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Speech and nonspeech: What are we talking about?

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Abstract

Understanding of the behavioural, cognitive, and neural underpinnings of speech production is of interest theoretically, and is important for understanding disorders of speech production and how to assess and treat such disorders in the clinic. This paper addresses two claims about the neuromotor control of speech production: (1) speech is subserved by a distinct, specialised motor control system, (2) speech is holistic and cannot be decomposed into smaller primitives. Both claims have gained traction in recent literature, and are central to a task-dependent model of speech motor control (Ziegler, 2003a). The purpose of this paper is to stimulate thinking about speech production, its disorders, and the clinical implications of these claims. The paper poses several conceptual and empirical challenges for these claims – including the critical importance of defining speech. The emerging conclusion is that a task-dependent model is called into question as its two central claims are founded on ill-defined and inconsistently applied concepts. The paper concludes with discussion of methodological and clinical implications, including the potential utility of diadochokinetic (DDK) tasks in assessment of motor speech disorders and the contraindication of nonspeech oral motor exercises to improve speech function.

INTRODUCTION

In recent years, there has been debate regarding the special status of speech among motor behaviours (Ballard, Robin, & Folkins, 2003; Bunton, 2008; Weismer, 2006; Ziegler, 2003a,b; Ziegler & Ackermann, 2013). Two main views in this debate are the task-dependent model (TDM; Ziegler, 2003a,b; Ziegler & Ackermann, 2013) and the integrative model (IM; Ballard et al., 2003)[1]. Briefly, the TDM proposes a specialised, distinct neuromotor control system dedicated to speech production, whereas other actions of the anatomical apparatus involved in speaking (e.g. laughing, novel oral movements) are controlled by fundamentally different motor control systems. In contrast, the IM proposes that speech production involves a particular, unique combination of skills and properties, some of which are shared with other motor behaviours, and as such proposes overlapping behavioural and neural control systems for speech and other motor behaviours. This debate is relevant for the understanding of human motor behaviour in general and speech behaviour

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[1]These are not the only possible views (Weismer, 2006), and each may represent a class of models. I focus on these two views, and these two specific claims, because they have been discussed relatively explicitly. Occasionally I will take liberties with stated positions to develop the broader discussion.

in particular as well as the neural mechanisms underlying such behaviour, but also for the methods by which we study or influence such behaviour in the lab and in the clinic. The TDM seems to represent a common, if not the prevailing view in current literature.

The present paper seeks to bring some of the issues into sharper focus, raise some critical questions for two particular claims integral to the TDM, and explicate and explore implications of these claims. The hope is that this paper will make a positive contribution by identifying areas where views diverge – and thus, where theoretical and empirical attention can be most fruitfully directed to adjudicate between alternatives and advance our understanding of speech production.

This paper will focus on two particular claims, versions of which have been eloquently laid out in recent years by various authors (Bunton, 2008; Weismer, 2006; Ziegler, 2003a,b; Ziegler & Ackermann, 2013). In particular, we will examine the claims that **(1) speech is controlled by a specialised, distinct, dedicated neuromotor control system**, and **(2) speech is a holistic behaviour which cannot be decomposed into smaller parts**. Although the intent is not to rehash old arguments, occasional clarifications of such arguments will be provided to resolve possible misinterpretations and develop the discussion. The purpose of this paper is to stimulate further thought about what it means to say that speech is special, and how different views affect clinical decisions regarding assessment and treatment of speech disorders.

Although occasional references to neuroimaging studies will be made, the primary emphasis in this paper will be on behavioural rather than neuroimaging studies (the interested reader is referred to Hickok, Houde, & Rong, 2011, for a synthesis and review). The main reason is that neural activation patterns represent dependent measures that can be interpreted and understood only in relation to the *behaviour* they are thought to capture (see also Coltheart, 2006, for further discussion of neuroimaging studies to address cognitive theories). In other words, an essential first step is to define the behaviour of interest and the tasks which represent this behaviour, so that tasks can be compared (Weismer, 2006). The literature contains findings of neural overlap (e.g. Chang et al., 2009; Segawa et al., 2015) as well as neural differences (e.g. Wildgruber et al., 1996) between tasks designated as speech or nonspeech. The present paper is concerned with this essential first step in that it discusses some of the complications in drawing distinctions between tasks and designating them as speech or nonspeech, and as such may help shed light on these discrepant findings from the neuroimaging literature and their implications for our understanding of speech motor control.

