election os of a segment for a slot that should have been empty.
4. Omissions are argued to arise from miss election of a null element followed by failed check-off.
5. Shifts are explained by two full misselections, one of a target for a null slot and the other of a null segment for a target slot.

In 1987, however, Shattuck-Hufnagel reexamined the evidence for the existence of processing units between word and phoneme levels and, in light of speech error data involving the movement or replacement of onsets, suggested that syllabic structure plays a more significant role in phonological encoding than she had initially thought. She went on to instead propose a two-stage model of phonological encoding that allowed for the serial encoding of syllabic structure. For a more thorough explanation than space permits me to give here, one should really refer to the original text.

2.3.2 Dell’s (1989) interactive activation account of phonological encoding

Dell’s (1989) interactive activation model is like Shattuck-Hufnagel’s (1987) slot-and-filler model in a couple of ways, both in that it can account for phonological encoding and in that Dell (1989), like Shattuck-Hufnagel (1987), revised his earlier (1986) model to allow syllables to encoded serially (previously he had assumed simultaneous encoding of onset, nucleus and coda within syllables).

According to Dell (1989), there exists in the mind two networks (a lexical network and a word-shape network) as in Figure 5.7. This, of course, captures the conception that the phonological structure of a word has two components: a sequence of slots that specifies the shape of the word and the kinds and quantities of syllables and phonemes it contains, and a representation of the sounds that fill them:

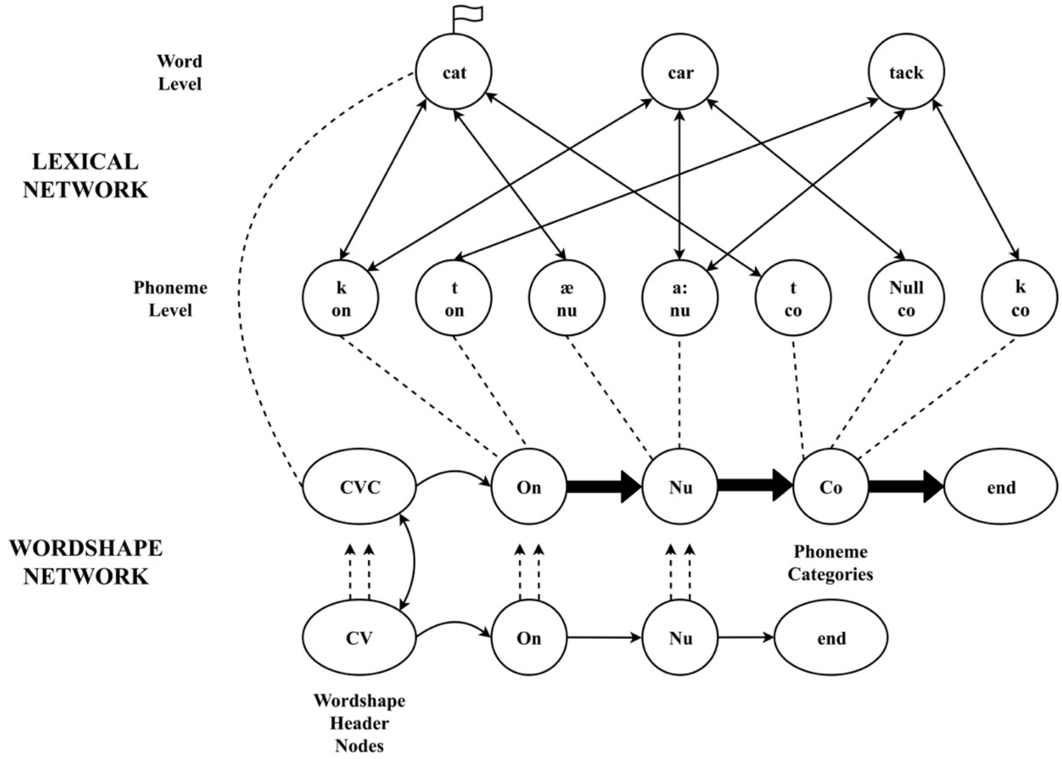

Figure 5.7 Dell’s lexical and word shape networks (adapted from Dell 1989). The intended word here is ‘cat’ as indicated by the flag on the listing of ‘cat’ at the word level in the lexical network. The dotted lines in the diagram represent connections between the lexical network and the word shape network, and the arrows between phoneme categories in the word shape network their sequence of activation. As in Dell’s (1986) model, On = onset, Nu = nucleus and Co = coda

To Dell’s (1989) mind, the lexical network is where words in the word level are connected with the phonemes that make them up in the phoneme level. At the phoneme level, phonemes are specified for their position in the syllable (i.e. whether they’re syllable initial (lie in the onset), syllable medial (lie in the nucleus) or syllable final (lie in the coda)).

Word nodes in the lexical network are also connected to word shape header nodes that represent the pattern of phoneme categories for them, he says (e.g. the word node for ‘cat’ in Figure 5.7 is connected to the word shape header node ‘CVC’ which tells the speaker the word to be produced is a single closed syllable). The word shape header node then connects to a series of phoneme categories (e.g. the word shape header node ‘CVC’ for the word node ‘cat’ connects to three phoneme categories On (onset), Nu (nucleus) and Co (Coda) where /k/, /æ/ and /t/ will go, respectively).

The word that’s currently being phonologically encoded in Dell’s (1989) model is flagged (quite literally so with a flag in his drawing of the model but with an arbitrary amount of activation (100 units) in the mind) as being the ‘current’ word. Upcoming words in the phrase are primed for retrieval with a lesser amount of activation (50 units), and during each time step, nodes send some fraction of their activation to nodes they are connected to above and below them.

After a certain number (that’s determined by the speech rate) of time steps has passed the most highly activated phonemes (in the category nodes of the word shape network) are selected for speech production. What order they’re articulated in depends on the word shape network’s word shape header nodes: these are sent activation from word nodes in the lexical network, and the header that’s the most highly activated gets to decide what phonemes appear where.

Phonological errors occur, Dell (1989) says, if an incorrect phoneme is more active than the correct one of the same category (as was the case in his 1986 model). Say the target, like in Figure 5.7, was ‘cat’ for example. If the phoneme /t/ in onset position (which was meant for ‘tack’, not ‘cat’) was more activated than was /k/ in onset position, it might well be mistakenly selected for speech production resulting in the real word error ‘tat’. Likewise, if the null coda (which was meant for ‘car’) or /k/ coda (which was meant for ‘tack’) were more activated than the /t/ coda, a speaker might well produce ‘ca’ or ‘cack’ instead of ‘cat’.

2.4 Concluding remarks

In this (rather large) section, some background research to current methods in the remediation of spoken word production after brain damage has been outlined. Some of the key assumptions of cognitive neuropsychology have been discussed, as have some of the sources of data that have been called on to develop spoken word production theories (such as speech error data, tip of the tongue data and data from people with aphasia). The degree to which each can adequately account for speech error data has been analysed to some degree, but whilst word finding difficulties can indeed be explained by these models, the phonological mechanisms in the models aren’t actually substance-free (and, as I made clear in Chapter 4, this is the theory of phonology that I myself subscribe to).

I am, of course, supportive of the idea that there are multiple components of language processing – a semantic one, and a phonological one – the aphasiology data evidences that much. I also see no reason why semantics and phonology couldn’t be active at the same time. However, whilst it wasn’t (and still isn’t) my intention to propose a new model of word retrieval in this thesis, what I will say is this:

If phonology is substance-free (and it certainly seems to be so), then the phonological component of word retrieval should be devoid of phonetic content. Rather, phonology and phonetics should be instantiated separately, with an interface (or interfaces) amid them that bridge(s) the gap between the two. Importantly, deficits in the interface(s) (or beyond it/them) shouldn't be described as phonological ones of word retrieval (even if the interface(s) is/are connected to phonology).

Another shortcoming of what's so far been sketched out in the clinical literature is that the models don't make it clear what the role of the *lexicon* is, where lexical entries are *stored*, where syntax is situated and how connections between the semantic, syntactic and phonological modules are made and communication handled. Thankfully, in *Foundations of Language: Brain, Meaning, Grammar, Evolution*, Jackendoff (2002) offers a solution to each of these problems.

2.4.1 An alternative account owing to Jackendoff (2002)

2.4.1.1 The role of syntax

Traditional generative grammar only sees syntax as being generative. Neither semantics nor phonology are, in the eyes of generativists. There is 'a fundamental assumption embedded deep in the core of generative theory[…] that the free combinatoriality of language is due to a single source, localized in syntactic structure' (Jackendoff 2002: 107). Jackendoff (2002) takes issue with this, and develops 'the alternative assumption that language has multiple sources of combinatoriality, each of which creates its own characteristic type of structure'. The outcome is a theory of grammar which has a has a tripartite organisation and in which phonology, syntax and semantics are equally as generative as each other. In Jackendoff's (2002) model, syntax is all but one of the three parallel generative components. It is among the combinatorial systems, but it is far from the only one (Jackendoff 2002: 126):

> '[The figure reveals] the role of syntax in the parallel architecture. […] Syntactic structure serves as a "way-station" between [the semantic and phonological] structures, making the mapping between them more articulate and precise. Thus, although syntax is in the center [...], the grammar is no longer syntactocentric[.] Rather, syntax is simply one of the three major generative components in the grammar'

This model, as well as being different from models we see in traditional generative grammar, is different from the models of word retrieval we viewed earlier (e.g. the logogen model

(Morton 1970, 1979), the semantic lexicon model (Butterworth 1989), the WEAVER++ model (Levelt et al. 1999), the interactive activation models (Stemberger 1985 and Dell 1986), and models which gave interactive activation accounts of phonological encoding (Shattuck-Hufnagel 1979 and Dell 1989).

The first difference between aphasiological models of word retrieval and Jackendoff's (2002) is that under Jackendoff's (2002) view, there aren't just two components of word retrieval (semantics and phonology) there are three: one having to do with phonology, another having to do with syntax (which was absent, as its own module at least, from all of the models discussed thus far) and one still more having to do with phonology.

2.4.1.2 The integrative and interface modules

Jackendoff (2002) argues for phonological, syntactic and conceptual processors (which are similar to Fodor's (1983, 2000) modules, but it's necessary to point out here that his use of the word *module* is a little different than Fodor's (1983, 2000), as is some other terminology he uses. What Fodor calls *modules* Jackendoff calls *processors,* and for Jackendoff, there are three types:

- inferential processors (which are the exact same thing as Fodor's central systems),
- integrative processors (Fodor's modules) and
- interface processors (which make the integrative processors able to communicate)

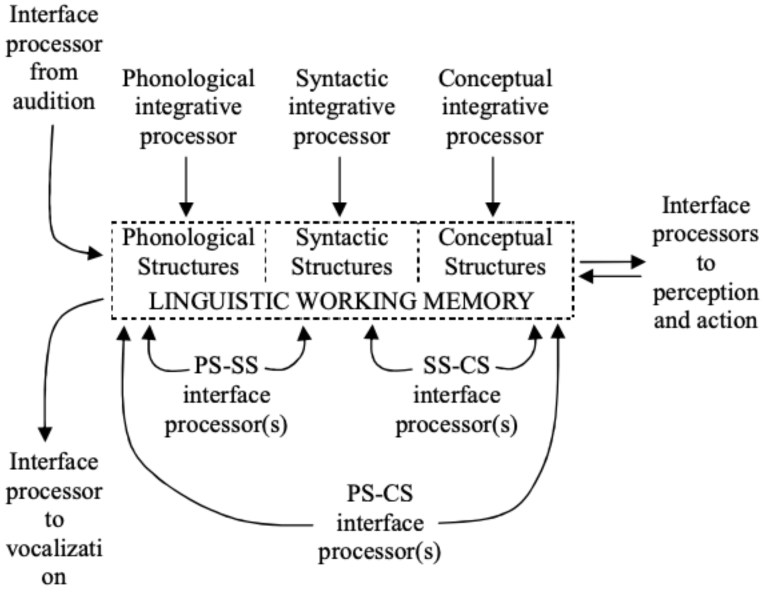

Figure 5.8 Jackendoff (2002)'s model of the language faculty (Jackendoff 2002: 199)

According to Jackendoff's (2002) model of the language faculty, there are three integrative processors independent of one another: the phonological integrative processer (which processes phonological structures), the syntactic integrative processor (which processes syntactic ones), and the conceptual integrative processor (which of course, processes structures that are conceptual). The conceptual integrative processor is sometimes called the semantic integrative processor, and I myself use the terms *conceptual* and *semantic* interchangeably throughout.

They communicate with one another via interfaces: the phonological and syntactic integrative processors are interfaced by a phonological structures-semantics structures (PS-SS) processor, the syntactic and conceptual integrative processors being interfaced by a syntactic structures-conceptual structures (SS-CS) processor and the phonological and conceptual integrative processors take the phonological structures-conceptual structures (PS-CS) as an interface.

Under his view, phonology, syntax and semantics are all informationally encapsulated modules, which communicate with one another via interfaces that take their outputs and turn them into inputs the other modules are able to interpret – the PS-SS processor turns phonological output into syntactic input and vice versa, the SS-CS processor syntactic output into semantic input and vice versa, and the PS-CS processor phonological output into semantic input, and semantic output into phonological input.

There are also a couple of phonetics-phonology interfaces specialised for perception and production that a) tally well with a substance-free view of phonology and b) importantly account for anomias that are phonological and post-phonological in nature. It's because of these interfaces, first and foremost, that I subscribe to a model of the mind like Jackendoff's (2002), and so I'll describe them here.

2.4.1.2.1 The interfaces

Some have said that the phonetics-phonology interface is mediated by a transduction process that converts substance-free phonological forms into substance-laden phonetic ones. This approach to the interface (that is, that it is a transducer) is indeed the one that's argued for by Fodor (1983) (recall from Chapter 2, section 2.2), but it can be traced back even further, to Chomsky and Halle (1968) and Jakobson et al. (1952). In more recent years, Hale et al. (2007) and Hale and Reiss (2008) have also assumed this position.

What's interesting about theories like these (well, to this thesis at least) is that in differentiating between phonology and phonetics (and separating them with a substance infusing transducer), they can more adequately account for the difference between phonological and post-phonological deficits than current aphasiological models can (see 3.2). Phonological deficits occur due to damage of the phonological module, and post-phonological deficits due to damage of the phonetics-phonology interface(s), I imagine.

In the *Foundations of language: Brain, memory, grammar, evolution,* Jackendoff (2002) offers an alternative view of the interface (or interfaces, I should say), which is the one that I myself am inclined to concur with. Jackendoff (2002) doesn't just propose that there is one interface between phonology and phonetics, you see, he proposes that there are two: one from audition, which translates phonetic data having to do with hearing into phonological data, and one to vocalisation, which translates phonological data, into data having to do with speaking.

These interfaces are therefore different from the others (the PS-SS, SS-CS and PS-CS processors), as whilst the interface processor from audition is designed to take only phonetic information as input and the interface processor from vocalisation is designed to take only phonological information as input (meaning that they're specialised for one domain each), the modules between phonology and syntax and syntax and phonology are specialised for two; the interfaces between phonology and syntax, syntax and semantics and phonology and semantics in Jackendoff's (2002) model are required to have access to both the vocabulary of both the sending and the receiving module (the phonology/syntax for both phonology and syntax, the syntax/semantics for syntax and semantics, and so on) (Scheer 2014). Jackendoff (2002) calls the phonetics-phonology interfaces domain specific, and the PS-SS, SS-CS and PS-CS interfaces *bi*-domain specific.

2.4.1.2.1.1 A side note on the phonetics-phonology interface

Whilst what's going on in the interfaces looks a little like transduction as Jakobson et al. (1952), Chomsky and Halle (1968), Fodor (1983, 2000), Hale et al. (2007) and Hale and Reiss (2008) etc., say in that they each serve to transform the output of one module (that's only interpretable by the module to which it belongs) into an input interpretable by another module, as you delve deeper into Jackendoff's (2002) discourse it's apparent that there is an important difference.

As Iosad (2013, 2017) so rightly points out about the phonetics-phonology interface, Jackendoff's (2002) processors are a lot richer compared to conceptions of the interface owing to Fodor (1983) in which the translation of information interpretable by one system to information interpretable by another is undertaken by transduction (e.g. Pylyshyn 1984). Transducers, for one, are innate and invariant (Hale et al. 2007: 647) and, in being so, they, of course, cannot adequately account for language variation in the same way that Jackendoff's non-innate (and therefore not invariant) interface phonetics-phonology processors can:

> these two transducers [perception → phonology and phonology → articulation] are innate and invariant – they are identical in all humans (barring some specific neurological impairment) and do not change over time or experience (i.e., they do not "learn")

In the 'poor', innate interface model (that's akin to the conception of Fodor (1983)) the mapping between phonological units and phonetic ones (having to do with production and perception) is consistent cross-linguistically for the most part.[6] This, of course, inadequately accounts for the variation we see between languages. In Jackendoff's (2002) 'rich', interface model, on the other hand, Iosad (2013, 2017) helpfully highlights, knowledge is not innate but may be learned, and so there is no expectation of universality.

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[6] Although Hale et al. (2007) do have a place for language specific mapping mechanisms (Iosad 2013, 2017).

Like Jackendoff (2002) (and indeed Iosad (2013, 2017), I hold the belief that the phonetics-phonology interface, or interfaces, are best viewed not as deterministic transducers, but as something akin to a module (or modules) that translate phonetic information into phonological information, and/or phonological into phonetic. As Iosad (2013, 2017) says, this view of the interface is necessary for the variability of phonetic phenomena. Models of the mind which paint a picture of interfaces as being nothing more than transducers cannot adequately account for the wide range of variability we see between surface representations, and so they surely can't be correct.

Jackendoff's (2002) parallel architecture model solves two problems that the models of word retrieval in the clinical literature cannot, then: a) it describes the part syntax plays in the grammar, b) it describes how syntax is instantiated (as an integrative module like the phonological and semantic integrative modules), and c) how those integrative modules, if informationally encapsulated, communicate with one another (via interface modules). It also, as I said, tallies well with the substance-free phonology thesis, in that it explains where phonetics, if not grounded in phonology, is instantiated and how phonology and phonetics communicate (via yet two more interfaces, one for perception and one for production).

Jackendoff (2002) doesn't stop there, though. He also, in the parallel architecture model, makes a convincing case for the role of the lexicon, and how that, together with everything else, plays its part in language processing.

2.4.1.3 The lexicon and lexical entries

Jackendoff's (2002) parallel architecture of the language faculty once again challenges some of the assumptions made in traditional generative grammar. The first assumption it challenges is that lexical items are stored in long-term memory and enter the grammar by being inserted into syntactic structures. The second is that those lexical items are always words. In his parallel architecture, lexical items emerge as *parts of the modules that interface integrative modules*. Lexical items aren't always just words either; they are of heterogeneous sizes. Some are smaller than words, e.g., affixes, and some are larger than words, e.g. idioms (Jackendoff 2002).

For most generativists, the lexicon is the store of words in long-term memory, which are used, by the grammar, to construct phrases and sentences (Jackendoff 2002). 'It is widely agreed that a word is to be regarded as a long-term memory association of phonological, syntactic, and semantic features', Jackendoff (2002: 130) says, including by himself.

However, 'whilst mainstream generative grammar, following Chomsky (1965), inserts lexical items as a whole into syntactic structure; their phonological and semantic features [aren't] interpreted [until] later in the derivation by the appropriate components' (Jackendoff 2002: 130). But whilst this approach isn't impossible, Jackendoff (2002) points out, it *does* raise the question of why syntax should carry around all of those phonological and semantic features that it can't itself see. Other researchers over the years, having had a problem with this too, have suggested as a solution late lexical insertion, but whilst these approaches are an improvement in that syntax doesn't have to carry around the phonological and semantic features for as long, it still has to carry them, which doesn't make much sense, he stresses (Jackendoff 2002).

Something quite different is suggested in his sketch of a parallel architecture. 'A word, by virtue of it having features in each of the components of grammar, serves as part of the linkage between the multiple structures', he says (Jackendoff 2002: 130). The proper way to regard [a word] is as a small-scale three-way interface rule[which] lists a small chunk of phonology, a small chunk of syntax, and a small chunk of semantics' (Jackendoff 2002: 130).

ten Hacken (2019) provides a most helpful example from Dutch. The lexical entry for the word *cheese* (which, in Dutch is *kas*), will look a little something like this, he says:

(1) a. /kas/
b. noun, non-neuter
c. [$_{\text{Thing}}$ CHEESE]

This explains why people with semantic anomia sometimes mistakenly say *ham* when they mean to say *cheese*, because although syntactically they are the same, they are different semantically and phonologically. An intact phonological processor likely wouldn't pose a problem. But an impaired semantic one might, especially given that *cheese* and *ham* are often associated with one another, in that they, apparently, make a perfect food pairing. The lexical entry for *ham* in Dutch (*ham*) would look something along the lines of this:

(2) a. /hɑm/
b. noun, non-neuter
c. $[_{\text{Thing}}$ CHEESE]

If the function of lexical items, Jackendoff (2002) argues, is to serve as interface rules, then, the lexicon as a whole has to be regarded as part of the interface components. '[T]he formal role of lexical items is not that they are "inserted" into syntactic derivations, [he says,] but rather that they establish the correspondence of certain syntactic constituents with phonological and [semantic] structures' (Jackendoff 2002: 31).

Now, in addition to the usual lexical items that list all three structures (phonology, syntax and semantics), there exist lexical items that have phonology and semantics but no syntax, phonology and syntax but no semantics, syntax and semantics with no phonology and stored pieces of phonology that lack both syntax and semantics.

Examples of lexical items that have phonology and semantics but no syntax can appear alone as utterances, but cannot be combined into sentences with other words. These include (Jackendoff 2002: 131-132):

a. yes, no
b. hello, goodbye, thanks
c. ouch, oops, wow, phooey, hooray, gadzooks, oboy, oy vey, dammit, shit, yuck, upsey-daisy
d. hey, fiddlesticks, pshaw, humph, oo-la-la
e. shh, psst, tsk-tsk
f. abracadabra, hocus-pocus
g. bow-wow, cockadoodledoo

Words that have phonology and syntax but no semantics include the *it* in *it's hot in here* which is present to carry tense and tense only, Jackendoff (2002: 132) points out, and an example of a lexical item that has syntax and semantics but no phonology is mainstream generative theory's PRO which serves as the subject of infinitives (*Bill tried [PRO to talk]*).

Stored pieces of phonology that lack both syntax and semantics, meanwhile, include the nonsense refrains that are used to take up metrical structure in songs: fiddle-de-dee, heydiddle-diddle, hickory-dickory-dock, eenie-meenieminie-moe, she-bop-she-bop, rikiti-tikiti-tin [and] ink-a-dink-a-doo', he says (Jackendoff 2002: 132).

So, to recap, lexical entries for Jackendoff (2002) are stored in long-term memory, and combined to form linguistic structures in short-term memory during language production. Jackendoff's (2002) model of competence is even more relevant to word retrieval than that, though. He uses it to sketch a model of performance.

2.4.1.4 Lexical processing

According to Jackendoff, there should be a clear connection between theories of competence (linguistic structure) and theories of performance (language processing). As such, he makes sure to show how his parallel architecture, and in particular, his treatment of the lexicon in the model, fits nicely into analyses of lexical access in production.

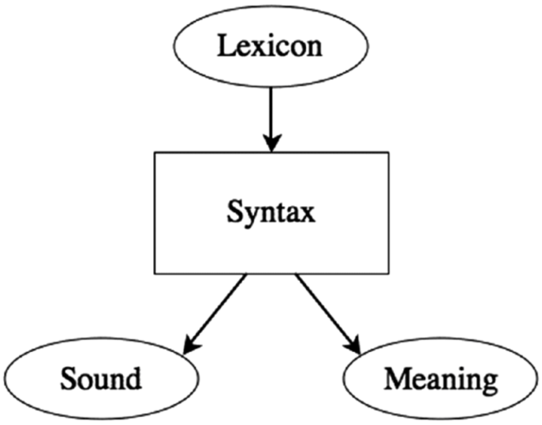

Figure 5.9 Chomsky's model of language (ten Hacken 2019: 207)

The standard architecture for generative grammar from *Aspects* looks a lot like what's sketched out in the model of language above. Generativists, recall, see grammar as being both syntactocentric and derivational as so, in Figure 5.9 above, with the generative capacity of language coming from syntax and syntax only (Jackendoff 2002). Linguistic structure is built by an initial stage of syntactic derivation in traditional generative grammar, Jackendoff (2002) explains. These derivational rules produce levels of syntactic structure that are subjected to semantic and phonological interpretation. Semantic and phonological structures, therefore, do not have any generative capacities of their own (Jackendoff 2002).

The logical directionality of this syntactocentric architecture, however, is at odds with the logical directionality of language processing, it's pointed out by Jackendoff (2002), for in perception, one has to get from sounds to meanings (phonology to semantics) and in production, from meanings to sounds (semantics to phonology). Jackendoff's (2002) parallel, constraint-based architecture, on the other hand, is not. In fact, it bears a striking resemblance to theories of processing, particularly Levelt et al.'s (1999).

'A crucial role in this processor is played by linguistic working-memory', Jackendoff (2002: 200) emphasises, 'which is to be understood not just as a static storage space for linguistic material, but as a dynamic "workbench" or "blackboard" on which processors can cooperate in assembling linguistic structures. It has three divisions or "departments" or "buffers," corresponding to the three levels of linguistic structure' (Jackendoff 2002: 200).

In language production, the processor starts with figuring out what concept it wants to retrieve the word for in the semantic integrative module. The module then sends a call to the lexicon (in long-term memory), asking what saved structures (from as small as affixes, remember) could potentially express parts of that, and various candidates are activated. The lexical items activated then compete for integration into semantic working-memory. At some point, one wins – this is called lexical selection in the clinical literature.

Once the lexical item's semantic structure is bound from long-term to working-memory, its syntactic and phonological structures follow suit and bind to their blackboards. Mind, this is all being done incrementally, Jackendoff (2002) emphasises – although this description of the model's processing *sounds* sequential, it is not necessary for one level of structure to be fully formed in order for its interfaces to start passing information up and down the line Jackendoff (2002).

Once the other (syntactic and phonological) structures are all activated in their respective domains, the semantic, syntactic, and phonological structures combine together to create the intended message. Phonology, of course, speaks via an interface to phonetics, and the word is pronounced.

This clearly tallies well with Levelt et al.’s (1999) model WEAVER++ model (recall from section 2.2.3) which makes mention of lemmas and word forms (and of course, all others that do). Levelt et al.’s (1999) lemmas and word forms have clear correspondences in Jackendoff’s (2002) model, even though there aren’t ‘lemma retrieval’ or ‘word-form encoding’ areas argued for by Jackendoff (2002) as they are Levelt et al. (1999):

> At the moment in time when a lexical item is initially activated, only its semantics is of interest. The conceptual integrative processor, which binds the lexical item to part of the thought being expressed sees nothing but meaning. The next step is to use the semantics-syntax interface to put some material on the syntax blackboard. For this process, it is crucial to activate and bind the item’s syntax. Then the syntactic integrative processor can begin to work this item into the syntax of the utterance being built. None of the processors invoked so far can see the item’s phonology-so for all intents and purposes they are working with the item’s lemma. And whether or not the item’s phonology is activated in long-term memory at this point is irrelevant to these processors. Similarly, the later step of phonological integration is carried out by the syntax-phonology interface processor and the phonology interface processor. These can see only the relation of the item’s syntax to its phonology, and are more oblivious to its meaning. Hence they are in effect working with the item’s word form.

Now, given the constraints on time and space, I will leave the discussion of Jackendoff’s (2002) model here for now, so that my musings don’t venture *too* far beyond the scope of this thesis, which was originally set to answering the question of whether or not phonology can or cannot be considered modular according to the definitions of modest (Fodor 1983, 2000) and massive (Carruthers 2006a) modularists. However, whilst it was not my intention in this thesis to pitch one model of the mind against another, or necessarily make an argument for Jackendoff’s (2002) model or even one of my own, it *is* worth thinking about this at least some shallow depth, I believe, as the conclusions I come to at the end of this thesis interestingly provide some support for, and against, the model of the mind Jackendoff proposed in 2002. In Chapter 4, I presented evidence for phonology’s informational encapsulation, and in the rest of Chapter 5 (coming up) I present evidence against phonology’s domain specificity, both of which are required, I argue, by Jackendoff (2002).

In what follows, I return to the exploration of whether or not the phonological module can be considered domain specific, through experimental treatment of phonological anomia with semantic and phonological therapies. I begin with a description of anomia in this next section, section 3.

3. Anomia

We obviously know now that aphasia is an acquired disorder of language that presents as a result of damage to the brain areas responsible for the production and/or comprehension of language and its components and that, depending on the site or sites of damage and the relative impairment or sparing of the language components, different subtypes of aphasia can emerge (Helm-Estabrooks et al. 2014). As I mentioned in section 2, there are two major subtypes of aphasia: Broca's aphasia and Wernicke's aphasia. These are caused by damage to the lateral frontal, suprasylvian, pre-rolandic area or the adjacent subcortical periventricular white matter, and the posterior third of the superior temporal gyrus, respectively (Helm-Estabrooks et al. 2014).

Broca's aphasia is otherwise known as expressive aphasia and is so-called because it is characterised by impaired language production with unimpaired language comprehension (Kertesz 1982). Wernicke's aphasia, on the other hand, (which goes by the name of receptive aphasia as well), causes problems of a similar vein but in the opposite direction. While a Broca's patient experiences problems with language production but not its comprehension, a Wernicke's patient has them with comprehension but not production (Albert et al. 1981). Other (more minor) subtypes of aphasia include conduction aphasia, transcortical motor aphasia, transcortical sensory aphasia and global aphasia (Helm-Estabrooks et al. 2014), and with all of the different subtypes, different symptoms are associated. Some level of anomia meanwhile seems to be seen in all of the aphasias (Manasco 2014).

Anomia (as I've briefly touched on before but will describe in more detail here) is an impaired ability at accessing words in and retrieving them from the mental lexicon (Goodglass and Wingfield 1997). This can range from a mild difficulty in producing desired words during conversational discourse to a virtual inability to produce them under any conditions at all (Helm-Estabrooks et al. 2014). Being that anomia is a symptom of aphasia, it is thought to result from damage to the regions of the brain associated with it in the left hemisphere (Woollams et al. 2008). This damage can be either traumatic or acquired (Damasio 1992), with causes of traumatic brain injury including the likes of falls, accidents or violence involving a blow to the head, and causes of acquired brain injury poisoning, infection, strangulation, choking, drowning, stroke, heart attacks, brain tumours, aneurysms and even neurodegenerative diseases such as Parkinson's disease, Alzheimer's disease and Huntington's disease (Budd et al. 2010).

In part 2 of this chapter, I provided an overview of some theories of spoken word production. The theories were developed with the aim of accounting for the patterns of speech errors we see in normal subjects and evaluated in terms of their ability to do so, but one could (and indeed Nickels (1997) does) argue that a model of language processing cannot be considered sufficient or efficient unless it can account for impaired performance as well as performance that is unimpaired. Some authors (for example Butterworth (1989, 1992) and Monsell (1987)) have used neuropsychological data as support for the sufficiency/efficiency of some theoretical models over others and this – using neuropsychological data to inform theory about how the mind is modelled, that is – is critical, according to Nickels (1997: 99):

> I would argue that a model of language processing cannot be considered adequate unless it can account for patterns of impaired performance in addition to the "normal" speech error and experimental data. The use of neuropsychological data in this way rests on the assumption that patterns of language breakdown reflect the structure of the normal language system. Thus, "the differences between normal language mechanisms and the set of language mechanisms available to aphasics are [considered to be] subtractive—that is, the aphasic is simply lacking some of the components available to normal language speakers, rather than inventing new ones" (Saffran, 1982, p.318).

Now, whilst the assumption that aphasics' 'patterns of language breakdown reflect the structure of the normal language system' (Nickels 1997: 99) is by no means a universally accepted one, it is the basis of the neuropsychological approach to aphasia therapy that is used today and is, therefore, implicit in much of what follows here. Discussion of aphasia treatment, however, will be best left to section 4 – in this section, I will first describe the different deficits of spoken word production in aphasia and relate them to the theoretical models that were reviewed in the previous one.

As I've said, in the past people have concerned themselves with relating the type of breakdown in naming to specific aphasic syndrome categories. Benson (1979) was among the first researchers to do this; he drew a distinction between four anomias (word production anomia, paraphasic word production anomia, word selection anomia and nominal anomia) and related them to four aphasic syndromes (Broca's aphasia, Wernicke's aphasia, conduction aphasia and anomic aphasia, respectively). Syndrome labels aren't always reliable indicators of the locus of difficulty in word retrieval, however, (as we'll see in section 4), and so more recently researchers have reduced Benson's four forms down to just three (for example Lambon Ralph et al. (2000). Lambon Ralph et al. (2000) distinguish between semantic anomia (a word finding difficulty caused by damage to the semantic level of language processing), phonological anomia (a word finding difficulty caused by damage to the phonological) and classical anomia (a form of anomia in which patients present with word finding difficulties that are neither caused by damage to the semantic not the phonological levels of language processing).

In this present chapter I'd prefer to be a *bit* more specific about the phonological aspect of word retrieval than Lambon Ralph et al. (2000) are however and differentiate between phonological (or word form) anomia and something called disordered phoneme assembly as Laine and Martin (2006) do in *Anomia: Theoretical and Clinical Aspects*. Like Lambon Ralph et al. (2000), Laine and Martin (2006) also argue for three anomias, but whereas Lambon Ralph et al. (2000) argue for semantic, phonological and classical anomias, Laine and Martin (2006) argue for semantic anomia, word form (read phonological) anomia and disordered phoneme assembly, which correspond to the main stages of word production in a standard logogen model: retrieval of meaning (at the semantic stage), retrieval of the word form (from the phonological output lexicon) and programming the relevant phonological output (in the phonological output buffer).

These anomia types also find their correspondences in the more recently founded functional models Laine and Martin (2006) point out (such as Levelt et al. (1999's) model of word retrieval). In Levelt et al's (1999) model, 'semantic anomia would roughly correspond to conceptual- and lemma-level deficits' and '[w]ord form [that is, phonological] anomia […] to difficulties in accessing the lexeme that specifies […] phonological information of the to-be-produced target word' they say (Laine and Martin 2006: 37). Disordered phoneme assembly, meanwhile, corresponds to 'impairments in the later processes of [e.g.] phonetic encoding that are needed to create the "articulatory score" of the target word for motor output' (Laine and Martin 2006: 37). This, of course, has nothing to do with lexical access (a point I'll come back to in section 3.2). To reiterate what I said in section 2.4, though, aphasiology models of word retrieval's phonological levels are grounded in substance. If phonology is substance-free, then a model which makes use of an interface theory like Jackendoff's (2002) would account for the inner workings of the mind more accurately. Phonological anomia and disordered phoneme assembly could still be differentiated if the phonological component of the model were to be substance-free. In Jackendoff's (2002) model, semantic anomia could be explained by damage to the semantic integrative module, phonological anomia the phonological integrative module and disordered phoneme assembly the interface module between phonology and phonetics.

Now, regardless of the differences in terminology, Benson's (1979) differentiation between semantic anomia and word selection anomia, Lambon Ralph et al. (2000)'s between semantic and phonological and Laine and Martin's (2006) between semantic and word form are all alike in the way that they separate semantic processes from phonological ones, and I will take this division as the starting point in my discussion. Discussions of the breakdown of word retrieval in aphasia have often centred around two specific papers: Howard and Orchard-Lisle's (1984) and Kay and Ellis' (1987) which each detail a single case study of an aphasic subject with impaired picture naming. Ellis and Young (1988) used these two subjects (JCU from Howard and Orchard-Lisle (1984) and EST from Kay and Ellis (1987)) to illustrate the division between semantic and phonological levels of impairment in naming, and in the following subsections, I will do the same.

The remainder of this section is split into two halves. In the first, I consider semantic anomia and in the second phonological, as well as deficits beyond phonological access in section 3.2.2 of 3.2 to explain why they won't be a topic of discussion in section 4.

3.1 Semantic anomia

Unsuccessful attempts at word retrieval often result in speech errors that resemble the target in meaning (semantic speech errors) or in sound (phonological speech errors). These are taken to be symptomatic of anomia. The first, semantic speech errors (or more accurately, semantic *paraphasias* with which irretrievable target words are substituted with words that are semantically related to them such as *refraction* for *reflection* (which both have to do with the dispersion of light (Damasio 1992; Fitzgerald 1996; Marshall et al. 1998)) are most commonly observed in people with semantic anomia. Phonological paraphasias, on the other hand, which are the substitutions of target words with words or nonwords that resemble them phonologically (like *viscosity* ([vɪskɒsɪti]) for *velocity* ([vɪlɒsɪti]) (a real word, formal error), for example, or *chromosome* ([krəʊməsəʊn]) for *chromosome* ([krəʊməsəʊm]) (a nonword phonemic error) (Brookshire 1996)) are said to be symptomatic of phonological anomia.

Semantic paraphasias suggest that while phonological information about a target word can be successfully retrieved by the patient, not all of its semantic information can be (Nickels 1997). Phonological paraphasias, on the other hand, sometimes suggest that while semantic information about a target is available for retrieval, there are problems with a patient's access to the sounds constituting its spoken form (Nickels 1997), (though that isn't *always* the case, as we'll soon see).

This section starts by reviewing some case studies of patients who produce semantic paraphasias in their attempts at word retrieval and are as such thought to have deficits at the semantic level of language processing. This is shortly followed by a review of studies in which participants produce phonological paraphasias (in 3.2), who are thought to have deficits at the phonological one. I must point out here, however, that I use the term 'semantic system' somewhat loosely throughout the thesis, as the nature of semantic representation is a matter of dispute. While some researchers (e.g. Levelt 1989) have argued for separate conceptual- and lexical-semantic levels in word retrieval, others (e.g. Riccoch et al. 1988; Caramazza et al. 1990; Hillis et al. 1990) have argued for a single, modality-independent central system. People have even 'argued for a multicomponent semantic system that separates visual and verbal semantics (Paivio 1991) or is organised along the different sensory-functional domains (Warrington & Shallice, 1984)' (Laine and Martin 2006: 40).

I will use semantic system to refer to all of these things, but will draw distinctions in what follows for purposes of clarity. A question that begs to be answered in this thesis as well (and so I do that in sections 3.1.1 and 3.1.2), though, is 'which semantic errors reflect impaired access to conceptual-semantic representations and which reflect permanent erasure of conceptual-semantic representations from long-term memory (Laine and Martin 2006)'? Progressive neurological diseases are thought to lead to either impaired access to (as in cases of progressive fluent aphasia or progressive non-fluent aphasia) or loss of (as in cases of semantic dementia) representations. Aphasias associated with non-progressive neurological damage such as strokes and head injuries, meanwhile, are thought only to impair access (Laine and Martin 2006).

As you've likely deduced from the discussion thus far, then, I'll be describing the difference between these through an exploration of cases found in the aphasiology literature. I'll study Howard and Orchard-Lisle's (1984) JCU and Hillis et al.'s (1990) KE in section 3.1.1, and Murre et al.'s (2001) AM in section 3.1.2.