The structure of the paper is as follows. First, I will set the stage by outlining the claims and contrast them with an integrative view to highlight the crux of the disagreement. Next, I will raise some conceptual and empirical challenges to the two claims above. Finally, I will discuss implications for the scientific study of speech production and for clinical practice.

PRELIMINARIES

A point of agreement is that both views accept that speech is indeed a special motor skill. To deny this is even on its face not a tenable position. Both views agree that typical speech production involves a particular combination of properties (e.g. control of an acoustic signal, articulator movements). At issue is how this behaviour is controlled, in terms of neural and cognitive organisation, and the associated scientific and clinical implications.

The TDM espouses two more specific claims about the specialness of speech. First, the specialness is reflected in the existence of a distinct motor control system used only for producing speech. This claim was formulated clearly by Bunton (2008: 271–272), who wrote ‘Even though [nonspeech tasks] may involve the same musculature as speech, the tasks are so different that their control must be assumed to be based on different neural networks.’ Similarly, Ziegler (2003: 5) stated ‘These subsystems [for speaking versus other tasks] are separate to the extent that each of them has unique properties, is subserved by a specialised neural circuitry […].’ In other words, this view postulates a *categorical* distinction between speech and other motor behaviours.[2] Second, speech motor control is holistic and speech movements cannot be decomposed into component parts (“primitives”). This claim is reflected in Ziegler and Ackermann’s (2013) statement that ‘[…] vocal tract gestures in speaking […] can only be understood properly through their joint interaction in fabricating the sounds of syllables and words. From such a connectivist point of view, the constituents of a speech motor action can neither be isolated from their gestural context nor from their linguistic reference frame.’ (p. 62). Similarly, Weismer (2006: 332) wrote that ‘disintegrating a system for isolated study of component parts does not allow study of the system’s typical behaviors.’ Although in a trivial sense this is true (one cannot observe the system’s typical behaviours when typical behaviours are absent), this claim suggests that speech motor control can only be understood and studied if all components of typical speech are present (i.e. the “primitive” is the task of speaking).

Although these are two separate claims, they are related in that the first claim depends on speech constituting a single category: delineation of *the* control system for speech versus other motor behaviours is best characterised in terms of broadly defined superordinate tasks (e.g. “speaking”, “chewing”), rather than in terms of the various subordinate properties or components involved in these actions. However, the second claim does not depend on the first: a holistic, indecomposable behaviour need not be subserved by a separate, dedicated neuromotor control system. Central to both claims is the delineation of speech as a unitary task category. Insofar as speech comprises describable components (e.g. articulator movement, control of acoustic signal), only when all components are present does the task represent speech (claim 2) and engage a distinct, specialised neuromotor control system (claim 1). When only a subset of these properties is combined into an action, a fundamentally different system is responsible for its control.

[2]Similar debates about the existence of speech-specific systems versus a more general system occur in the speech perception literature (e.g. Liberman & Whalen, 2000; Holt & Lotto, 2008). The focus of the present paper is restricted to speech production however.

In contrast, according to the IM, control of speech involves a motor system that integrates and coordinates movement properties and components for a variety of motor tasks, and speech is one of such motor tasks. Ballard et al. (2003: 38) proposed '… an integrative model in which some nonspeech motor tasks share principles with speech and some do not […]. This leads us to postulate overlapping neural and behavioural systems for the control of speech and volitional nonspeech tasks.' They go on to say 'Thus speech motor control is integrative, sharing properties with some but not all nonspeech motor tasks. We are not claiming complete task-independence or task-dependence, but rather suggesting that certain volitional nonspeech tasks share principles in common with speech and therefore speech motor anomalies (e.g. apraxia). We hypothesise that, at complex behavioural levels, there must be overlapping functional components and therefore overlapping and integrative neural pathways or networks.' (Ballard et al., 2003: 39). In other words, this view proposes a *gradient* distinction between speech and other motor behaviours, with some but not all properties shared. In a sense, this view represents a position intermediate between two extremes (a completely task-specific vs. a completely domain-general motor control system). This view holds that speech can be decomposed and that the motor control system can best be understood in terms of task components rather than broadly defined tasks (constellations of components). Thus, although speech is a special motor skill, it does not require postulation of a specialised neuromotor control system, and can be understood by examining properties in isolation and in various combinations (including typical speech, the "full" combination). Comparisons between typical speech and certain nonspeech motor tasks is considered potentially informative regarding organising principles underlying speech production.