3.1.1 Impairment in access to conceptual-semantic representations

Howard and Orchard-Lisle (1984) describe the case of JCU, a global aphasic who was rendered with a large fronto-temporo-parietal haemorrhage lesion in her left hemisphere and a smaller one in her right following a stroke. Both her comprehension and production was impaired, they said, with spontaneous speech being restricted to simple *yes/no* responses and a repertoire of recurrent utterances like *I don't understand.* Picture naming was poor too (JCU was only able to name 5 out of a possible 30 items from the Western Aphasia Battery (an instrument for the assessment of aphasics) correctly (Kertesz and Poole 1974)) but she was sensitive to phonemic cueing (a type of anomia therapy). When provided with phonemic cues (such as the first sound /t/ of the target *tiger*) JCU was able to name a further 18 out of 25 items correctly. Howard and Orchard-Lisle (1984) investigated in detail the effect of phonemic cues on her naming and found them to not only be highly effective for JCU, but that she could be induced to produce semantic errors when provided with false cues that were semantically related to the target. The target *tiger* could be cued by /l/ for example, for /l/ is the initial phoneme of *lion, tiger*'s semantic coordinate.

When these errors were re-presented to her in a picture verification task (e.g. she was shown a picture of a tiger and asked *Is this a lion?)*, JCU incorrectly accepted 55% of her semantic errors as the correct name for the picture. These observations, together with the one that JCU's output phonology (which was measured by single word repetition tasks) was preserved, more or less, led Howard and Orchard-Lisle (1984) to conclude that JCU's semantic errors were due to underspecified semantic representations at the semantic level of language processing.

Hillis et al. (1990), on the other hand, described the case of KE, a 52 year old right handed male who'd had a thrombo-embolic stroke six months before the study started. Like JCU, KE's speech output was limited, but for KE this was to single nouns and well-known phrases. Repetition of words and nonwords was preserved, and phonological paraphasias were few and far between. Hillis et al. (1990) tested KE's performance on a number of single word production tasks (auditory word to picture matching, visual word to picture matching, oral naming, written naming, oral reading and writing to dictation) and found that he made significant semantic errors in all (as can be seen by the results in Table 5.1, which depicts his performance on semantic tasks):

Task	**Total correct (n=144)**	**% correct**	**Semantic errors/normal errors**	**% semantic errors**
Auditory word to picture matching	83	58	58/61	95
Visual word to picture matching	91	63	39/53	74
Oral naming	80	56	59/64	92
Written naming	77	53	50/67	75
Oral reading	84	58	52/60	87
Writing to dictation	84	58	40/60	67

Table 5.1 KE's performance on semantic tasks (adapted from Hillis et al. 1990: 203)

KE's semantic processing was so impaired, in fact, that he made different errors for the same stimulus across a number of the tasks (when presented with a picture of an arm in an oral naming task and asked its name KE said *finger,* e.g., but when shown the same picture in a written naming task he said *leg,* when reading aloud he said *ear* and when writing to dictation *hand*). This led Hillis et al. (1990) to conclude that KE had a central semantic processing deficit that could be explained within a single modality-independent semantic system that represents the perceptual, functional and relational components of concepts like the one in Morton's (1970) logogen model.

Howard and Orchard-Lisle's (1984) patient JCU's deficits couldn't, on the contrary, be accounted for by this sort of structure though Butterworth et al. (1984) point out, for although JCU was impaired in verbal comprehension tasks, she performed perfectly normally in a nonverbal picture association task making her word retrieval difficulties lexical-semantic in nature. JCU's pattern of impairments could, however, be explained by two-step models like their own, they say, that propose partially independent modality-specific semantic systems.[7] According to Butterworth et al. (1984), within their model, an impairment at the semantic lexicon level with unimpaired semantic/conceptual representations would give rise to the kind of deficits JCU displays.

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[7] Clearly, then, it could be described by Levelt et al.'s (1999) WEAVER++ model as well.

3.1.2 Permanent erasure of conceptual-semantic representations from long-term memory

In their article *Slowly Progressive Aphasia without Generalized Dementia* Mesulam (1982) described six patients with progressive aphasia ('a slowly progressing aphasic disorder without the additional intellectual and behavioural disturbances of dementia' (Mesulam 1982: 592)). While these were not the first reported cases of the condition (see Pick (1892), Warrington (1975) and Schwartz et al. (1979) for other representative examples), Mesulam's (1982) paper was the first to highlight the fact that patients with neurodegenerative diseases could present with focal cognitive deficits (Murre et al. 2001). Following the publication of Mesulam's (1982) seminal report, over a hundred patients with progressive aphasia have been recorded. It's become clear from these, though, that the term has been used to describe two very different disorders: progressive *non-fluent* aphasia and progressive *fluent* aphasia (see Hodges and Patterson 1996 for a more thorough review).

Now, whilst the phonological and syntactic aspects of language are damaged in patients with non-fluent aphasia, patients with progressive fluent aphasia's speech structures are not (Croot et al. 1998). They do, progressive fluent aphasics, that is, display difficulty in producing the names of what were once familiar people, places and objects (i.e. noun retrieval) however, as well as deficits in word comprehension, failing to understand the most straightforward of questions sometimes and/or keep up with conversational discourse.

Deficits are seen on a number of different verbal based semantic tasks such as picture naming, word to picture matching, category fluency (e.g. 'producing as many exemplars from a semantic category (e.g. animals) in 1 min' (Murre et al. 2001: 649)), picture sorting (i.e. 'grouping black-and-white line drawings depending on various pre-specified criteria, such as living versus non-living; electrical versus non-electrical and so on' (Murre et al. 2001: 649)), naming an item when given a verbal description of it (e.g. 'toaster' for 'an electrical kitchen appliance that is used for browning bread' (Murre 2001: 649)) and the other way round, verbally describing an item when given its name (i.e. 'an electrical kitchen appliance that is used for browning bread' for 'toaster') (Hodges et al. 1992; Hodges and Patterson 1996; Murre et al. 2001).

On non-verbal testing of semantic memory, patients show deficits when they're asked to select the appropriate colours for black and white line drawings of what were once familiar objects (such as 'green' for an apple and 'orange' for an orange), to use once familiar objects appropriately (such as a comb to comb their hair), to draw once familiar objects from memory and to match common animal sounds to appropriate pictures (such as a meow to a cat and and a bark to a dog) (Bozeat et al. 2000; Hodges et al. 2000; Murre et al. 2001). Patients typically perform well on tests of non-verbal problem solving, visuo-perceptual and spatial ability and working memory though, even in the latter stages of the disease (Breedin et al. 1994; Hodges et al. 1994; Waltz et al. 1999; Murre et al. 2001).

The relatively selective loss of semantic memory seen in progressive fluent aphasia has led researchers (e.g. Snowden et al. 1989; Hodges et al. 1992; Breedin et al. 1994) to adopt the term 'semantic dementia' for it instead, and in 1992 Hodges et al.'s put forward five diagnostic features, which were succinctly summarised in 1997 by Nickels as follows (Nickels 1997: 113):

(i) selective impairment of semantic memory causing severe anomia, impaired spoken and written single-word comprehension, reduced generation of exemplars on category fluency tests and an impoverished fund of general knowledge;
(ii) relative sparing of other components of language output and comprehension, notably syntax and phonology;
(iii) normal perceptual skills and nonverbal problem-solving abilities;
(iv) relatively preserved autobiographical and day-to-day (episodic) memory and
(v) a reading disorder with the pattern of surface dyslexia.

Hodges et al. (1992) went on to describe five patients who presented with these features, and whilst all five of the patients' semantic systems were impaired, one patient (PP) was affected more than most. When asked whether she'd ever been to America, for example, PP replied 'what's America?', and when asked what her favourite food was replied 'food, food, I wish I knew what that was'. What's most interesting though is that while PP could match a photograph of an object to an unusual view of that object (i.e. another photograph of it taken from above, below or the side) when presented with visually similar distractors, she was unable to name any of the objects in question.

With regards to picture sorting tests from the Semantic Memory Test Battery (which, rather self explanatorily, is used to test participants' abilities to remember meaning) all five of Hodges et al.'s (1992) patients performed poorly, able to pictures using broad distinctions (such as living vs. not living) but unable to do so with subtler distinctions (such as British vs. foreign animal or fierce vs. non-fierce animal). When it came to intermediate level distinctions (such as separating household items from musical instruments from vehicles and land animals from sea creatures from birds), PP was impaired on those too (though the others weren't significantly affected).

Perhaps the most representative case of semantic dementia, however, is the one described in Murre et al. (2001) of AM. AM was a 64 year old well-educated man with an undergraduate degree in engineering and a master's degree in science. He worked in management (responsible for over 450 employees) for a renowned international company and was referred for clinical examination after presenting to his general practitioner with progressive word finding and comprehension difficulties. A structural MRI revealed temporal atrophy in the left hemisphere of his brain, and upon examination, his speech was semantically empty (though fluent with preserved syntax and phonology).

When it came to picture naming AM was considerably compromised, scoring only 3 out of 48 correct when tested on a set of high frequency items that should have been familiar to him (Knott et al. 1997). He most frequently responded with nothing at all, but on occasion produced some short, circumlocutory responses that were very vague. Semantic paraphasias were rare, and no phonological paraphasias were observed at all in AM's naming. AM's performance on a word-picture matching task using the same items that were elicited by the picture naming task was relatively normal, and the same was true for other word-picture matching tasks (though he did display a frequency effect with better comprehension of higher-frequency items than lower-frequency ones). Of the errors AM produced on the word-picture matching tasks semantic ones were most common, and he was also impaired on a semantic feature questionnaire which required him to answer with a yes or no to simple questions such as 'Does an ostrich have a long neck?'.

The nonverbal version of the Pyramids and Palm Trees test (which was made to measure conceptual knowledge) proved particularly difficult for AM: whilst his repetition of single words and nonwords was within normal limits for items up to three syllables long, he had a strong tendency for phonological errors in longer words and nonwords, and words of a low frequency. Further evidence for a conceptual-level impairment came from AM's difficulties in the appropriate selection and use of everyday items (he put a closed umbrella over his head horizontally in a rain storm and orange juice into his lasagne, for example).
A sample of his AM's speech demonstrating the extent to which he was unable to retrieve words is given below (Murre et al. 2001: 651):

> *Examiner:* Can you tell me about a time you were in hospital?
> AM: Well one of the best places was in April last year here (ha ha) and then April, May, June, July, August, September and then October, and then April today.
> Examiner: Can you remember April last year?
> AM: April last year, that was the first time, and eh, on the Monday, for example, they were checking all my whatsit, and that was the first time, when my brain was, eh, shown, you know, you know that bar of the brain (indicates left), not the, the other one was okay, but that was lousy, so they did that and then doing everything like that, like this and probably a bit better than I am just now (indicates scanning by moving his hands over his head).

Now, although the three cases presented here (JCU from Howard and Orchard-Lisle (1984), KE from Hillis et al. (1990) and AM from Murre et al. (2001)) all exemplify patterns of deficit that can be called semantic anomia, AM differs from JCU and KE in that his semantic anomia is due to dementia, not aphasia, and therefore cannot be treated. Treatments for semantic anomia in aphasia are designed to strengthen connections between semantic and lexical representations (Jefferies and Lambon Ralph 2006; Antonucci and Reilly 2008), but in dementia, it is too late for that as those connections haven't been weakened through damage but lost altogether (Peach and Shapiro 2012). This thesis will, therefore, look at cases of semantic anomia in aphasia only, from here on out. No more will be said about disorders of semantic memory, and cases of them that are in the collection of texts I extract my data from will be excluded from my analysis in section 4.

3.2 Phonological anomia

In this section, I turn my attention to the wide range of phenomena that have to do with deficits in the retrieval of a lexical item's phonological form. Patients with phonological anomia are those that have intact processing at the semantic level, but fail to retrieve the phonological forms of target words in output (Nickels 1997). Phonological errors can occur for one of two reasons, though: a) because there is impaired access to the phonological representations themselves (the reason which will be explored in section 3.2.1) or b) because of impairments in the processes involved in phonological encoding (which is the one that'll be focus of 3.2.2) (Nickels 1997). Only the first kind of error can be attributed to phonological anomia, however, as only these have to do with lexical access. The second kind of error is due to some post-lexical impairment, as section 3.2.2 will explain.

3.2.1 Impairment in access to output lexicons from the lexical-semantic system

The first possible post-semantic place of impairment that could lead to anomia is in the connections between semantics and the output lexicon, Laine and Martin (2006) say. With this, one would expect to find word retrieval impairments with unimpaired comprehension and normal performance on word production tasks that can be completed without semantic support (Laine and Martin 2006).

The most clear-cut cases of impairment in access to output lexicons from the lexical-semantic system are perhaps those that were provided by Lambon Ralph et al. (2000), who reported on two traumatic head injury patients (GM and JS). GM's word retrieval difficulties were milder than those of JS: his score on the Boston Naming test (a tool that's used in speech and language pathology to determine patients' abilities to name) was 31/60 while JS' was 16/60. When GM failed to retrieve a target, he tended to produce circumlocutions such as 'the burial chamber for Egyptian kings… it's got three 'beats'' for 'pyramid', though semantic errors (that were spontaneously rejected) were sometimes produced as well. GM evidenced tip of the tongue behaviour too, in that he was able to provide phonological information about targets (such as the number of syllables they were made up of, as evidenced by 'three 'beats'' for 'pyramid') in the aforementioned example. JS, on the other hand, didn't evidence any phonological knowledge, and circumlocutions for JS were produced upon request only. For JS omissions were the most frequently occurring error type, though he did produce (and like GM, spontaneously reject) semantic errors as well.

Neither GM nor JS produced any phonological paraphasias in speech output tasks, and they both performed within the normal range on word comprehension and semantic-associative tasks (with the exception of GM on something called the PALPA Word Semantic Association task (which is used to check for impairments in semantic processing) where he had problems with abstract items). Repetition and reading performances on both words and nonwords were also within normal limits, which (together with the rest of the results) would suggest semantic processing (but not phonological processing) was intact in both patients.

The data paints a picture, Lambon Ralph et al. (2000) argue, of patients who have access to the meanings of target words (and sometimes even partial information about them), but not enough phonological information to retrieve the targets as nonanomics would be able to. GM and JS' lack of access to full phonological information leads to a selection problem, they say, whereby semantically close alternatives to targets with more easily accessible phonologies compete for output (and sometimes win).

Similar patterns were reported in 1987 by Kay and Ellis who described the case of EST, a patient whose word retrieval deficits were dramatic, but speech was so fluent that he was able to conceal them in spontaneous speech by using alternative words to, or circumlocutions in place of, the target. In picture naming tasks he produced both semantic and phonological errors and in auditory-verbal repetition tasks better repetition of real words than non-words, but a semantic deficit couldn't be the cause of EST's anomia Kay and Ellis (1987) argued, for he showed complete comprehension of the object names he was unable to process for production.

Kay and Ellis (1987) attributed EST's anomia to a difficulty in activating spoken word forms in an intact phonological (output) lexicon instead, stressing that it was his access to items that was impaired not the items themselves, as evidenced by his ability to retrieve names successfully sometimes after extensive effort. They also assembled a more formal argument for their assertion, pointing out that EST showing a superiority for words over non-words in auditory-vocal repetition tasks could very well suggest 'that [the] lexical phonological entries for such words [could] in fact function in response to [that] type of stimulus and […] contribute to successful word repetition performance' (Kay and Ellis 1987: 626).

3.2.2 Impaired phoneme assembly

A distinction must be made, however, between phonological errors that occur as a result of difficulties with lexical access as in 3.2.1 and phonological errors that occur as a result of something further 'down' models of spoken word retrieval, after the phonological form has been retrieved. Lexical access (as you'll recall from section 2 of this chapter) is generally thought of by Butterworth (1989) and Levelt et al. (1999) as being made up of a number of hierarchically organised processes. According to Levelt et al.'s (1999) lexical access theory, as an example, whereby after the phonological form has been retrieved it is encoded with information interpretable by the system that's responsible for its articulation (Laine and Martin 2006). To my mind, deficits in this domain should not be thought of as phonological anomia, then, since phonological anomia is a lexical retrieval deficit having to do with an inability in accessing the phonological form, and these deficits appear post-lexically after the phonological form has already been retrieved.

The terms phonological encoding and phonetic encoding and phonological assembly and phoneme assembly are used interchangeably in the literature to mean the same thing, but the danger, I feel, in doing this is that it blurs the distinction between phonetics and phonology (which, to my mind, are, of course, two entirely different things). The terms phonological encoding and phonological assembly being used in the aphasiology literature to describe a disorder that is post-phonological in nature can be confusing, I feel, and so I'll use the terms phonetic encoding and phoneme assembly only from this point onwards. Recall also that disorders of phonetic encoding/phoneme assembly can easily be described in Jackendoff's (2002) model which argues for a phonological integrative module (which could be the locus of phonological anomia) and a phonology-phonetics interface module (which could be the locus of disorders of phonetic encoding/phoneme assembly.

The classical aphasic syndrome that's been linked to anomia due to impaired phoneme assembly is conduction aphasia (Laine and Martin 2006). This is a form of fluent aphasia in which comprehension is fairly well preserved, but speech output (which is otherwise well articulated, actually) is punctuated by phonological paraphasias (Laine and Martin 2006). These errors are often quite close to the target, which suggests that the patient producing them was able to retrieve the correct lexical-phonological representations for them, but failed with prearticulatory (i.e. phonemic) planning (Kohn 1984). People with conduction aphasia are particularly sensitive to word length, with longer words causing more production problems than shorter ones due to the larger amounts of phonological information they're made up of (Laine and Martin 2006).

A typical case of conduction aphasia has been described by Laine et al. (1992) who explored the nature of naming deficits in 10 people with classical aphasia syndromes. C2, a 64 year old patient with symptoms of conduction aphasia was mildly anomic, scoring 44/60 on the Boston Naming Test and below normal performance on two others (a 106 picture naming task and a synonym production task). Over half of C2's naming errors were phonological, and of those that were left, only one was semantic (though even that he corrected). C2 scored perfect performance on a semantically based picture classification task, a word to picture matching task and a visual odd-one-out semantic-associative task, and these things, coupled with the fact that estimated familiarity of targets had no bearing on success, led Laine et al. (1992) to conclude that C2 had a phoneme assembly deficit that affects spontaneous speech, oral naming and word repetition.

The phonological errors e.g. ‘beelwharrow’ for ‘wheelbarrow’ that occur in *nonaphasic* slips of the tongue, note, are generally attributed to errors in post-lexical phonological processes (such as the copying of segments into slots, for example, in a slot-and-filler model like Shattuk-Hufangel (1979)’s, for their speaker’s phonological knowledge is intact (Nickels 1997). When aphasic subjects are in tip of the tongue states however, only partial phonological information (such as the initial phoneme of the word or the number of syllables it has) is available for lexical retrieval, and so more complex phonological real word and nonword errors (such as ‘Siam’ (a real word error) and ‘sympoon’ (a nonword error) for the target ‘sampan’ (Brown and McNeill 1966)) are produced.

The phonological errors observed can help researchers reach a conclusion about the cause of them, but there are many more reliable ways of distinguishing deficits of lexical access and post-lexical phoneme assembly deficits than that, such as ‘silent phonology’ tasks, for instance. In these types of task, subjects are asked questions such as ‘Do ‘write’ and ‘right’ sound the same?’ (a task that requires a judgement of homophony) and ‘Do ‘white’ and ‘bright’ rhyme?’ (a task requiring, rather self explanatorily, a judgment of rhyme). Pseudo-homophone detection judgement tasks (e.g. ‘Which one sounds like a real word - ‘brane’ or ‘plane’?’) may be used too, as may phonological lexical decision judgement tasks (e.g. ‘Does ‘brane’ sound like a real word or not?’). Whether or not patients can perform these types of task demonstrates whether or not they can access phonological representations. If they are better at these tasks than they are at producing the same words, we can conclude that the problem is post-lexical in nature.

Since this chapter has to do with lexical access (and therefore word retrieval deficits that are lexical and not post-lexical in nature), I won’t be including participants with disorders of phoneme assembly in my analysis of anomia therapy in section 4. Researchers don’t always specify the cause of their participants’ phonological anomias, however; so if the cause of a participant’s phonological anomia is unclear, I won’t include them in my analysis either. An alternative option would be for me to assess the phonological errors myself in order to work out whether they are due to impaired access to the phonological representations themselves or because of impairments in the processes involved in retrieving the representations for phone assembly, but given that I’m not a clinician I’m clearly not qualified to do so. My analysis will, therefore, include the following: participants with semantic anomia (which always does have to do with lexical access) and participants with true phonological anomia (i.e. participants whose phonological disorder been diagnosed (by aphasiologists) as being at the lexical level). This I'll, of course, come back to when I discuss my data source and sampling in 4.1.

4. Anomia therapy

In this section I will be comparing the effects of semantic and phonological therapies at treating semantic and phonological anomia in order to see whether the semantic and phonological processors are domain specific and therefore modular by Carruthers (2006a)'s definition. If semantic therapy was found to treat phonological anomia (recall from the introduction to this chapter (and indeed the introduction to the thesis in general)) this would count as clear evidence that the phonological processor was able to operate on semantic input and so not modular, for domain specific processors, recall from chapter 3, are those that are able to only operate on input that's relevant to their function. Input relevant to the function of the phonological processor would, of course, be phonological, not semantic, and so it will be interesting to see how the different therapy's efficacies at treating phonological anomia pattern.

4.1 Data source and sampling

Now, since impairments of word retrieval are a common symptom of aphasia, much clinical time has been spent attempting their remediation. Anomia therapy has focused on improving access to words' meanings in the semantic system (semantic therapy) and improving access to words' representations in the phonological output lexicon (phonological therapy), but there is a complex relationship between the types of language deficits patients have and the types of tasks that are used in therapy to remediate them (Whitworth et al. 2013). Semantic tasks, for example, have been used with the aim of facilitating word retrieval in people with semantic impairments as well as people with phonological impairments. Phonological tasks, meanwhile, have been used to treat both phonological anomia and semantic anomia too, and so tasks aren't always targeted to the level of language breakdown like they were a long time ago.

Since semantic tasks focus on word meanings, examples include word to picture matching (both spoken and written), word to picture verification, feature matching and feature verification, categorisation, relatedness judgements, generation of semantic features and the provision of semantic information in cues (Whitworth et al. 2013). Phonological tasks, on the other hand, which promote access to word forms, include the likes of repetition and reading aloud, as well as tasks which ask participants to reflect more explicitly on a words' phonological features, e.g. by counting syllables, identifying initial phonemes or syllables or performing rhyme judgments. The provision of phonological and orthographic cues by therapists are counted as phonological tasks too, in the literature, and although orthographic approaches to treatment mightn't at first glance look very phonological for they have to do with graphemes, not phonemes, I feel this warrants a bit of discussion here as there is evidence to suggest that orthographic cues work just as well as phonological ones do (e.g. Bruce and Howard 1987; Best et al. 1997; Basso et al. 2001; Hickin et al. 2002a).

In 1987, Bruce and Howard successfully treated five people with word finding difficulties with an aid that made use of orthographic cues. This was replicated by Best et al. (1997) in a single case study, and it was found that both graphemic and phonemic cues improved naming. Best et al. (1997) suggested that the reason why orthographic cues work is because they enable the client to develop an (unconscious) strategy whereby they link graphemes to their respective insufficiently activated phonemes so that enough activation to facilitate their retrieval can be delivered: '[i]t appears that the treatment affect[s] a fundamental change in […] word finding, altering automatic […] not strategic processes in [the] linguistic system' (Best et al. 1997: 134). With regards to orthographic approaches to treatment, then, like Bruce and Howard (1987), Best et al. (1997), Basso et al. (2001) and Hickin (2002a) (among others) I'll be counting them as phonological tasks in this chapter.[8]

In this thesis, the emphasis will be on studies that involve investigation of single cases or series of single cases which enable a detailed examination of patterns of deficit. In studies which combine patients into groups, the results are uninterpretable and therefore uninformative (Ellis and Young 1988). A number of research papers that are devoted to single case studies (and series of single case studies) spanning 1980 to 2002 that examine the efficacy of treatments for word retrieval impairments have been collected by Nickels (2002), and it's from that collection of papers that I compile my own.

The scope of my review has to be narrower than Nickels' (2002), though, for whilst Nickels (2002) restricted her review of the literature to papers published in peer reviewed journals and books that address impairment-level treatments of spoken word production in aphasia (and so did not review those that 'address[ed] treatments that […] use alternative (non-linguistic)

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[8] I will remove orthographic therapies from the data set at the end of Chapter 5, though, to see whether without them a different effect is observed for phonological therapy's efficacy at treating phonological anomia.

means of communication to overcome a word-retrieval/production impairment' (e.g. drawing (Hunt 1999; Sacchet et al. 1999)) (Nickels 2002: 936), those that 'focus[ed] on impairments at an articulatory level' (e.g. apraxia of speech (Ballard et al. 2000; Wambaugh and Doyle 1994)) (Nickels 2002: 937), 'treatments "beyond the single word"' (e.g. sentence processing (Mitchum et al. 2000)) (Nickels 2002: 937) or 'rehabilitation [that's] primarily focused at reducing levels of handicap/disability where the rehabilitation is not impairment-based' (e.g. Lesser and Algar 1995) (Nickels 2002: 937), my review restricts itself to those papers in Nickels (2002) that focus on the remediation of spoken word retrieval only (excluding importantly case studies where the causes of patients' deficits have to do with disorders of phoneme assembly).

As I said in section 3, this chapter is concerned with the retrieval of the phonological form, which means that word retrieval deficits that are post-lexical in nature and therefore don't have to do with lexical access such as disorders of phoneme assembly must be excluded from the analysis. If there were phonetics/phonology mismatches in the data set, what the efficacy of semantic therapies at treating phonological anomias could contribute to debates about the modularity of phonology is unclear.

Something I should also point out though, is that whilst Nickels (2002) reports on cases that have more than one damaged level of language impairment (e.g. AER, TRC and PA from Nickels and Best (1996) who have semantic and post-lexical, semantic and phonological and semantic and post-lexical impairments, respectively), I will include only the cases that have semantic impairments only, or phonological impairments only, in my analysis. Nickels (2002) also reports on cases that attempt to facilitate word retrieval with both semantically and phonologically targeted treatments (e.g. Aftonomos et al. 1997), but since I need to answer the question of whether the phonological processor is domain specific (and therefore able to operate only on phonological input) or not, I'd do better to compare the effects of semantically targeted treatments (that act as semantic input) and phonologically targeted treatments (that act as phonological input) have on phonological anomia only; mixed treatments which make use of both semantics and phonology wouldn't reveal anything about the domain specificity of phonology, for if they helped with word retrieval it would be unclear whether it was the semantic input or the phonological input in them that was responsible for the remediation.

Now, with that being said, of the 43 papers reporting treatment for impaired word retrieval summarised in Nickels (2002), only 10 met these criteria. The 33 papers that have been excluded from the analysis (and my reasons for excluding them) are presented in Table 5.2 below, but even in the 10 papers I do present I have had to exclude some *results* along the way (for being statistically insignificant), on the one hand, and *participants* (for their treatment having been mixed or for their impairments or treatment outcome being mixed or unknown) on the other. Where the latter was the case, I've made sure to explain the reasoning behind my exclusion of them in my qualitative analysis of the data sample to follow. And, when the reasons for exclusion were even more complicated than that (as was the case with, e.g. Pederson et al. (2001) and Fink et al. (2002)), I've mapped those out in the analysis as well.

Paper	**Participant**	**Impairment**	**Treatment**	**Outcome (treated)**	**Reason for exclusion**
Primarily semantic tasks					
Lowell et al. (1995)	BB	Conduction aphasia	Semantic	Yes	Impairment unknown
	BG	Anomic aphasia	Semantic	Yes	Impairment unknown
	SB	Conduction aphasia	Semantic	No	Impairment unknown
McNeil et al. (1997)	BO	Anomic aphasia	Semantic	Yes	Impairment unknown
McNeil et al. (1998)	BO	Anomic aphasia	Semantic	Yes	Impairment unknown
Nickels and Best (1996)	AER	Semantic and post-lexical	Semantic	No	Impairment mixed
			Semantic	Yes	Impairment mixed
			Semantic	Yes	Impairment mixed
	TRC	Semantic and phonological	Semantic	Yes	Impairment mixed
			Semantic	No	Impairment mixed
	PA	Semantic and post-lexical	Phonological	Yes	Impairment mixed
			Semantic	No	Impairment mixed
Semantic and phonological tasks					
Aftonomos et al. (1997)	VAMC patients 1, 2, 3	?	Semantic and phonological	?	More than one patient, impairment unknown, treatment mixed and outcome unknown
	Patients 1, 2 9, 11, 18, 20	?	Semantic and phonological	?	More than one patient, impairment unknown, treatment mixed and outcome unknown
	Patients 4, 8, 16, 17	?	Semantic and phonological	?	More than one patient, impairment unknown, treatment mixed and

					outcome unknown
Annoni et al. (1998)	JNH	Semantic and phonological	Semantic and phonological	?	Impairment mixed, treatment mixed and outcome unknown
	GE	Semantic	Semantic and phonological	?	Treatment mixed and outcome unknown
	EG	?	Semantic and phonological	?	Impairment unknown, treatment mixed and outcome unknown
Deloche et al. (1993)	RB	Surface dysgraphia	Orthographic and semantic	Yes	Impairment unknown and treatment mixed
	GC	Morpholexical and word retrieval	Orthographic and phonological	Yes	Impairment unknown and mixed and treatment mixed
Deloche et al. (1997)	B	Word finding	Orthographic, semantic and phonological	Yes	Impairment unknown and treatment mixed
	L	Word finding	Orthographic, semantic and phonological	Yes	Impairment unknown and treatment mixed
	A, D, J, Q, C, K	Word finding	Orthographic, semantic and phonological	Yes	More than one patient, impairment unknown and treatment mixed
	I, O	Word finding	Orthographic, semantic and phonological	Yes	More than one patient, impairment unknown and treatment mixed
	H, G, P, M, R	Word finding	Orthographic, semantic and phonological	Yes	More than one patient, impairment unknown and

					treatment mixed
	N	Word finding	Orthographic, semantic and phonological	No	Impairment unknown and treatment mixed
	E	Word finding	Orthographic, semantic and phonological	No	Impairment unknown and treatment mixed
	F	Word finding	Orthographic, semantic and phonological	No	Impairment unknown and treatment mixed
Eales and Pring (1998)	MB	Semantic	Semantic and phonological	Yes	Treatment mixed
	NO	?	Semantic and phonological	Yes	Impairment unknown and treatment mixed
	PU	Semantic	Semantic and phonological	Yes	Treatment mixed
	SK	?	Semantic and phonological	Yes	Impairment unknown and treatment mixed
Greenwald et al. (1995)	SS	Semantic and phonological	Semantic and phonological	Yes	Impairment mixed and treatment mixed
			Semantic and phonological	Yes	Impairment mixed and treatment mixed
			Semantic and phonological	No	Impairment mixed and treatment mixed
	MR	Semantic and phonological	Semantic and phonological	Yes	Impairment mixed and treatment mixed
			Semantic and phonological	Yes	Impairment mixed and treatment mixed
			Semantic and phonological	No	Impairment mixed and treatment mixed
Hillis (1998)	HG	Semantic and phonological	Semantic	Yes	Impairment mixed and

					treatment mixed
			Phonological	Yes	Impairment mixed and treatment mixed
			Phonological	Yes	Impairment mixed and treatment mixed
			Semantic and phonological	Yes	Impairment mixed and treatment mixed
Le Dorze (1991)	LR	Phonological	Semantic and orthographic	Yes	Treatment mixed
Le Dorze and Pitts (1995)	RT	Semantic and phonological	Semantic and phonological	Yes	Impairment mixed and treatment mixed
			Semantic and phonological	?	Impairment mixed and treatment mixed
			Semantic	?	Impairment mixed and treatment mixed
			Semantic	No	Impairment mixed and treatment mixed
Li et al. (1988)	-	Conduction aphasia	Semantic and phonological	No	Impairment unknown and treatment mixed
			Semantic	Yes	Impairment unknown and treatment mixed
Pederson et al. (2001)	KB	Phonological	Unsupervised computerised semantic	Yes	Treatment unsupervised and patients Danish
			Unsupervised computerised phonological	Yes	Treatment unsupervised and patients Danish
			Unsupervised computerised orthographic	Yes	Treatment unsupervised and patients Danish

	JI	Semantic	Unsupervised computerised semantic	Yes	Treatment unsupervised and patients Danish
			Unsupervised computerised phonological	Yes	Treatment unsupervised and patients Danish
			Unsupervised computerised orthographic	Yes	Treatment unsupervised and patients Danish
	RI	Phonological	Unsupervised computerised semantic	Yes	Treatment unsupervised and patients Danish
			Unsupervised computerised phonological	Yes	Treatment unsupervised and patients Danish
			Unsupervised computerised orthographic	Yes	Treatment unsupervised and patients Danish
Sbisa et al. (2001)	ML	?	Phonological	Yes	Impairment unknown
			Semantic	Yes	Impairment unknown
Spencer et al. (2000)	NR	Phonological and post-lexical	Semantic and phonological	Yes	Impairment mixed and treatment mixed
Thompson and Kearns (1981)	-	?	Semantic and phonological	Yes	Impairment unknown and treatment mixed
Primarily phonological tasks					
Basso et al. (2001)	RF	Anomia	Orthographic	Yes	Impairment unknown
			Phonological	No	Impairment unknown
			Phonological	Yes	Impairment unknown
	MR	Word retrieval	Orthographic	Yes	Impairment unknown
			Phonological	Yes	Impairment unknown
			Phonological	Yes	Impairment unknown
Beeson (1999)	ST	Post-lexical	Orthographic	Yes	Impairment neither

					semantic nor phonological
Carlomagno et al. (2001)	Patients 1-8	?	Phonological	?	More than one patient, impairment unknown and outcome unknown
			Phonological	?	More than one patient, impairment unknown and outcome unknown
Fink et al. (2002)	GM	Post-lexical	Phonological	Yes	Impairment neither semantic nor phonological
	AS	Phonological and semantic	Phonological	Yes	Impairment mixed
	BM	Phonological and semantic	Phonological	Yes	Impairment mixed
	EL	Phonological and post-lexical	Phonological	Yes	Impairment mixed and one is neither semantic nor phonological; treatment unsupervised
	EG	Post-lexical	Phonological	Yes	Impairment neither semantic nor phonological; treatment unsupervised
	RH	Phonological	Phonological	Yes	Treatment unsupervised
Kiran et al. (2001)	RD	Conduction aphasia	Orthographic and phonological	Yes	Impairment unknown
	RN	Conduction aphasia	Orthographic and phonological	Yes	Impairment unknown
Miceli et al. (1996)	RBO	Phonological and post-lexical	Phonological	Yes	Impairment mixed
	GMA	Phonological and post-lexical	Phonological	Yes	Impairment mixed
Murray and Karcher (2000)	HR	?	Orthographic	Yes	Impairment unknown

Raymer et al. (1993)	CG	Phonological and post-lexical	Phonological	Yes	Impairment mixed
	RJ	Phonological, semantic and post-lexical	Phonological	Yes	Impairment mixed
	MR	Phonological, semantic and post-lexical	Phonological	Yes	Impairment mixed
	RE	Phonological and semantic	Phonological	Yes	Impairment mixed
Robson et al. (1998)	GF	Phonological, post-lexical and semantic	Phonological	Yes	Impairment mixed
			Phonological	No	Impairment mixed
			Phonological	No	Impairment mixed
Sugishita et al. (1993)	HK, MH, SK	?	Phonological	Yes	More than one patient and impairment unknown
	MK, BY, ST, MT, TI, YA, YK	?	Phonological	Yes	More than one patient and impairment unknown
	AI, TD	?	Phonological	Yes	More than one patient and impairment unknown
	YH	?	Phonological	Yes	Impairment unknown
	TZ, TE	?	Phonological	No	More than one patient and impairment unknown
Pashek (1997)	KR	Apraxia of speech	Phonological	Yes	Impairment unknown
			Phonological	Yes	Impairment unknown
Pashek (1998)	WT	Noun and verb retrieval	Phonological	Yes	Impairment unknown
			Phonological	Yes	Impairment unknown
Rose et al. (2002)	AB	Phonological and post-lexical	Phonological and orthographic	Yes	Impairment mixed
			Phonological	Yes	Impairment mixed

			Phonological	Yes	Impairment mixed
Other tasks					
MB	MB	Phonological	Semantic	?	Outcome unknown
Thompson et al. (1986)	S1	Word retrieval	Semantic	?	Impairment unknown and outcome unknown
	S2	Word retrieval	Semantic	Yes	Impairment unknown
	S3	Word retrieval	Semantic	No	Impairment unknown

Table 5.2 A breakdown of the 33 single case (and series of single case) studies of word retrieval in Nickels (2002) that I've excluded from my analysis and my reasons for doing so

Something else I'd like to point out before I delve into the discussion of the case studies leftover though is this: whilst in 1985 Howard et al. drew a distinction between three terms: cueing (which has to do with the immediate effects of treatment), facilitation (which has to do with the effects of treatment measured some time later (e.g. an hour, say)), and therapy (which has to do with the long-term effects of treatment), I should point out here that when I say therapy I don't necessarily mean it in Howard et al. 1985's sense. Rather I use treatment and therapy interchangeably to mean the same thing, facilitate to mean remediate and cueing to refer specifically to types of semantic and phonological (treatments/therapies – see) that make use of semantic and phonological cues, respectively.

4.2 Data analysis

4.2.1 A qualitative analysis of the data

4.2.1.1 Primarily semantic tasks

4.2.1.1.1 Marshall et al. (1990)

Marshall et al. (1990) undertook three single case studies to examine the effects word to picture matching tasks had on word retrieval in patients with aphasia. The patients studied differed in their deficits (one having a deficit in access to the phonological output lexicon and the other two in semantics), but all three were treated with semantic therapy (semantic discrimination tasks).

The first case, RS, was a 45 year old company director who'd suffered a left hemisphere cerebrovascular accident (CVA) in November 1986 which resulted in both dysphagia and right hemiplegia. When seen ten months later in 1987 RS had good comprehension still but had developed anomia, with a particular difficulty in retrieving verbs. On a word to picture matching test (owing to Kay et al. (1989) in which words are matched with pictures (one being the target and the other four semantic distractors)), RS scored 39/40, and on a spoken synonym matching test (whereby pairs of high- or low-imagery words are judged for similarity of meaning) RS scored better on high-imagery words than he did low- ones (36/38 compared with 26/38, respectively). On the pictorial version of the Pyramids and Palm trees test (Howard and Orchard-Lisle 1984) (which looks at semantic associations between objects), RS made three errors only, which, it should be noted, is within the normal range.

RS was able to readily discriminate between correct and inappropriate names for pictures, but could not be induced into making semantic errors with semantic or phonological cueing. With regards to written stimuli, on these words, he also performed adequately. When it came to RS' ability to read aloud single words he was almost unimpaired, reading all classes of words well and both high- and low-imagery words without error. RS' deficits led Marshall et al. (1990) to conclude that RS had a deficit in accessing words from the phonological output lexicon. Not in semantics, not in output phonology (i.e. phoneme assembly) but in the route connecting these, rather.

The type of treatment used to remediate RS' word retrieval was semantic. Given 50 drawings of low frequency words to name, 25 of these were to be treated whilst the other 25 acted as controls. Over a 2 week period around 3 hours of therapy time was devoted to semantic matching tasks with the treated groups, and at the end of the 2 weeks it became evident that RS was significantly better at naming treated items than untreated items (20/25 compared with 10/25, $x^2 = 5.16$, $p < 0.05$). This suggests, at first glance, that the phonological processor isn't domain specific and therefore massively modular, for semantic therapy was found to facilitate naming.

IS, on the other hand, was a 76 year old retired civil servant who had been left dysphasic and right hemiplegic too following a left hemisphere CVA in the January of 1988. After a period of intensive speech therapy, IS was discharged to a nursing home 4 months later in May, where she remained up until Marshall et al. (1990) conducted their study. Like RS IS presented with good comprehension of simple conversation, but her deficits seemed more likely to be semantic in nature for she had poor expressive language that was non-fluent and limited to just social phrases such as greetings and salutations and high frequency nouns. IS had quite severe word retrieval difficulties and performed poorly on semantic tasks.