Several authors have posed the question for the IM of how to define similarity of speech and nonspeech movements (Bunton, 2008; Weismer, 2006). Indeed, the onus is on the IM to identify, in a principled manner, the properties presumed to be shared between speech and nonspeech tasks, and this enterprise has not been straightforward (Weismer, 2006). This is a valid criticism, and I will not reiterate the cogent arguments presented by these authors. Instead, I submit that neither view escapes this need to clarify and define criteria of similarity. Just as the IM must define *similarity*, the TDM must define *dissimilarity* between speech and nonspeech motor behaviours. This is essentially the same concern approached from opposite directions, but this requirement is perhaps even more pressing for a view that stipulates a categorical distinction. To understand what speech is, we must also understand what it is not. As I discuss below, this enterprise is not straightforward either, and has largely been avoided to date (Kent, 2015).

CONCEPTUAL AND EMPIRICAL CHALLENGES

In this section, I will pose some critical challenges for the two claims of the TDM. I will do so by addressing two main questions: *What is speech?* and *What is a system?* This section ends with a brief discussion of challenges regarding the emergence of dedicated systems. As will become evident, possible solutions to these challenges tend to be unprincipled, inconsistently applied, and/or constitute de facto acceptance of the IM and decomposability of speech.

Definitions: What is speech?

A special, distinct control system for speech is predicated on delineation of speech from other behaviours. Yet despite the centrality of "speech" to the TDM, three nontrivial problems exist regarding definition and delineation of this construct: lack of explicit definitions, lack of consensus about necessary and sufficient criteria, and inconsistent application of definitions.

First, as Kent (2015) noted, explicit definitions of speech are often conspicuously absent from articles proposing a TDM, even those that include a section with definitions (e.g. Weismer, 2006). To quote Weismer (2006: 343): 'Gardner (1985, p. 286) […] wrote, "One cannot have an adequate theory about anything the brain does unless one also has an adequate theory about that activity itself." ' I would argue that this includes an adequate *definition* of that activity.

Second, although proponents of a TDM have suggested a number of task properties that supposedly delineate speech from nonspeech tasks, there does not appear to be a consensus about which ones are necessary and/or sufficient. Some tasks not considered speech by proponents of a TDM share these properties, and other tasks considered speech by TDM proponents lack these properties. In the next few paragraphs, I review several proposed properties to illustrate some reasons for this lack of consensus. Two important suggested properties of speech are that (1) it produces an acoustic signal (2) that is used to communicate (Weismer, 2006; Ziegler, 2003a,b). The first property (acoustic signal) appropriately excludes oral movements without acoustic consequences such as tongue wagging. However, it also excludes articulation without sound, as may occur in natural environments (Gick et al., 2012) or in experimental contexts. One could argue that such soundless tasks are not speech, yet TDM proponents consider covert speech (silent mouthing of words) to reflect the speech system (Bunton, 2008; Wildgruber, Ackermann, & Grodd, 2001; Wildgruber et al., 1996), despite differences between overt and covert speech in terms of neural circuitry (e.g. Riecker, Ackermann, Wildgruber, Dogil, & Grodd, 2000). Thus, an acoustic signal appears to be neither sufficient nor necessary.

The second property (communicative purpose) appropriately excludes oral motor behaviours that produce rhythmic acoustic signals but are not used to communicate, such as human beat box performance (De Torcy et al., 2014). It also excludes diadochokinetic (DDK) tasks (e.g. saying *pataka* rapidly and repeatedly), which are indeed explicitly designated nonspeech tasks by TDM proponents (Bunton, 2008; Ziegler, 2002). However, the requirement for communicative intent also excludes behaviours that might be considered speech, such as talking in one's sleep (Kent, 2015), or speech-like, such as infant babble (Moore & Ruark, 1996).[3] To complicate matters further, oral movements that produce acoustic signals with a communicative purpose, such as the click sound *tsk-tsk* to convey disapproval or sighing

[3]Moore and colleagues referred to variegated babbling as 'prespeech' behaviour (Moore & Ruark, 1996: 1036) and considered such babble to have no communicative intent (vocalisations were generated during self-directed play and judged to be 'neither meaningful nor referential'; Moore & Ruark, 1996: 1037). Although Moore and colleagues (Moore & Ruark, 1996; Moore, Caulfield, & Green, 2001) have convincingly demonstrated significant kinetic and kinematic differences between first words and oral motor behaviours such as chewing, their work also shows considerable similarities between variegated babbling and first words (in fact, Moore et al., 2001, grouped vocalizations, babbling, and 'real' speech into a single category for analysis given lack of differences).

loudly to communicate exasperation, are not considered speech (Aichert & Ziegler, 2013). Thus, communicative purpose is not a necessary or sufficient property of speech either.