On Kay et al.'s (1989) word to picture matching task IS performed poorly (scoring 25/40 for spoken stimuli and 19/40 for written). On a synonym matching task with high- and low-imagery items IS scored just 24/38 for spoken presentations and 21/38 for written ones and on the Pyramids and Palm Trees test (Howard and Orchard-Lisle 1984) just 34/50. IS' poor performance on semantic tasks in conjunction with the fact that IS made 0 errors on a test when phonemic distractions were used but only 14/20 when semantic distractors were made for a diagnosis of semantic anomia, which Marshall et al. (1990) tried to treat with a semantic therapy similar to that they used with RS.

A hundred drawings were presented to IS for naming which were divided into 4 groups of 25. Group 1 was treated after a 2 week period and Group 2 after 4 weeks. Groups 3 and 4 (the control groups) remained untreated. Around 2 to 2 and a half hours was devoted to therapy in each 2 week period, and two types of picture matching tasks were used.

The scores (out of 25) for the 4 groups showed that improved naming followed treatment. IS scored 10/25 on Group 1 before treatment, and 22/25 after it. On Group 2, meanwhile, IS scored 10/25 before treatment as she did on Group 1, and after treatment this score increased to 23/25. After the first 2 weeks, IS performed significantly better on Group 1, which she'd received treatment for than she did Group 2, which she hadn't ($x^2 = 6.10$, $p < 0.02$). Following the second period of treatment (after 4 weeks) on Group 2, IS performed significantly better on this than she did on Groups 3 and 4 ($x^2 = 11.86$, $p < 0.01$) see Table 5.3 for a breakdown of IS' scores which demonstrate the positive effect semantic therapy had on word retrieval in semantic anomia, which one would expect if semantics were a domain specific module of mind:

	Before treatment		**After 2 weeks**		**After 4 weeks**
Group 1	10	Treated	22	Not treated	20
Group 2	10	Not treated	13	Treated	23
Group 3	9	Not treated	12	Not treated	14
Group 4	7	Not treated	13	Not treated	10

Table 5.3 IS' scores (out of 25) for the four groups at successive testing (adapted from Marshall et al. 1990)

The third case (of FW, a 76 year old woman who'd suffered a left hemisphere CVA in the May of 87) was similar to that of IS. FW's speech was marked by severe word finding difficulties that were said by Marshall et al. (1990) to be due to a semantic comprehension deficit (as evidenced by semantic errors in naming (13 were produced when trying to name 120 line drawings) and poor performance on Kay et al.'s (1989) word to picture matching test (in which she scored 28/40 for auditory presentation and 23/40 for visual).

During therapy, FW was given 50 drawings but was only able to name 17 of them. These were divided into a treatment group and a control group (9/25 and 8/25 of the correctly named items in each of them, respectively). Picture matching tasks, though, administered for around 3 and a half hours each week over a 3 week period showed that unlike IS', FW's semantic impairment couldn't be treated by semantic therapy. A slight improvement was seen in naming, for the treated group (from 8/25 correct to 11/25 correct), but this result was not a significant one.

Marshall et al. (1990) also conducted a group study with RS, IS and FW which made use of a similar therapeutic task, but was required to be carried out in their own homes with a relative or volunteer, as opposed to a therapist, assisting. Semantically treated items showed significant gains when compared with controls, which is interesting, but since no therapist was present at the time of testing and testing was not recorded I've decided not to include the results of the group studies in my analysis, for since the accurate administration of treatment can't be guaranteed, there is nothing to say that quality was controlled.

4.2.1.1.2 Marshall et al. (1998)

Marshall et al. (1998) reported on four patients diagnosed with jargon aphasia (RMM, TD, JC and CM). Of these four patients, all had problems with word retrieval as was evidenced by their respective scores on naming tests (0/40, 7/40, 7/40 and 14/40). A diagnosis of deficit was only given for one of the patients (CM), though, and so I won't be including the results of the others' treatments in my analysis.

CM's speech, Marshall et al. (1998) said, largely consisted of neologistic jargon which was, for the most part, incomprehensible. Social phrases and the occasional relevant noun or noun phrase were produced, and he was better at reading aloud than he was repeating. CM showed an awareness of his speech and language disorder and, recognising that his wife was struggling to understand him, was keen to receive therapy. Nevertheless, when it came to talking, CM seemed not to have good comprehension. Unsure whether he was making sense or not during conversational discourse, he often looked to his conversational partner for confirmation, but his poor comprehension was confirmed by his scores on a series of tasks (a minimal pair discrimination requiring picture selection task (37/40), an auditory lexical decision test (127/160), spoken and written word to picture matching tests (39/40 and 32/40) and a synonym matching test (47/60: 27/30 for high image words and a lower 20/30 for low image words).

In order to determine the exact nature of CM's deficit, Marshall et al. (1998) asked him to name or repeat items and judge the correctness of his responses by saying *yes* or *no* or by pointing at cards displaying either a tick or a cross. On experiment 1 (a naming experiment) CM was asked to name 40 pictures from the PALPA test and judge whether or not his responses were correct. CM often produced more than one response per picture, which is why there are 49 judgements included in the data. His total score of correct judgements was only 32/49 with 14 of those being a perfect score on his correct responses, but of the 35 incorrect responses that CM had to judge, he incorrectly did so for half (scoring 6/13 on his verbal paraphasias, 11/20 on his neologisms, 1/1 on his phonological errors and 0/1 on one that was labelled by Marshall et al. (1998) as 'other').

It was found that CM was better at judging repetition than naming, however, (59/66 (26/26 for correct responses, 3/3 for verbal paraphasias, 19/20 for neologisms and 11/17 for phonological errors)), which indicated to Marshall et al. (1998) that CM's problems more than likely had to do with access to semantics or from semantics to the phonological output lexicon than they did something post-lexical (for repetition doesn't make use of the two stages of word production that naming does: the need to access semantics or the need to access phonology from semantics).

A picture selection task (experiment 3) was used to identify which impaired naming process it was that impacted CM's monitoring. In the task, CM was shown two semantically related pictures and heard one of their names, and was required to point to the picture that matched the name heard. Marshall et al. (1998) hypothesised that if it was the impaired retrieval of semantic representations that disrupted CM's monitoring, a decline in awareness should be observed, but if, on the other hand, it was the accessing of phonology from semantics that was impaired, monitoring should be preserved, actually.

40 items in the task stimulated 48 responses, and of the whole 48, CM made only one error when judging his picture selection responses. With regards to the responses themselves, most of his failures occurred with phonological errors as opposed to semantic ones. These results, coupled with those of experiment 3 brought about a diagnosis of an anomia that was due to damage to access to the phonological output lexicon for CM, which Marshall et al. (1998) tried to treat with semantic therapy tasks in experiments 5 and 6.

In experiment 5, CM was shown two semantically related pictures (for example of a hammer and a nail) and asked to point to one of the pictures upon hearing its spoken name before trying to name the other (i.e. pointing to the picture of the hammer when hearing the word *hammer* before trying to name the picture of the nail). It was expected that this task might assist naming by priming the relevant semantic field and increasing activation to phonology from the semantic lexicon, but results showed that this wasn't the case (see his pre-therapy scores out of 40 compared with his post-therapy scores out of 40 in Table 5.4):

Responses	**Naming**	
	Unprimed	***Primed***
Correct	6/40	6/40
Semantic errors	3/40	5/40
Phonological errors	6/40	6/40
Verbal paraphasias	12/40	10/40
Neologisms	12/40	11/40
No response	1/40	2/40

Table 5.4 Unprimed and primed naming in CM (adapted from Marshall et al. 1998: 99)

In experiment 6, CM was required to associate the concepts of target pictures with those of semantically related written words before selecting the name of the picture from four options (the target itself and three semantic distractors). Six one hour therapy sessions were provided, and the efficacy of it was evaluated by asking CM to name the same 40 pictures before and after therapy. An untreated control group of pictures (which were matched for frequency) was tested in the same way, but therapy was found to have no effect on naming (in fact, CM's performance at naming both sets of words declined over the therapy period as the results in Table 5.5 show):

Responses	**Controls**		**Treated items**	
	Pre-therapy	***Post-therapy***	***Pre-therapy***	***Post-therapy***
Correct	14/40	7/40	13/40	11/40
Phonological error	8/40	6/40	6/40	7/40
Semantic error	8/40	8/40	6/40	3/40
Verbal paraphasia	4/40	6/40	2/40	7/40
Neologism	6/40	13/40	13/40	12/40

Table 5.5 Pre- and post-therapy naming of treated and control items (adapted from Marshall et al. 1998: 100)

In short, then, neither of the semantic therapies seemed to be effective at treating CM's phonological anomia. This is, of course, of interest to this thesis, because it's what a massively modular model of phonology would predict.

4.2.1.1.3 Nettleton and Lesser (1991)

In Nettleton and Lesser's (1991) study of anomia therapy, six aphasic patients with naming difficulties were selected according to the following criteria (Nettleton and Lesser 1991: 142):

(a) diagnosed as aphasic on the Boston Diagnostic Aphasia Examination (BDAE) (Goodglass and Kaplan 1983);
(b) at least six months after a single cerebrovascular accident, i.e. well past the period of substantial spontaneous recovery;
(c) with good hearing and visual acuity;
(d) without dysarthric difficulties;
(e) within the age range 50-70 years;
(f) with a naming difficulty, as assessed on the Boston Naming Test (BNT) (Goodglass and Kaplan 1983).

Two subjects were diagnosed as having problems with the semantic system, two were diagnosed as having problems relating to the phonological lexicon and two with problems at the level of phoneme assembly. After Lesser (1989), the criteria used to distinguish the three types of naming disorder were as follows:

(1) *For a naming disorder relating to the semantic system* (Nickels et al. 1991: 142-143):

  i. Performance on the auditory word-picture matching test of semantic ability from Psycholinguistic Assessments of Language Processing in Aphasia (PALPA) (Kay *et al.* 1991) fell below 39/40, i.e. the cut-off level for normal performance. […]
  ii. The majority of errors on the BNT consisted of semantic paraphasias.
  iii. When cued by a phonemic cue for a semantic associate of a target word, the subject generally produced the cued associate and accepted it as correct for the target.

(2) *For a naming disorder relating to the phonological output lexicon* (Nickels et al. 1991: 143):

  i. The score in the auditory word-picture matching subtest from PALPA was at least 39/40.
  ii. The majority of errors on the BNT consisted of anomic circumlocutions. […]
  iii. The percentile for the repetition subtests in the BDAE was at or above the percentile for the auditory and comprehension subtests.

(3) *For a naming disorder relating to the phonological output buffer* (Nickels et al. 1991: 143):

  i. The score on the auditory word-picture matching subtest from PALPA was at least 39/40.
  ii. The majority of errors on the BNT were phonemic paraphasias […].
  iii. Repetition percentile on the BDAE was below the percentile for auditory comprehension.

Two subjects were studied in each of the three categories. Two men, PD and FF, met the criteria for a naming disorder relating to the semantic system and two women, DF and MC, met the criteria for a naming disorder relating the phonological output lexicon. For a naming disorder relating the phonological output buffer another two women, MH and NC, met the criteria, but since the latter two patients' problems had to do with assembly and not access, I shall be excluding them from my analysis, and so no more will be said about them here.

PD was a 55 year old car park attendant with fluent aphasia following a cerebral infarct six months prior in the left parietal area. FF was a 68 year old former labourer who had a fluent aphasia that was marked by empty speech and poor auditory comprehension. DF was a 63 year old housewife who'd been left with a moderate aphasia following a stroke the year before and had relatively good social communication skills and MC a 57 year old housewife whose eight year ago stroke due to an embolism following mitral stenosis left her with non-fluent, agrammatic and dysprosodic speech as can be seen in the patient profiles in Table 5.6:

Subjects	
Naming disorder related to the semantic system	
PD	Male, aged 55, car park attendant Six months post stroke, with fluent aphasia BDAE comprehension 59% PALPA comprehension 75% (nine close semantic errors) BNT 20% (+17% with a semantic cue, +10% with a phonemic cue) On PTM pictures accepted most cued semantic associates as correct
FF	Male, aged 68, retired labourer Three years post stroke, with fluent aphasia, hemiplegia and hemianopia BDAE comprehension 15% PALPA comprehension 62% (12 close semantic errors) BNT 12% (+7% with a semantic cue, +0% with phonemic cue) On PTM pictures accepted all cued semantic associates as correct
Naming disorder related to the phonological lexicon	
DF	Female, aged 63, housewife One year post stroke, with severe anomic aphasia BDAE comprehension 90% PALPA comprehension 97% BNT 10% (with +0% with a semantic cue, +0% with a phonemic cue)
MC	Female, aged 57, housewife Eight years post stroke, with non-fluent agrammatic aphasia BDAE comprehension 50% PALPA comprehension 97% BNT 19% (+0% with a semantic cue, +25% with a phonemic cue)

Table 5.6 Subjects (adapted from Nettleton and Lesser 1991: 145)

The materials to be used in therapy were taken from the Cambridge pictures (Howard et al. 1985) (which are 300 drawings of common objects). These pictures were given the name Set A, to distinguish them from the subsets AT, AU and AUS. Each subject was asked to name the 300 pictures in Set A, and from the items they were unable to name, a random selection of 100 was made (meaning that each subjects' subsets were different). For each subject's 100 drawing subset, the 100 items were randomly split down the middle to make a further two sets of 50, a set to be treated (AT) and a set to remain untreated (AU). The remaining 200 items from Set A for each patient acted as a control group, and this was given the label AUS (A

UnSeen), so Set A (300) comprised three subsets of 50, 50 and 200, AT, AU and AUS, respectively.

The next eight weeks constituted the therapy phase; each subject attended clinic for an hour of therapy twice a week during this period, whereby either semantic naming therapy or phonological therapy was given. Each subject experienced only one type of therapy though, with the two patients diagnosed with semantic anomia (PD and FF) receiving model appropriate semantic therapy and the two patients diagnosed with phonological anomia (DF and MC) model appropriate phonological therapy. Patients' abilities to name the items in Set A post-therapy were compared with their ability to name them pre-therapy to measure their effects. The semantic and phonological therapies were found to largely work, with PD seeing a significant improvement at naming AT (the treated items) after the therapy phase $p < 0.05$ ($x^2 = 4.4.840$) but not the untreated items or the unseen items (AU or AUS).

DF scored significantly better on AT after the therapy phase than she did before it ($x^2 = 5.121$; $p = 0.025$) but not AU or AUS too, and just like the others MC saw significant improvement in her AT scores post-therapy ($x^2 = 6.428$; $p = 0.01$) but no significant changes in her AU or AUS ones. When it came to FF's scores, however, although he saw some improvement in naming the AT items post-therapy (going from 2 pre-therapy to 6 post-), this result was not statistically significant. It can be said, then, that whilst semantic therapy facilitated word retrieval in one patient with semantic anomia (PD) which is consistent with the thesis that semantics is a domain specific module of the mind, it did not do the same for the other (FF). Phonological therapy, meanwhile, saw naming improve in both patients with phonological anomia (DF and MC). This, of course, is what one would expect if phonology were domain specific and therefore massively modular.

4.2.1.2 Semantic and phonological tasks

4.2.1.2.1 Best et al. (1997)

In 1997 Best et al. reported the case of a man with dysphagia following a stroke. He presented with severely impaired word finding abilities, and so they intervened with a number of treatments, including a case study that compared the effects of lexical (both semantic and phonological treatment) with phonological treatment and a case study that looked at whether naming could be treated with a phonological cueing aid (see Bruce and Howard 1987). A set of pilot treatments were conducted prior to the two treatment studies, but since the methods of these weren't reported in any great depth, I've made the decision to exclude them from my analysis.

The patient, JOW, was 62 years old at the time of the study. He was a former entrepreneur, and had suffered a left hemisphere stroke in the September of 1990 (though there was no scan information to specify the exact site of his lesion). JOW had had speech and language therapy from one year post-trauma (the September of 91), when he also began to attend a local stroke support group for people with dysphagia. Testing on the Western Aphasia Battery classified JOW as an anomic aphasic, with a quotient of 69.

JOW performed within normal limits on a number of input processing tasks such as auditory discrimination and auditory lexical decision. Semantic processing of concrete items was also good (JOW scored 95% on both the auditory and written versions of word to picture matching tests with semantic and visual distractors (*n* = 40) (Kay et al. 1992)) and on the three picture version of the Pyramids and Palm Trees test (Howard and Patterson 1992) JOW's score was within normal limits.

When asked by Best et al. (1997) to describe the goings on in a picture, JOW said the following (where the words in the parentheses represent the intended targets determined on the basis of pointing, and the words in the square parentheses the therapists' prompt question):

> *'A tree, a plag, am er kite there's bun and a girl and a boy and what do you call it? A bicycle, no sorry what's it called? (dog) A garage and um a bais oh (radio) no not a bicycle. [What are they doing?] They're sat on a bike and pouring out something. He's reading a book. Is that enough?*

Word finding difficulties were clearly obvious in JOW's pre-treatment speech, then, but further testing confirmed this to be the case. On a picture naming task, it was evident JOW's performance was impaired; he was only able to name 28% of a set of 194 pictures correctly. Interestingly, he was better at naming items that were high in operativity than those that weren't (Rank sum test, $z = 2.00$, $P = 0.05$) and those that were rated high in imageability/concreteness than those that rated low on those measures (Rank sum test, $z = 2.29$, $p = 0.025$).

These findings, together with the fact that JOW's naming accuracy was not significantly affected by animacy, frequency, familiarity, age of acquisition, number of syllables or number of phonemes pointed Best et al. (1997) to a diagnosis of a breakdown in language processing at the lexical-semantic level. His error responses suggested the same thing, they said, with his main error types being of no response (34% of the time) or the provision of semantically related information (11% of it). Only 2% of JOW's responses were phonologically related to the target, which Best et al. (1997) took to be evidence against a phoneme assembly deficit. The fact that JOW's naming wasn't influenced by variables such as frequency or length led them to conclude that his breakdown couldn't be anything other than semantic, for those variables are generally held to reflect processing beyond that.

The effect of lexical therapy (both semantic and phonological) was considered first in study 1, but since this thesis is concerned with the effects of separate semantic therapies and phonological therapies only, I won't be including the lexical therapy part to study 1 in my analysis. The second part to study 1 was concerned with the effects of semantic therapy, however, which is something I definitely will be paying attention to.

Provided with a set of pictures (n = 36) that had a picture of the target on the face of the card and four semantically related names written on the reverse, JOW's task for the semantic therapy part to study 1 was to underline the target name and write it below the picture (e.g. when the target was *apple*, a picture of an apple appeared on the front of the picture card, and the words *apple*, *banana*, *orange* and *pear* on the back, requiring JOW to underline the written word *apple* on the back of the card and write it beneath the picture on the front).

After this, the effects of phonological treatment was considered in study 2. 50 items JOW found hard to name were selected for therapy, and matched in difficulty to 50 control items. The control items were used to detect whether any improvement found on treated items would generalise to untreated ones, and treatment took place for an hour a week, over five weeks using an electronic cueing aid that had nine keys labelled with letters that produced their corresponding phoneme (plus schwa) when pressed. JOW was instructed to use the aid every time he was unable to name a picture, and interestingly enough the phonological treatment with a cueing aid resulted in a highly significant improvement (from 8% targets being correctly named pre-therapy to 40% post- (z = 3.86, p = 0.001)) which generalised to naming of untreated items (z = 2.05, p < 0.05). Similarly significant results were seen with the semantic treatment with 45 targets being named correctly post-treatment from 21 pre-treatment evidencing improved naming.

This is interesting to a thesis of modularity, as if semantics was a domain specific massive module then we would only expect the semantic therapy to have had an effect on naming in semantic anomia and, of course, both the semantic and the phonological therapy were found to have had. This could count as evidence against the semantic processor's domain specificity, then, and by extension its massive modularity.

4.2.1.2.2 Hillis (1989)

In 1989, Hillis looked at the efficacy of treatment at reducing aphasic naming errors too. Two severely aphasic patients who made frequent semantic errors in both spoken and written naming were studied, but contrasting patterns of errors across a variety of tasks revealed that their naming errors arose from different deficits underlyingly. Whilst patient 1's errors were said to be caused by a single, semantic deficit, patient 2's errors were said to have two sources (an inability to retrieve the correct phonological representations from the lexicon for spoken naming and an inability to maintain the graphemic representation of the word when it was being written). Because patient 2's errors are caused by a mixed deficit, I shan't be including the results of their treatment in the analysis. The results of treatment for patient 1 will still be considered, however, and so we'll look at their case report in more detail in what follows.

Patient 1 was a 51 year old, college educated male who'd suffered a thromboembolic stroke three months prior to the investigation. A computerised tomography (CT) scan revealed that this was due to infarction in the left fronto-parietal area of his brain, and a right hemiplegia followed (which resolved over the course of the next two weeks to a mild paresis of the right arm only). Patient 1's ambulation improved to a normal gait but apraxia persisted, and by the time he'd reached seven weeks post-onset it was clear that his expression and comprehension of language was compromised. On the Minnesota Test for Differential Diagnosis of Aphasia (which tests comprehension), patient 1 was able to point to named objects with only 30% accuracy, his yes or no responses to acontextual questions were only at chance level of accuracy and his reading comprehension was severely impaired too. Most errors made on word to picture matching tasks were semantic confusions (e.g. *dog* for *cat*), and when tested on a total of 144 items with a series of tasks (spoken naming, written naming, reading aloud, writing to dictation, spoken word to picture matching and written word to picture matching) around 40% of his responses for each were errors (most of which were semantic as can be seen in Table 5.7):

Task	Error rate (%)	Semantic errors (%)
Spoken word to picture matching	42.4	40.3
Written word to picture matching	36.8	27.1
Spoken naming	44.4	40.9
Written naming	46.5	34.7
Writing to dictation	41.7	27.7
Reading aloud	41.7	36.1

Table 5.7 Patient 1's performance on lexical tasks (where the stimuli for each task was 144 names for or pictures of concepts)

Patient 1's homogenous pattern of semantic errors across all six tasks, Hillis (1989) said, indicated a unitary impairment in semantics – an impairment which went on to be treated with phonological therapy.

A set of 50 black and white line drawings of objects and events that were familiar to the patient served as stimuli in the phonological therapy. Patient 1 was asked to name all 50 stimuli in three consecutive sessions, and the ten he was unable to name each time were selected for treatment. The remaining 40 were used as controls, to measure the efficacy of the treatment. Change in performance on naming across the spoken and written modalities was evaluated. Patient 1 was presented with a random picture from the stimulus set (of ten items) followed by the following hierarchy of cues (adapted from Hillis 1989: 634) until the target was written accurately:

6. Pictured stimulus (independent written name).
5. Scrambled anagram and two distractors.
4. Scrambled anagram without distractors.
3. Initial letter cue.
2. Spoken name (correct writing to dictation).
1. Written name presented briefly (correct delayed reproduction).

The subject was then asked to say the word, but no feedback was given about whether they had done either of those things rightly or wrongly. The percentages of accurately written and spoken names produced in response to the picture stimuli were recorded, and results showed that not only did both oral and written naming improve significantly with phonological treatment on trained words, but that this generalised to untrained words as well. This, of course, isn't consistent with what a massively modular model of semantics would predict. If semantics were massively modular, then it would have to be domain specific, and, as I've said before, if semantics were domain specific, then it wouldn't be able to operate on input irrelevant to its function as it does here with phonological information.

4.2.1.2.3 Hillis and Caramazza (1994)

Hillis and Caramazza, meanwhile, present a series of single subject studies of brain-damaged patients who each make semantic errors in naming. A cognitive analysis of each case's performance provided evidence to propose relatively selective impairment to components of lexical processing, but whilst Hillis and Caramazza diagnosed cases (KE and JJ) as having one primary deficit (a semantic impairment) they diagnosed two others (HW and SJD) as having word retrieval impairments that were due to damage to two levels of lexical processing (at the level of one modality specific output lexicon (the phonological for HW and the orthographic for SJD) and to sublexical procedures for converting sound to print and print to sound). PM, on the other hand, who was their fifth patient, was diagnosed with substantial impairments in accessing information from both the phonological and orthographic lexicons and in using orthography to phonology and phonology to orthography procedures.

Neither the results from HW and SJD's cases nor the results from PM's will be included in my own data analysis. Attention will be paid to KE and JJ, however, and so I describe their diagnoses and treatments here. KE was a 51 year old, well educated (to college level) male who suffered a left fronto-parietal infarct six months prior to the study. His identical pattern of performance across lexical tasks was interpreted as evidence for selective impairment to the semantic system. A set of 144 items were presented to him for spoken naming, written naming, writing to dictation, reading aloud, spoken word to picture matching and written word to picture matching and of all the incorrect responses that KE made, nearly all were semantic.

A set of 50 drawings of familiar objects (to test nouns) and 10 drawings of familiar actions (to test verbs) was presented for consecutive oral and written naming without feedback. When the patient was unable to name the item, a hierarchy of phonological cues were provided. Following a correct response, each of the cues were provided again but in reverse order, until the patient was able to produce the name for the item independently when looking at the picture, and results showed that treatment with the phonological cueing strategy resulted in improved naming of both trained and untrained items for KE.

JJ, on the other hand, was a 67 year old ex-corporate executive who sustained a left temporal and basal ganglia stroke 9 months before he began treatment with Hillis and Caramazza (1994). Like KE, JJ had a college education (2 years) and also like KE, he showed selective damage to the semantic component of lexical processing. On the lexical tasks that were presented to KE, JJ also showed frequent semantic errors, though whilst KE showed semantic errors across all categories of words, KE showed semantic errors across all categories of words but animals. According to Hillis and Caramazza (1994) this could be explained by assuming selective damage to the semantic system that selectively spared semantic representations. Why or how that would occur they didn't know, but they did stress that unlike PM, his ability to use sublexical sound to letter and letter to sound mechanisms was spared. The effects on spoken naming performance on two different types of treatment were compared, a semantic word to picture matching treatment and a phonological cued reading treatment. Results of the two treatment approaches showed that whilst the semantic treatment facilitated JJ's ability to name items (of the 20 pictures that were presented to JJ for naming an average of one or two (and therefore 10-20% of) items he was unable to name before treatment he was able to following it) a change in picture naming performance wasn't seen for JJ with the phonological treatment.

The fact that JJ has semantic anomia which was treated by semantic therapy but not phonological therapy suggests, to my mind, that semantics is indeed domain specific (and so massively modular). KE's result, on the other hand, was inconsistent with a thesis of massive modularity, for word retrieval in her semantic anomia was facilitated by phonological therapy. This suggests that the information semantics operated on, in this case, wasn't relevant to its function.

4.2.1.2.4 Pederson et al. (2001)

Pederson et al. (2001) investigated the effects of computerised rehabilitation of anomia in aphasia with three single case studies. The therapy was carried out in the patients' own homes without the presence of a speech and language therapist, and it wasn't overseen by any other clinician, family member or friend either. Just like I excluded Marshall et al. (1990)'s unsupervised group study from my analysis, then, so too will I exclude this one. I also exclude this one because the patients were selected from the neurological departments of the Bispebjerg and Hvidovre hospitals in Copenhagen, Denmark and through the Danish Stroke and Aphasia Association, meaning that the Western Aphasia Battery (WAB) (Kertesz 1982) and Psycholinguistic Assessment of Language Processing Abilities (PALPA) (Kay et al. 1992) tests used in the assessments of their anomias had to be translated from English and adapted for Danish for use in this study.

Whilst I've listed Pederson et al. (2001) as one of the studies excluded in Table 5.2, given that my reason for doing so didn't have to do with the patients' impairments and treatments being mixed or the patients' impairments and treatment outcomes being unknown (as the rest of them do) I decided to elaborate here. No more will be said about Pederson et al. (2001) now, and so I'll turn my attention to Raymer and Ellsworth (2002).

4.2.1.2.5 Raymer and Ellsworth (2002)

Raymer and Ellsworth (2002) compared the effects of semantic and phonological therapies on verb retrieval in a patient (WR) with semantic verb retrieval impairments. Accuracy of picture naming was examined on trained and untrained verbs before and after therapy, and results showed that the treatments significantly improved naming (with trained verbs showing a significant improvement and untrained verbs no improvement). The effects of a rehearsal type of treatment were also studied, but since this involved repetition of targets only it had to with post-lexical processing (i.e. phoneme assembly) it doesn't meet the criteria to be included in my analysis.

The star of the single subject investigation WR was a 54 year old woman who discontinued schooling aged 13 to become a hairdresser. A left hemisphere CVA left her with a right hemiparesis and non-fluent aphasia that was marked by word retrieval difficulties. A CT scan taken at the time of the CVA revealed a left dorsolateral frontal lesion which encompassed a portion of Broca's area (as well as the anterior insular region and subcortical white matter), so it made sense that her aphasia was one that caused problems with production. Standardised testing with the WAB (Kertesz 1982) 3 months post-stroke led to a more formal diagnosis of transcortical mortal aphasia, and results of the Bosting Naming Test (owing to Kaplan et al. 1983) and Action Naming Test (Obler and Albert 1986), respectively, indicated that her naming problems had more to do with verbs than they did nouns (naming for nouns was affected minimally for her level of education (44/60 correct) whereas picture naming for verbs was quite a way reduced (37/62 correct)).

To evaluate her word retrieval difficulties further, WR was tested on a noun and verb battery made up of 30 verbs and two sets of 30 nouns. The battery included four tasks, the first two owing to Zingeser and Berndt (1990) and the other two to Williamson et al. (1995) which Raymer and Ellsworth (2002: 1034) described as follows:

1. Picture naming: WR viewed each of the 90 black and white line drawings of the noun and verb stimuli and provided a one word label for the object or action depicted in each picture.
2. Sentence completion: The examiner read aloud a phrase as WR also read along. She then completed each of the 90 sentences with an appropriate noun or verb.
3. Crossmodal picture-to-word matching: WR pointed to one of three words that corresponded to the target picture (e.g., write). In the related condition, both distractor words were semantic coordinates of the 90 targets (e.g., read, draw), and in the unrelated condition, both distractor words were unrelated to the 90 targets (e.g., ask, take).
4. Picture-picture associate matching: WR pointed to one of two pictures that was most closely related to a target picture (e.g., baking a pie). The distractor pictures were semantic coordinates of the 90 correct pictures in one condition (e.g., correct—grilling a steak; distractor—peeling a potato) and were unrelated in a second condition (e.g., correct—peeling a potato; distractor—leaking water). For half of the picture associate pairs, in the unrelated condition, the distractor picture from the unrelated condition became the correct answer when a completely unrelated foil picture replaced the other associate picture.

WR's results on the battery are displayed in Table 5.8:

Task	**Nouns**	**Verbs**
Spoken picture naming	52/60	22/30
Sentence completion	52/60	24/30
Picture-picture associate match		
Related	45/60	21/30
Unrelated	57/60	39/30
Crossmodal picture-word match		
Related	56/60	28/30
Unrelated	60/60	30/30

Table 5.8 Number of items correct for WR on the Florida Noun/Verb Battery, a set of lexical tasks for noun and verb comprehension and retrieval (adapted from Raymer and Ellsworth 2002: 1035)

To determine whether her naming impairments related to the semantic or phonological level of language processing WR's performance on picture matching tasks (which rely on semantic processing for correct responses) was examined, for impairment on them would suggest that semantic dysfunction contributed to her naming difficulties. It was found that her performance was slightly below levels observed with normative subjects. This led Raymer and Ellsworth (2002) to conclude that WR's word retrieval deficits were at the semantic level of language processing, and after that, they began her treatment.

WR was asked to name 222 black and white line drawings of various actions. Responses that were the target or a synonym for the target were accepted as correct. Analysis of WR's errors in two baseline tests indicated that WR made 44-45% errors in verb naming and on the basis of this performance, 60 verbs that WR had consistent difficulty in naming were selected as experimental stimuli. The 60 items were split into three groups: 20 for use with phonological treatment, 20 for use with semantic treatment and 20 for a control set (which later became the rehearsal treatment set, though this has no place in this thesis).

WR was given two to three 1 hour sessions per week over the course of four months. In treatment phase 1, WR underwent phonological treatment for the first set of verbs and after a week-long break underwent semantic treatment with the second set. In both the phonological and the semantic treatment, WR was presented with a picture to name aloud and given feedback with regards to her response accuracy. When her response was inaccurate, she was asked two questions designed to help her develop a word retrieval strategy consistent with that of a normative subject's. In phonological treatment, the questions encouraged WR to think about the sounds of the target words (e.g. for the word *pay* she was asked an initial phoneme question ‘Does *pay* start with [p]?’ and a rhyming word question ‘Does *pay* sound like *way*?’) and in semantic treatment, the questions encouraged her to think about the meanings of them (e.g. for *bake* the coordinate verb question ‘Is this similar to *grilling*?’ was asked, as was the associated noun question ‘Does this have to do with *pie*?’).

Results showed that when phonological treatment was applied to the first set, MR’s verb naming performance rose from an average of 8.33% correct in the baseline sessions to a 90% correct criterion level, which was found to be statistically significant on a McNemar’s test ($x^2 = 14.06$, $p < 0.001$). This is clearly inconsistent with what a domain specific and therefore massively modular model of phonology would predict. WR also saw her verb naming performance improve with semantic treatment, though: when semantic treatment was applied to the second set an average of 17.5% correct across the baseline sessions rose to a 100% criterion level thereafter and a significant McNemar’s test result was found for this too: ($x^2 = 13.07$, $p < 0.001$).

4.2.1.3 Primarily phonological tasks

4.2.1.3.1 Fink et al. (2002)

Now, although Fink et al.’s (2002) study is to be excluded from my analysis it’s worth mentioning here because my reasoning for excluding it is more complicated than that of other studies. Fink et al. (2002) assessed the benefits of a computer delivered, hierarchical phonological cueing protocol under two conditions of instruction with six different patients: (1) with full clinician guidance for GM, AS, and BM, and (2) with full independence for EL, EG and RH. Since no speech and language therapist was present during the second condition of instruction with EL, EG and RH, I’ve decided to exclude the results of it from my analysis, but I’ve also decided to exclude the results of those that were clinically guided (GM, AS and BM) too, but for reasons having to do with their deficits.

Testing revealed that GM’s primary locus of impairment was at the level of phoneme assembly, which meant that he had to be excluded from the analysis because his impairment was neither semantic nor phonological. While AS and BM’s primary deficits were at the access to phonological output lexicon level of language processing, however, both of them had some involvement of the semantic system meaning that their impairments were mixed and they couldn’t be studied.

This meant that for a number of different reasons, not one of the six patients studied in Fink et al. (2002) could be included in my analysis: GM's impairment was post-lexical, AS and BM's impairments were mixed, and when it came to EL, EG and RH regardless of their level of impairment (which were mixed as well, actually (impaired access to the phonological output lexicon and phonological encoding (that is, phoneme assembly)), a deficit in phoneme assembly and a deficit in accessing phonology from semantics, respectively), not one of them could be considered because of the study design that their therapy revolved around (they were part of a self-guided group).

4.2.1.3.2 Hickin et al. (2002a)

Hickin et al.'s (2002a) study set out to investigate whether the use of phonological and orthographic cues in the treatment of word finding difficulties was effective or not. The study used a case series design, and the participants involved were 8 people with acquired aphasia who were all at least a year post-onset, had a single left CVA and had word finding difficulties as a significant symptom of their aphasia. Detailed assessment of each participant was carried out to diagnose the type of aphasia they had (see Table 5.9), but a diagnosis of the impairment underlying the word retrieval difficulty was only made for 4 of the 6 (PH, DC and NK (who were all diagnosed as having difficulties with mapping between semantics and phonology, that is, a deficit with access to the phonological output lexicon) and SC (who was diagnosed with two sources of word retrieval difficulties, problems at the semantic level of language processing and problems with phoneme assembly)).

Participant	**Years post-onset**	**Age**	**Aphasia type**
HM	6	45	Broca's
PH	3	77	Anomic
SC	5	65	Mixed/Wernicke's
DC	5	70	Anomic
OL	2	65	Anomic
NK	3	52	Anomic
IK	3	68	Broca's
KR	8	38	Broca's

Table 5.9 Background information for the participants included in the study, including aphasia type as assessed by the Comprehensive Aphasia Test (Swinburn et al. 2004) (adapted from Hickin et al. 2002a: 986)

Since SC’s impairment diagnosis was mixed and a diagnosis of impairment wasn’t given for GM, OL, IK or KR, I shan’t be including any of them in my data analysis. I’ll be concerning myself with the results of PH, DC and NK only, who, interestingly, were said to be the three who benefited most from therapy.

The study consisted of three phases of around eight weeks in length. First, there was an assessment phase, and after that, there was a treatment phase (which focused on improving word finding by using phonological cues in a picture naming task). Progress was monitored over a number of assessments, with one at the beginning and one at the end of the study.

As part of phase 1, naming was assessed using a set of 200, black and white line drawings. Naming was tested at the beginning and the end of the assessment phase to establish baseline naming performance. Written naming was assessed using a subset of items (40 in total) taken from the set and comprehension, auditory discrimination, short-term memory, reading and repetition were assessed using the tests detailed in Table 5.10:

Test	**n**	**Participants**		
		PH	***DC***	***NK***
Picture naming tests 1 and 2: mean	200	0.36	0.67	0.56
Semantic tests				
CAT spoken word to picture matching test	30	0.93	1.00	0.93
CAT written word to picture matching test	30	0.97	0.97	0.97
Pyramids and Palm Trees test (three picture version)	52	0.90	0.92	0.87
Picture naming: semantic errors as a proportion of total errors		0.25	0.50	0.33
Phonological tests				
ADA auditory discrimination test	40	0.68	0.85	0.90
Short-term memory test: phoneme span		2.50	2.30	2.70
Repetition of words	152	0.97	0.95	0.99
Repetition of nonwords	26	0.58	0.50	0.81
Repetition of nonwords: initial phoneme correct	26	0.88	0.85	0.96
Picture naming: phonological errors as a proportion of total errors		0.05	0.11	0.00
Reading real words	152	0.97	0.97	0.92
Reading nonwords	26	0.35	0.15	0.08
Reading nonwords: initial phoneme correct	26	0.85	0.92	0.92

Table 5.10 Participants’ performances on a number of background assessments (adapted from Hickin et al. 2002a: 987)

During the treatment phase, 200 items were divided into two sets, one a treatment set and one a control one. The treated set was divided again into two sets of 50 (one to be treated with phonological cues and the other orthographic), and assessments were made before and after treatment to test the treatments' efficacies.

As I said earlier, treatment took place for around 8 weeks, with each session lasting around one to one and a half hours. Items were presented for naming, and if they were unsuccessfully retrieved so too were the phonological or orthographic cues. In the phonological cueing therapy, the first phoneme (plus schwa) and an unrelated distractor were presented first and if the patient was still unable to retrieve the target the phonological information provided in cues increased to the first syllable and then the whole word. In the orthographic cueing therapy the same process was followed, except instead of the cues being spoken by the therapist they were written down by them.

As a result of the treatment, eight participants (including PH, DC and NK) showed a significant improvement in naming the treated items. Only one showed significant improvement on the untreated items (DC), but all three showed overall more significant improvement on the treated items than the untreated items, which is consistent with a massively modular model of phonology that requires its domain specificity. None of the participants showed a significant advantage of one cue over the other.

4.2.1.3.3 Hickin et al. (2002b)

Hickin et al. (2002b) investigated the effects of phonological treatment on word finding in aphasia too. They present the results of therapy from two patients: HM and PH. HM is a married male with two children in his forties who was working as a carpenter/cabinet maker when he suffered a single, left hemisphere stroke 5 years prior to his involvement in the study. Following his stroke HM and his wife separated, meaning he gets to see his children less often than he would have liked to. Living alone now in a flat, he attends a day care centre three times a week, where he's busy building a model railway. HM's other interests include steam trains and rock music, and he is said to have an expressive aphasia (that is, an aphasia that causes problems with production).