One possible solution was suggested indirectly by Weismer (2006). His definition of nonspeech tasks refers to phonetic goals: ‘Oromotor, nonverbal tasks: Any performance task, *absent phonetic goals*, in which structures of the speech mechanism […] are measured’ (p. 319, italics mine). Similarly, Ziegler and Ackermann (2013) refer to ‘vocal tract motor circuitry which is specifically dedicated to the generation of acoustic patterns typical of a speaker’s native language’. (p. 61). Kent (2015), one of few authors to provide an explicit definition, also refers to phonetic structure: ‘Speech is defined as movements or movement plans that produce as their end result acoustic patterns that accord with the phonetic structure of a language’ (p. 765).

Phonetic structure does appear to constitute a necessary condition for speech (assuming that covert speech has phonetic structure). Nevertheless, even here complications arise. For instance, if phonetic patterns must be those of the native language, this implies that non-native speech patterns involve a nonspeech oral motor system. In support of this idea, Ziegler and Ackermann (2013) note the persistence of foreign accents in late second-language learners. However, an alternative interpretation is that the residual accent is evidence *for* use of a speech motor system: the accent reveals the influence of the native-language speech motor system. Use of a nonspeech motor system cannot account for language-specific influences on the second language (Flege, Schirru, & MacKay, 2003) or vice versa (Major, 1992). Instead, one would predict more universally similar non-native accents. Further, some oral motor behaviors not typically considered speech also have phonetic structure, such as human beat box performance (De Torcy et al., 2014: 38: ‘to achieve their ends, the beatboxers manipulate speech sounds’) and communicative utterances such as *mmm* (/m:/) to convey enjoyment of a tasty treat, or *shh* (/ʃ:/) to request silence (Aichert & Ziegler, 2013). Aichert and Ziegler attempt to resolve this conflict by stating that speech patterns must consist of at least a syllable. Thus, isolated diphthongs or vowels such as /ɑɪ/ (*eye*) or /ɑ/ (*awe*) are speech because they can be syllables, but utterances consisting of isolated consonants are instead ‘high frequent, overlearned nonverbal expressions and not speech’ (p. 1194), because they cannot be syllables. However, some consonants can also form syllables used to communicate, both as “nonverbal expressions” (e.g. *m-m* [ʔm̩ʔm̩] to express disagreement) or as (parts of) words (e.g. *rhythm* [ɹɪðm̩], *pack them up* [p^{h}æk m̩ ʌp]). Finally, DDK tasks (e.g. saying *pataka* repeatedly)[4] generate acoustic-phonetic patterns of the native language. Yet, as noted above, such tasks are explicitly designated as nonspeech by TDM proponents (Bunton, 2008; Weismer, 2006; Ziegler, 2002, 2003a).

What, then, are the critical task aspects that delineate speech from nonspeech tasks such as DDK? Two features have been proposed as distinguishing criteria (Ziegler, 2002), including repetitive production and maximal rate demands. However, repeated production of a sound sequence also occurs in conversational speech, for example in emphatic (dis)agreements (*yes*

[4]Typically, the task instructions are to ***say*** *pa* or *pataka* (etc.), not ***make this movement pattern***. That is, typically DDK tasks are presented as a speaking task.

yes yes or *no no no no*), the Seinfeldian ellipsis phrase *yada yada yada*, invocations (*Beetlejuice Beetlejuice Beetlejuice*), or utterances such as *It went on and on and on*. Thus, repetitive production is neither necessary nor sufficient to change speech into a nonspeech task.

Several authors suggested maximal repetition rate as a distinguishing criterion (Weismer, 2006; Ziegler, 2002): producing *pataka* at a normal rate (*This is a nice pataka*) is speech, but repeating *pataka* as fast as possible is nonspeech (a 'DDK mode' of oral motor control; Ziegler, 2002: 571). However, by this criterion, the acoustic patterns of oral communication produced by auctioneers would *not* be speech, because of their very rapid (likely maximal) rates. Further, if one slows speech down enough, the speaker may enter 'an alternative, more conscious control mode' (Ziegler, 2003a: 24). Does this mean that speakers with apraxia of speech (AOS) or dysarthria, who may have slow speech rate (although they may speak at the fast end of their range), do not produce speech? The difficulty here is how to independently, in a principled way, establish the "speech range rate" for a given speaker. At what rate does a pattern of oral movements with acoustic output change from speech to nonspeech (on either end of the range)?