PH, meanwhile, is a seventy something year old female, who has lived in London all her life but now lives there in sheltered accommodation. PH is very sociable, engaging in bingo, lunches and day trips out to the south coast. PH has a large and supportive family that live nearby, with whom she has regular contact. Like HM, PH is said to have suffered a left hemisphere stroke three years before being involved in the study. PH's diagnosis is of aphasia. Unlike HM, her expressive speech is fluent, and she frequently experiences word finding problems.

Table 5.11 shows the results of the assessments that were carried out to diagnose the specific deficits in language processing that led to HM and PH's aphasias.

Deficit	**HM**	**PH**
1. Naming 200 pictures (number of correctly named items)	44	36
2. CAT spoken word to picture matching (*n* = 30)	100	93
3. CAT written word to picture matching (*n* = 30)	87	97
4. Pyramids and Palm Trees (*n* = 52)	94	90
5. Percentage of semantic naming errors (out of total naming errors)	52	25
6. Imageability effect in naming	N	Y
7. ADA spoken discrimination (*n* = 40)	82	68
8. Short-term memory phonemes	1.4	2.5
9. Repetition of words (*n* = 152)	73	97
10. Repetition: non-words (*n* = 26)	31	58
11. Repetition non-words: 1st phoneme correct (*n* = 26)	54	88
12. Percentage of phonemic naming errors (out of total naming errors)	20	5
13. Length effect in naming	Y	N
14. Reading words (*n* = 152)	70	97
15. Reading non-words (*n* = 26)	0	35
16. Reading non-words: 1st phoneme correct	38	85
17. Written naming (*n* = 25 for HM and 40 for PH)	16	38

Table 5.11 Language assessment in percentages prior to therapies (from Hickin et al. 2002b: 72)

Hickin et al. (2002b) concluded of the results that whilst HM and PH both made semantic errors in naming, neither of them had semantic processing deficits. HM's difficulties with naming and repetition, coupled with the fact that his naming was affected by length led Hickin et al. (2002b) to conclude that his deficit could either have been in phonology or in access to the output lexicon, and so I will eliminate him from my own analysis because his exact diagnosis is unclear. PH, meanwhile, was said to have a deficit in access to the phonological output lexicon. Her superior performance to HM's in reading and repetition suggested to Hickin et al. (2002b) that her output phonology was intact but not easily accessed, and this, together with the fact that her semantic processing was relatively intact as well, led to a diagnosis of a phonological access deficit in the mapping of semantics to phonology.

Phase 1 of treatment (which was phonological) focused on picture naming involving a choice of cue. In the second phase of treatment, Hickin et al. (2002b) moved away from picture naming, however, instead encouraging the use of targeted words in interaction related to PH's interests. During phase 1 of treatment, 50 words were treated with phonological cues. When PH was unable to name a picture (or made an error in picture naming), she was exposed to the first sound of the word along with that one or more distractors. Targets and distractors were matched for syllable structure but were different in terms of their first sounds, as well as not being semantically related to the target, e.g. *banana* and *piano* for *tornado.* In phase 2, on the other hand, the emphasis was on the use of target words in everyday speech. Tasks included naming to definition, naming in a pseudo-realistic speech situation, making lists, reminiscing, telling anecdotes and conversational discourse around certain subjects. In all of the sessions, PH had access to pictures and written semantic cues if they needed them. It's clear, them, that phase 2 of PH's treatment, although semantically targeted, would involve some implicit phonological activation. For this reason, I have decided to define that phase of treatment as being mixed (semantic-phonological) and have struck it from my analysis. Results showed that PH made gains in picture naming in phase 1 (the phonological task).

4.2.1.3.4 Jokel and Rochon (1998)

In Jokel and Rochon (1998), finally, treatment for an aphasic patient PD with severe naming difficulties is described. The 90 year old woman (who had suffered a left frontal haemorrhage in the July of 95)'s initial diagnosis was of global aphasia with a probable phonological output impairment, but Jokel and Rochon (1998) went on to conduct a number of assessments on to confirm whether or not this was the case.

Pictureable nouns were administered over four different tasks (confrontation naming, spoken word to picture matching, repetition and reading aloud). In the confrontation naming tasks PD's ability to name 134 items was tested three times in three separate sessions to establish a baseline naming performance for her (this was only 10% correct). Another set of 120 items were used in the word to picture matching, repetition and reading aloud tasks, and PD achieved 100% in the naming of these items. Because of the pervasive word retrieval deficit and having demonstrated intact comprehension, repetition and reading aloud performance for stimuli she was unable to retrieve in confrontation naming, it was hypothesised that she had an impaired phonological output lexicon.

Three different types of phonological treatment were provided: repetition, reading and sentence completion. The 120 (above) words were divided into four separate sets, with 30 in each of the treatment conditions (e.g. repetition (30), reading (30) and sentence completion (30)) and 30 acting as a control group. Treatment on each set was given for five days, with all three types of treatment being provided each day. Performance on a confrontation naming task which contained all 120 items was measured before and after each treatment condition, and it was found that confrontation naming pre- vs. post-repetition was 0/30 vs. 2/30, pre- vs. post-reading 4/30 vs. 12/30 and pre- vs. post-sentence completion 2/30 vs. 0/30. The only significant difference found was for that of the reading treatment (Fisher's Exact test p = 0.04), and when it came to the sentence completion treatment, the post-treatment score was lower than the pre-treatment, suggesting that treatment had the opposite effect of what was wanted.

This left a total of 10 case studies made up of 18 participants and 24 results that were relevant to this thesis:

Paper	Participant	Impairment	Treatment	Outcome (treated)
Primarily semantic tasks				
Marshall et al. (1990)	RS	Phonological	Semantic	Yes
	IS	Semantic	Semantic	Yes
	FW	Semantic	Semantic	No
Marshall et al. (1998)	CM	Phonological	Semantic	No
			Semantic	No
Nettleton and Lesser (1991)	PD	Semantic	Semantic	Yes
	FF	Semantic	Semantic	No
	DF	Phonological	Phonological	Yes
	MC	Phonological	Phonological	Yes
Semantic and phonological tasks				
Best et al. (1997)	JOW	Semantic	Semantic (Study 1: semantic therapy	Yes
			Phonological (Study 2: cueing aid therapy)	Yes
Hillis (1989)	Patient 1	Semantic	Phonological	Yes
Hillis and Caramazza (1994)	JJ	Semantic	Semantic	Yes
			Phonological	No
	KE	Semantic	Phonological	Yes
Raymer and Ellsworth (2002)	WR	Semantic	Phonological	Yes
			Semantic	Yes
Primarily phonological tasks				
Hickin et al. (2002a)	PH	Phonological	Phonological	Yes
	DC	Phonological	Phonological	Yes
	NK	Phonological	Phonological	Yes
Hickin et al. (2002b)	PH	Phonological	Phonological (phase 1)	Yes
Jokel and Rochon (1998)	PD	Phonological	Phonological	No
			Phonological	Yes
			Phonological	No

Table 5.12 A breakdown of the 10 single case studies of word retrieval relevant to this thesis

Some others had to be struck out too though, along the way. This was a decision that was made on statistical significance grounds, and so I go on to explain that in what follows in 4.2.2.

4.2.2 Outcome classification (treated vs. untreated)

Now, something needs to be said about the outcome column here. In this thesis, I chose to classify therapies as having treated anomias when the patients' naming was better on a set of items post-therapy than it was pre-, and the improvement was found to be statistically significant. When patients named a set of items better after they'd received therapy for their anomia than they had before but no statistical significance was found, then the classification of not-treated was given. Rather self explanatorily, though, if the number of correctly named items stayed the same before and after therapy or if fewer items were correctly named after it than there were before the patient received therapy for their anomia, the therapy was classified as having not treated the patient.

However, whilst some researchers calculated statistical significance in this way (i.e. through tests that made use of 2x2 contingency tables to see whether there existed a relationship between the independent variable (otherwise known as the cause, which in this case was treatment (pre- and post-)) and the dependent variable (otherwise known as the effect, which was of course, in this case was outcome (number of items named correctly and incorrectly)) such as the chi-squared test or Fisher's exact test, for example (Kim 2017), others were more interested in whether the effects observed could be generalised to control items (e.g. Marshall et al. 1990), and so didn't calculate statistical significance in the way that is of interest to this thesis. When this was the case (or when statistical significance wasn't calculated at all, for that matter), I made sure to extract the data myself from the studies (well, where I was able to at least), in order to calculate the statistical significance of the effects of treatment on outcome using the Fisher's exact test calculator within the SPSS statistical package for Mac.[9]

4.2.2.1 Marshall et al. (1990)

As I pointed out in section 4.2.1.1.1 Marshall et al. (1990) did indeed find their results for RS to be statistically significant, but the statistical significance reported had to do with whether his performance on treated items was better than that on untreated items. Now, whilst this was interesting from a qualitative perspective in that it looked like his naming improved with treatment, in order to make for a fair test I had to see whether this could be supported by statistics by running the numbers of items he did and didn't name pre-treatment and post-treatment through a Fisher's exact test in SPSS. This was easy enough to do as the authors

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[9] An explanation of why the Fisher's exact test was used is given in 4.2.3.1.

made it known to the reader that in the to-be-treated set of 25 items, only 8 were named correctly pre-treatment compared with 20/25 post-.

The effect observed was one that could be considered statistically significant; a *p* value of 0.0014 was produced (which is <.05 (the *p* value taken to be the level of statistical significance in the social sciences)) and, as such, the semantic therapy for phonological anomia, in this case, was classified as having treated it.

The results reported by Marshall et al. (1990) for patients IS and FW were again not really relevant to this study, and so I conducted Fisher's exact tests for those too to see whether there was a relationship between treatment and naming for them. Marshall et al. (1990) point out in their paper that IS' score on Group 1 items prior to therapy was 10/25 and after it 22/25, which gives us a significant result: $p = 0.0009$ and so the semantic therapy for IS' semantic anomia was classified treated too. It was said about FW meanwhile, that she scored 8/25 on a set of items pre-treatment and 11/25 on the same set after 3 weeks of treatment. This, as one would probably predict, produced a statistically insignificant result (of $p = 0.5607$), and so I classified the semantic therapy that was used to facilitate word retrieval in FW's anomia as untreated.

4.2.2.2 Marshall et al. (1998)

Marshall et al. (1998), on the other hand, didn't test for statistical significance – but then again, nor did they have to. CM, who had phonological anomia, received two types of semantic treatment. In one experiment (experiment 5) semantic treatment made no difference to naming (6/40 items were named correctly pre-treatment and the same number again, 6/40, post-treatment), and in the other (experiment 6) CM was actually worse at naming after treatment than they were before (13/40 and 11/40 items were correctly named pre- and post-treatment, respectively). This led me to classify neither the first nor the second treatment as having treated CM's phonological anomia without further tests for statistical significance, as can be seen in Table 5.12.

4.2.2.3 Nettleton and Lesser (1991)

Nettleton and Lesser (1991) conducted their tests of statistical significance in the same way that I would have. They compared the number of items that were named pre- and post-semantic treatment by each of the semantic subjects (PD and FF), and before and after phonological treatment by the patients with phonological anomia (DF and MC). As I said in section 4.2.1.1.3 results indicated that semantic treatment did indeed facilitate word retrieval in semantic anomia in PD's case (14% of the items were named correctly pre-treatment compared with 36% post-), and these results were found to be statistically significant by Nettleton and Lesser (1991) ($p < 0.05$ ($x^2 = 4.840$)) which led me to immediately classify semantic therapy as having treated semantic anomia in the case of PD.

For FF, though, whose anomia was also semantic, although some improvement was seen in naming following therapy (6% of items were named after compared with 2% before), the results did not reach significance. Thus semantic anomia was classified as having not treated semantic anomia in this case.

For DF, whose anomia was phonological, scores were significantly better after semantic therapy than they were before ($x^2 = 5121$; $p = 0.025$). Before therapy for DF, percentage success in naming was 20% and after therapy it was 46%, so a classification of treated was given.

Last but by no means least MC, who like DF was a phonological anomic who received phonological therapy, performed significantly better after it than she did before ($x^2 = 6.428$; $p = 0.01$). Their percentage success went from 20% to 50% before and after naming and so their therapy, like PDs and DFs, was classified as having treated anomia in this instance.

4.2.2.4 Best et al. (1997)

Now, Best et al.'s (1997) report required me to calculate statistical significance for their results too, because although for JOW, a patient with semantic anomia, they found phonological therapy to significantly facilitate word retrieval (taking him from an average of 8% of items named correctly before treatment to an average of 40% (without an aid) after it (Wilcoxon, $z = 3.86$, $p = 0.01$)), when referring to the before and after results of semantic therapy, they reported that semantic therapy significantly improved naming but did specify what p value led them to come to that conclusion.

Statistically significant results for phonological therapy's ($p = 0.0003$) and semantic therapy's ($p = 0.0178$) effects on naming were found through Fisher's exact tests (after the percentages were put back to raw token numbers, that is) by myself, however. And so I made sure to classify the two as having treated the patients' anomias.

4.2.2.5 Hillis (1989)

It was when I looked into the statistical significance of Hillis (1989) that I really ran into problems. Not only did Hillis not report raw token numbers for items named before and after treatment, the percentages provided were done so on two figures which were difficult to decipher. As such, I made the decision to exclude this study from my own, which is why it's missing from Table 5.12. I couldn't conclude anything about how many words had been retrieved at each point with any accuracy at all, and I didn't want to run the risk of any mistakes being made interpreting their results impacting my own.

4.2.2.6 Hillis and Caramazza (1994)

The semantic therapy that was used to facilitate word retrieval in JJ's semantic anomia was classified as having successfully treated it, whereas the phonological therapy that was used was not. As Hillis did in her 1989 study, though, Hillis and Caramazza (1994) depicted JJ and KE's performances pictorially. Percentages of correctly named nouns were plotted, but those percentages weren't (for the most part) provided (and nor were the raw token numbers that those percentages were figured out from).

Hillis and Caramazza (1994) did say, however, that phonological treatment did not facilitate word retrieval in JJ who had semantic anomia, and so I was automatically able to put that in the untreated pile. Semantic therapy, on the other hand, was said to improve naming in JJ though and a p value of <0.02 was reported, and so I classified that as having treated JJ's anomia. Improvement in naming in KE was seen by Hillis and Caramazza (1994), but since no statistical tests were conducted by them nor were percentages/numbers reported to the reader that I could have used to carry out tests of my own, effects for KE were excluded.

4.2.2.7 Raymer and Ellsworth (2002)

Raymer and Ellsworth's (2002) study didn't need any intervention. Only one participant was involved (MR), and both the therapies she had (semantic and phonological) were found to facilitate word retrieval in her semantic anomia. WR saw her naming performance significantly improve from 17.5% correct to 100% correct with phonological treatment ($p < 0.001$). Naming performance rose from an average of 8.33% of words named correctly pre-treatment to a 90% correct criterion level for semantic treatment. This was found by Raymer and Ellsworth (2002) to be statistically significant also (again, $p < 0.001$), and so the semantic therapy was classified by myself as having treated her semantic anomia too.

4.2.2.8 Hickin et al. (2002a)

Hickin et al. (2002a), who studied the effects of phonological treatment on phonological anomia in three participants (PH, DC and NK) reported statistically significant results for all. PH saw an improvement to 55% correctly named items of 200 post-treatment from an average of 35% pre-, DC an improvement from 72% of 200 items correctly to 89% post-treatment and NK from 57% to 71% post-treatment. All 3 results were said to have generated *p* values of <0.01, and so the therapies were all classified as having treated phonological anomia in this thesis.

4.2.2.9 Hickin et al. (2002b)

In Hickin et al. (2002b), PH's anomia was studied once more. They received phonological treatment. *n* equalled 50, and PH was able to name items an average of 34% of the time before phonological treatment compared with 60% of the items after it was administered. A Fisher's exact test result comparing the 17/50 words named before treatment and 33/50 words after treatment (calculated by myself) ruled the increase statistically significant with a *p* value of 0.0025.

4.2.2.10 Jokel and Rochon (1998)

The final report's results to be reviewed here, are that of Jokel and Rochon's (1998) who compared the efficacies of three phonological therapies at treating phonological anomia in a participant called PD. 120 words were divided into 4 sets, and 3 of those were treated with repetition, reading and sentence completion. Scores for naming were reported as 0/30 vs. 2/30 pre- vs. post-repetition, 4/30 vs. 12/30 pre- vs. post-reading and completion 2/30 vs. 0/30 pre- vs. post-sentence completion. With the third phonological treatment, then, a negative effect on naming was observed and so this was classified as not having treated PD's phonological anomia. With the first two (repetition and reading) positive effects were observed, but the reading treatment was the only one a significant difference in naming (Fisher's Exact test *p* = 0.04) was found for, and so whilst the second treatment was classified as having treated PD, like the third treatment, the first was not.

This left 9 case studies made up of 16 participants and 22 results to be quantitatively analysed:

Paper	Participant	Impairment	Treatment	Outcome (treated)
Primarily semantic tasks				
Marshall et al. (1990)	RS	Phonological	Semantic	Yes
	IS	Semantic	Semantic	Yes
	FW	Semantic	Semantic	No
Marshall et al. (1998)	CM	Phonological	Semantic	No
			Semantic	No
Nettleton and Lesser (1991)	PD	Semantic	Semantic	Yes
	FF	Semantic	Semantic	No
	DF	Phonological	Phonological	Yes
	MC	Phonological	Phonological	Yes
Semantic and phonological tasks				
Best et al. (1997)	JOW	Semantic	Semantic (Study 1: semantic therapy	Yes
			Phonological (Study 2: cueing aid therapy)	Yes
Hillis and Caramazza (1994)	JJ	Semantic	Semantic	Yes
			Phonological	No
Raymer and Ellsworth (2002)	WR	Semantic	Phonological	Yes
			Semantic	Yes
Primarily phonological tasks				
Hickin et al. (2002a)	PH	Phonological	Phonological	Yes
	DC	Phonological	Phonological	Yes
	NK	Phonological	Phonological	Yes
Hickin et al. (2002b)	PH	Phonological	Phonological (phase 1)	Yes
Jokel and Rochon (1998)	PD	Phonological	Phonological	No
			Phonological	Yes
			Phonological	No

Table 5.13 A breakdown of the 9 single case studies of word retrieval that I've included in my analysis

4.2.3 A quantitative analysis of the case studies

4.2.3.1 Statistical significance testing

We can, of course, determine whether relationships exist between independent variables (otherwise known as causes) and dependent variables (otherwise known as effects) by testing for statistical significance. In data sets such as mine, where the distribution of one categorical variable (in this case, treatment type) is to be compared with the distribution of another categorical variable (e.g. like mine, treatment outcome) either the chi-squared test or Fisher's exact test tend to be used (Kim 2017).

Now in order to determine whether a statistically significant relationship existed between treatment type (the independent variable) and treatment outcome (the dependent variable) in this thesis, a couple of tests (one looking at the relationship between treatment type and treatment outcome in patients with phonological anomia and another between treatment type and treatment outcome in patients with an anomia that was semantic) had to be conducted. The chi-squared test, however, should only be used in cases of large sample sizes, for it applies an approximation and mine (n=22) was definitely not that. Fisher's exact test, on the other hand, runs a procedure that works for small sized samples too (Kim 2017), and so that was the one I used.

I calculated the statistical significance of the effect of treatment type on treatment outcome in both phonological and semantic anomia with two Fisher's exact tests using the SPSS statistical package for Mac; results and discussion can be found in the following section.

4.2.3.2 Results and discussion

Of the 9 studies I decided to include in my analysis, 22 different results for the effects of therapy type on anomia type were elicited. Results showed that phonological anomia could be treated with phonological therapy 7 times out of 9 (or 78%) of the time and semantic therapy 1 out of 3 (33%):

Therapy type	**Therapy result**	
	Treated	Not treated
Semantic therapy	1	2
Phonological therapy	7	2

Table 5.14 The effects of semantic therapy and phonological therapy at treating phonological anomia in the sample

Semantic anomia, on the other hand, could be treated with semantic therapy 71% of the time (5 times out of 7) and phonological therapy 67% (2 out of 3 times):

Therapy type	**Therapy result**	
	Treated	Not treated
Semantic therapy	5	2
Phonological therapy	2	1

Table 5.15 The effects of semantic therapy and phonological therapy at treating semantic anomia in the sample

This is interesting at first glance, because although phonological therapy looks to be much more effective than semantic therapy at treating phonological anomia, the fact that semantic therapy has a successful effect 33% of the time is indicative that the phonological processor is able to act on semantic input after all (a third of the time, in fact), which would suggest that phonology is neither domain specific nor massively modular.

Fisher's exact tests which considered the effects the two therapy types had on the treatment outcome in patients with semantic and phonological anomia showed that the above results weren't statistically significant, however. The results for patients with phonological were p = 0.2364, which, being >0.05 (the p value taken to be the level of statistical significance in the social sciences), counts as weak evidence against the null hypothesis, and so it is unlikely that there exists a relationship between treatment type and treatment outcome in patients with phonological anomia.

The p value for the outcomes of therapy types happening by chance in people with semantic anomia was even greater at 1, which means that the result for people with semantic anomia is even less statistically significant than the one for people with phonological anomia is. For patients with semantic anomia, it is even less likely that treatment type and treatment outcome are related than they are for phonological anomia.

Now, the reason I studied the efficacies of semantic and phonological therapies at treating semantic anomia when this thesis questions whether phonology can be considered a module of the mind is because, if both semantics and phonology were part of the grammar, we would expect them to pattern the same way. Noel Burton-Roberts (2011) argues that phonology, in being grounded in phonetics, is neither modular nor part of the grammar (while semantics is), but since a treatment type/treatment outcome relationship was found for neither phonology nor semantics, this is indicative, to my mind, that whatever their statuses are, modular or ammodular, it's likely that they're the same – either both of them are parts of a modular grammar or neither of them are parts of a modular grammar.

However, there are a number of variables at play here that have the ability to affect the statistical significance of these results. The size of the sample is the one that first comes to mind, but word frequency and length and orthography are known, in the aphasiology literature, to also affect naming and so I consider those in 4.2.3.2.1. It's also important to consider the effects that publication bias (if there's been any) could have on the conclusion I come to in this thesis, and so I will do that too in section 4.2.3.2.2.

4.2.3.2.1 The effects on statistical significance

4.2.3.2.1.1 Sample size

When constructing a sample, one should really consider the number of results necessary to achieve representativeness of the sample in the wider context, but agreeing on the optimum number of subjects that guarantees this representativeness has always been a bit of an issue for linguists (Silva-Corvalán 2001). In early work, it was thought that linguistic behaviour was relatively homogenous so small samples would be sufficient to measure correlation (e.g. Labov (1966: 181) proposed that '10-20 instances of a given variable is sufficient to assign a value that fits into a complete matrix'), but more recently people (e.g. Milroy (1987)) have proposed that sample sizes this small are insufficient with Milroy (1987) arguing that n=30 should be taken as the watershed between sufficient and insufficient samples.

Of course, it should go without saying that the smaller the sample size the less sure we can be about the accuracy of the conclusions we draw from that sample, but as Milroy and Gordon (2003) rightly point out, sample sizes are partly dictated by practicalities (as mine was). A great deal of data was considered; it just wasn't all relevant to the answering of my research question, it turned out. This is because clear cases of semantic anomia and phonological anomia are few and far between – as are purely semantic and purely phonological therapies. The ways in which researchers diagnosed phonetic disorders as phonological ones posed a problem too, as did the ways in which they tested for statistical significance. It makes sense to say then that while we can draw qualitative conclusions from my data sample (e.g. the fact that semantic therapy was found to treat anomia 33% of the time could cast doubt on theories that phonology is a domain specific massive module), because *n* only equalled 22 (and the efficacies of only 3 semantic therapies at treating phonological anomia were considered) we should draw quantitative conclusions only tentatively. More robust results would be produced from a sample that was greater in size than mine, and so I suggest that for future research.

4.2.3.2.1.2 Factors that affect naming

4.2.3.2.1.2.1 Word frequency and length

There have also been a number of other factors found to affect naming such as word frequency, word length, familiarity, age of acquisition, imageability and concreteness, operativity, and animacy (Nickels 1997).

Newcombe et al. (1965), for example, found there to exist a linear relationship between the latency of word naming and the words to be named's frequencies, with lower frequency words taking longer to name than higher ones. Something similar was seen in a number of other studies (e.g. Howes 1964; Rochford and Williams 1965; Butterworth et al. 1984; Howard et al. 1985), but whilst Newcombe et al. (1965) found there to be a negative correlation between word frequency and response latencies (as word frequency went up, response latencies went down), others found word frequency and naming accuracy to be positively correlated (the higher in frequency a word was, the more accurately it was named by participants).

Nevertheless, despite the seeming robustness of these results, not everyone (e.g. Howard et al. 1985; Caramazza and Hillis 1990; Nickels and Howard 1995) has found word frequency to affect naming. Nickels and Howard (1995), for instance, studied the effects of frequency on naming in 27 aphasic subjects and found that frequency was only a significant predictor of performance for 2 of them; the other 25 did not evidence any effects of frequency on naming. Nickels and Howard (1995) suggest that the stark contrast of their results compared with other people's might well be because word frequency, in other studies, is oftentimes confounded with other variables (especially length, to which I'll now turn). In order to determine the amount of variance accounted for by frequency alone, Nickels and Howard (1995) argue, authors would do well to conduct multiple regression analyses.

With regards to word length, a negative correlation between that and naming accuracy has been found by most authors (Dubois et al. 1964; Goodglass et al. 1976; Caplan 1987; Ellis et al. 1983). As was the case with word frequency, though, not every aphasic has been found to show an effect of word length on naming (EST, from Kay and Ellis (1987), didn't, for example, and Best (1995) actually describes the case of a patient who showed a reverse length effect (that is, found shorter words harder to name than longer ones)).

4.2.3.2.1.2.2 Orthography

Now, regardless of whether word frequency and word length or indeed other variables which have the potential to affect naming that I briefly mentioned but that I didn't go into in section 4.2.2.2.1 (such as familiarity, age of acquisition, imageability and concreteness, operativity, and animacy (see Nickels (1997) for a thorough review) had an effect on naming in the participants in my data set, I am particularly interested in figuring out whether treating orthographic therapy as being phonological therapy might have affected the results also.

Of the 7 studies that made use of phonological treatments (Nettleton and Lesser 1991; Hillis and Caramazza 1994; Best et al. 1997; Jokel and Rochon 1998; Hickin et al. 2002a; Hickin et al. 2002b; Raymer and Ellsworth 2002), 3 of them made use of graphemes as well as phonemes in the phonological treatment (Best et al. 1997; Hickin et al. 2002a; Hickin et al. 2002b). In these 3 studies were 5 results, and so I excluded them from my analysis to see how much of an effect including orthographic treatments had. 4 had to do with phonological anomia, and, interestingly enough, it did affect the results, bringing the efficacy of phonological therapy at treating phonological anomia down to 60% of the time from 78%.

Therapy type	**Therapy result**	
	Treated	Not treated
Semantic therapy	1	2
Phonological therapy	3	2

Table 5.16 The effects of semantic therapy and phonological therapy at treating phonological anomia in the sample (excluding those phonological therapies that made use of orthography)

The effect of semantic therapy on phonological anomia stayed the same, of course, at 33% and so, with phonological therapies that make use of orthography excluded, there is even less of a difference between the efficacies of phonological treatments and semantic treatments at treating phonological anomia, which could count as evidence that phonology is, indeed, ammodular.

I will point out, though, that once again, the *p* value produced by a Fisher's exact test was 1, which is, of course, not significant at $p < 0.5$. This means that, although we can conclude that spoken phonological therapies and semantic therapies have a similar success rate for the treatment of phonological anomia (3/2 compared with 2/2, respectively) in this particular sample, it is difficult to determine whether the same success rates would be seen beyond it.

4.2.3.2.2 The effects of publication bias

Publication bias is defined as the failure to publish the results of a study on the basis of the negativity of its findings (DeVito and Goldacre 2018). In empirical research this is a common occurrence – studies that have positive results are more likely to be published than studies that have negative ones. Meta-analyses like mine (which involve statistical analysis of pooled data) should therefore ask themselves whether their data collection has been affected by publication bias, as the answer 'yes' to his question would (and should) affect their data analysis. One way to test for this is by using a funnel plot. If the data when plotted looks to be in the shape of a symmetric funnel, then publication bias is unlikely, it is said. If the plotted data looks asymmetric, on the other hand, then the opposite is thought to be true (Hedin et al. 2016).

Egger's regression is the standard statistical measure for quantifying funnel plot asymmetry, DeVito and Goldacre (2018) explain, but the minimum required number of studies for the use of it is 10. Of course, in this thesis, only 2 studies (which elicited a total of 3 results) were collected for the analysis of the effects of semantic therapy at treating phonological anomia, and so my set is too small for this assessment.

It is important to bear in mind the possible impact publication bias may have on the conclusions I come to in this thesis regardless, I think, for if a number of negative results, that is, a number of studies evidencing that semantic therapy did not effectively treat phonological anomia, went unpublished, had they been so, the percentage of semantic therapy effectively treating phonological anomia in the data may have been much lower. It may have even been so low that I was led to a different conclusion, that phonology is indeed domain specific, and therefore modular.

Now, this is not an argument I'm making. There isn't enough evidence to do so. But this is a variable that neither I can (nor any scientist can) control for that has the potential to affect the analysis of the results and therefore the conclusion of this thesis, and so this is something that should be brought attention to in any scientific study.

5. Concluding remarks

I guess it's possible to argue from the above, then, that, according to Carruthers' (2006a) definition of modularity, phonology could well be considered ammodular. This is because for Carruthers (2006a) the domain specificity (the inability of it to operate on anything that is irrelevant to its function) of a cognitive processor is necessary for its modularity, and semantic therapy, in being able to treat phonological anomia 33% of the time, demonstrates that on those occasions, the patients' phonological processors were operating on semantic information. This would suggest that that phonology is neither domain specific nor massively modular, as semantic information is of course, irrelevant to the function of phonology.

As I said though, we should be careful how much we make of this conclusion, as a Fisher's exact test produced a *p* value of 0.2364 when orthography was included in the phonological therapies and 1 when it wasn't, which means that the results weren't statistically significant either way, and so we should proceed with caution in trying to generalise what was observed in this narrow sample to the wider population.

What I will say is this, though: with such small numbers you're unlikely to get a significant *p* value from Fisher's exact tests *anyway*, regardless of the ways in which the data patterns. Interested to see what pattern would produce a significant *p* value I ran some hypothetical Fisher's exact tests and found that in order for the results to be significant, phonological therapy would have had to have treated phonological anomia 100% of the time and semantic anomia 0% of the time. Even if phonological therapy treated phonological anomia 8 times out of 9 and semantic therapy 1 time out of 3 the *p* value would have been > 0.05 (0.1273):

Therapy type	**Therapy result**	
	Treated	Not treated
Semantic therapy	1	2
Phonological therapy	8	1

Table 5.17 The hypothetical effects of semantic therapy and phonological therapy at treating phonological anomia if the data patterned differently

For this reason, then, I think it’s safe to say then that my results might not necessarily be statistically insignificant because of the data’s patterning. Rather, it might be because the sample was simply too small. We could, therefore, (very) tentatively conclude that phonology is amodular, but in order to reach a clearer conclusion than the one I have here, further research must be conducted.

Chapter 6: Conclusion

This philosophy of mind (and language) thesis was unique in that it drew on data from different domains of linguistics (phonology and language change, in particular) in Chapter 4, and a different field of inquiry altogether (that of speech and language pathology) in Chapter 5, in order to shed some light on the question of whether or not phonology can be considered a module of the mind. My hope was that an answer would one day inform the models of word retrieval that are used in aphasiology, and in turn, the work of speech and language pathologists to improve treatment for phonological anomia.

In Chapter 4, I explored the extent to which phonology could be considered substance-free, and therefore an informationally encapsulated, Fodorian (1983, 2000) module of the mind, and in Chapter 5, whether or not word retrieval in phonological anomia could be facilitated by phonological therapy alone (or semantic therapy too), and therefore whether or not phonology could be considered a domain specific, Carrutherian (2006a) module.

At first glance, it looked as though each Chapter led me to a different conclusion; that phonology is substance-free and therefore can be considered modular according to Fodor's (1983, 2000) definition of modularity on the one hand (in 4), but isn't domain specific and therefore can't be considered modular according to Carruthers' (2006a) definition on the other (in 5).

This finding has important implications for Jackendoff's (2002) parallel architecture for the language faculty. Jackendoff's (2002) model, recall, is made up of three informationally encapsulated integrative modules (phonology, syntax and semantics) with likewise informationally encapsulated interface modules between them, that convert phonological to syntactic information (and vice versa), syntactic to semantic information (and vice versa) and semantic to phonological information (once again, and vice versa). The phonological, syntactic and semantic modules, therefore, operate on information specialised for their domain only (phonology on phonological information, syntax or syntactic information and semantics on semantic information).

Cognitive systems are informationally encapsulated, remember, to the extent that in the course of *processing* a given set of inputs (that are relevant *or* irrelevant to their function) they cannot access information stored beyond the module; all they have to go on is the information that's contained in those inputs, and what's stored in the system itself. Phonology is informationally encapsulated in this model, in that is free of phonetic substance. It is at the phonetics-phonology interface, he proposes, that underlying phonological representations are turned into surface phonological representations, and at the phonetics-phonology interface (for he proposes two different ones) that phonetic forms become phonological.

Domain specific cognitive systems, remember, operate only on certain sorts of inputs – those that are specialised for their domain. They can operate only on inputs relevant to their function, as phonology, syntax and semantics all do in this model. The two features, although similar, are decidedly different then in that domain specificity restricts what information is accepted as input – what kind of information makes a system operate – and informational encapsulation restricts what kind of information is able to influence its operation.

To reiterate, Jackendoff's model (2002) therefore requires phonology to be both informationally encapsulated *and* domain specific. To his mind, phonological data is free of phonetic substance (because phonetic data is computed to become phonological data before it enters the module), and the phonological processor is domain specific, in that it is specialised to operate on the domain of phonological data only.

The finding in Chapter 5 of this thesis that the phonological processor was able to operate on semantic information in that phonological anomia could be facilitated by semantic anomia therapy, in evidencing that phonology is not domain specific leads us to a question, also, Jackendoff's parallel architecture of the language faculty. If phonology is not domain specific, it is neither modular, according to Carruthers (2006a), nor does Jackendoff's (2002) model appropriately capture its character.

Once again though, I will point out that Chapter 5's conclusion wasn't as clear-cut as the one in Chapter 4 was. The results in my dataset were found through Fisher's exact tests to be statistically insignificant, and so a definitive answer as to whether semantic therapy consistently facilitates word retrieval in phonological anomia (making phonology non-domain specific and ergo not massively modular) will require more results to be analysed than have been so here.

Using statistical procedures, one is able to determine whether the null hypothesis (that what's observed was due to chance, and chance only) is accepted or rejected, and since my results weren't statistically significant, scientifically speaking, we should accept that. However, there is something to be said about the sample size here. In focusing exclusively on the statistical significant results of studies that involved investigation of single cases or series of single cases in which patients had semantic impairments only, or phonological impairments only, and received therapies that were purely semantic and/or purely phonological, a large number of papers I extracted from Nickels' (2002)'s catalogue had to be excluded from the analysis in order to ensure accuracy. Of the 43 papers available, only 9 turned out to meet the criteria for inclusion. This left me with a sample size of $n = 22$ (which is perhaps what made for the statistically insignificant test results, rather than the patterning).

As I said in Chapter 5, with a sample size as small as mine was (just 12 results for the treatment of phonological anomia) it's unlikely a Fisher's exact test would have yielded any statistically significant *p* values, no matter the patterning of the data. If the *same* pattern were observed in a sample ten times the size than mine (i.e. if 120 results were yielded instead of 12 with phonological therapy treating phonological anomia successfully 70 times out of 90 instead of 7 times out 9, and semantic therapy treating phonological anomia 10 times out of 30 successfully instead of 1 time out of 3, then the Fisher's exact test *p* value would have indeed been statistically significant (at 0, no less):

Therapy type	**Therapy result**	
	Treated	Not treated
Semantic therapy	10	20
Phonological therapy	70	20

Table 6.1 The hypothetical effects of semantic therapy and phonological therapy at treating phonological anomia if the data patterned the same, but the sample were ten times the size

If this were the case, one would be able to conclude confidently that although phonological therapy was better at facilitating word retrieval in people with phonological anomia, semantic therapy did too 33% of the time, which would count as clear evidence against phonology's domain specificity (and massive modularity).

My results might well have posed a challenge to the view that phonology is domain specific if my sample size was larger then, which is interesting because if that were the case, one might conclude that whether phonology can be considered modular or not depends, really, on your definition of modularity. The fact of the matter is though; it was not. The sample size was small, and so whilst we can say that there is no real difference between the efficacies of semantic and phonological therapies at treating phonological anomia in *this* data set, it's difficult to say too much about what this means for modularity without being able to generalise what's been seen in this portion of the population to the whole population (which is what a statistically significant result would have allowed us to do).

It's safe to say then that this thesis has raised a number of questions. On the one hand, I've argued that phonology is substance-free and therefore can be considered an informationally encapsulated Fodorian (1983, 2000) module of the mind but on the other have found (an albeit small and statistically insignificant amount of) evidence to suggest that phonology might not be domain specific after all and therefore ammodular by Carruthers' (2006a) definition.

My results incline me to conclude (albeit tentatively) that phonology is substance-free and therefore modular in the modest sense of the word, but not domain specific (and therefore not modular) in the massive one – which of course casts some doubt on whether the model of the mind described by Jackoff is adequate after all, since Jackendoff (2002) requires phonology to be both.

However, whilst we can define a module as being informationally encapsulated but not domain specific in *theory*, it's not clear to me how that would work in *practice*. A non-domain specific phonological module would be able to operate on, say, semantic and syntactic information. But, in it being informationally encapsulated, semantic and syntactic knowledge wouldn't be able to influence its operations. This doesn't make much sense.

I should also probably point out, that even in a substance-free model of phonology, domain specificity is expected. For if phonology is able to operate only on substance-free phonological computation, then it is specialised for the domain of substance-free phonological computation, not substance-free semantic computation, say, or substance-free syntactic computation. To me, it appears that the two, informational encapsulation and domain specificity, that is, would go hand in hand, though that's not what the results of this thesis predict.

Of course, a study of a similar methodology to this one but of a larger sample size than mine would have to be carried out in order to confirm or deny whether that latter component of my conclusion (that phonology is not domain specific) is indeed the case. Because although I have found some evidence to suggest that phonology is able to operate on semantic information and is therefore not domain specific, as I've said, the sample size was small making for a statistically insignificant result. There are a number of other variables that have the potential to have affected these results too, including, but not limited to, the possibility that publication bias could have influenced which effects of semantic anomia therapy at treating phonological anomia were published, and which ones weren't. As I said in Chapter 5, studies that have positive results are more likely to be published than studies that have negative ones in the sciences, and had there been more studies with negative results been published and included in my data set, then the percentage of semantic therapies that were found to treat phonological anomia in thesis may well have been lower than 33%.

I regret that a more definitive answer to my research question has not been found, but hope that what I have done lies some significant groundwork for future study by establishing a direction of inquiry, and sufficiently summarising the limitations with the current study such that they can be circumvented if a similar one were to be undertaken. Whether phonology is modular or not then, as ever, remains a mystery. But until we have just one thesis of modularity (and phonology, for that matter), I think that's likely to be the case.

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