The third problem with defining and delineating speech and nonspeech tasks is that criteria have been inconsistently applied between, and even within authors. For instance, in addition to examples already given (e.g. acoustic signal is necessary vs. covert speech), reiterant syllable production in DDK is considered a nonspeech task (e.g. Bunton, 2008; Ziegler, 2003a), yet elsewhere reiterant syllable production has been considered speech (e.g. Bunton & Weismer, 1994; Deger & Ziegler, 2002). This problem has important consequences for how we study, and draw inferences about, speech motor control (see *Methodological Implications* below).

The foregoing discussion highlights difficulties in delineating, in a principled, consistent manner, speech as a single behaviour that is categorically distinct from nonspeech behaviours. The crux of the difference between the IM and TDM is that, unlike the IM, the TDM essentially stipulates such a distinction between (more or less speech-like) nonspeech tasks and "true" speech (which itself also likely comprises a range of tasks; Kent, 2015). The lack of consistent and principled criteria to support such a delineation, upon which the TDM is predicated, undermines the validity or utility of the distinction – and thereby the notion of a specialised control system. Perhaps the wide range and complexity of oral motor behaviors makes it fundamentally impossible to delineate all speech from all nonspeech tasks. However, clear and consistent delineation of speech and nonspeech tasks is necessary in order to advance and empirically test a theoretical view that critically hinges on the existence of a category of speech as distinct from nonspeech tasks. Note also that the postulation of 'quasi-speech' tasks (Weismer, 2006: 319) is at variance with a categorical distinction and indecomposability: accepting that tasks can be more or less speech-like (in both speech and nonspeech categories) suggests a gradient distinction or a task space ranging from very speech-like (e.g. naturalistic conversation) to very nonspeech-like (e.g. lateral tongue wags). This is in fact what the IM proposes. To stipulate some (ill-defined) categorical point along this task space, more-or-less arbitrarily refer to one set of tasks as "speech" and another as "nonspeech", and then propose specialised machinery for these categories is neither necessary nor illuminating.

Dissociations and differences: What is a system?

Inextricably linked to the issue of distinguishing speech from nonspeech behaviours is the question of how to distinguish systems. A common approach is to identify task dissociations or differences (e.g. in kinematic or neural measures). Associations are less informative about the organisation and architecture of a cognitive system than dissociations, especially double dissociations, given possible third-variable correlations with factors such as severity or shared neural tissue (Weismer, 2006; Ziegler, 2003a).[5] There is no disagreement here. However, assuming clear, consistent, and agreed-upon task definitions can be formulated, two considerations limit the value of dissociations to distinguish speech from nonspeech motor control: (1) dissociations need not reflect *motor system* distinctions, and (2) they also exist between different speech tasks.

First, dissociations do not require an interpretation involving different *motor* systems, one for speech and one for nonspeech tasks. For example, in addition to the many differences in motoric aspects (Ballard et al., 2000; Ziegler, 2003a), dissociations between speech (AOS) and nonspeech volitional oral movements such as tongue protrusion (nonverbal oral apraxia) may be explained in terms of visuo-spatial processing[6] (e.g. Bizzozero et al., 2000; Kramer et al., 1985), language deficits (Botha et al., 2014; Square-Storer et al., 1990), or other cognitive factors.

Second, even if non-motoric factors are ruled out, a dissociation between two motor tasks does not in itself indicate that one is a *speech* motor task and the other is a *nonspeech* motor task, because dissociations and differences also exist between tasks that are both considered speech (e.g. Caviness, Liss, Adler, & Evidente, 2006[7]; Deger & Ziegler, 2002; Tasko & McClean, 2004; Tsao & Weismer, 1997; Ziegler, Kilian, & Deger, 1997). For instance, Tsao and Weismer asked participants to read a passage 10 times each at a habitual and at maximum rate, and classified speakers into a slow and a fast group based on their habitual speaking rate. They reported a double dissociation: at least one speaker from the slow group produced among the fastest maximum rates, and several speakers from the fast group had maximum rates in the range of the slow group. Does this mean that speaking at habitual rate and speaking at maximum rate are controlled by two distinct motor control systems – and that only one of these is speech? This would be consistent with the notion that DDK tasks are not speech because of the maximal rate demands. Yet Tsao and Weismer do not draw this conclusion, and instead suggest that variation in habitual speaking rate may be explained by differences in motor limits, which may depend on a cerebellar time keeping mechanism also involved in limb motor control.

[5]No quantified and independent measures of such third variables have been proposed, to my knowledge (severity as operationalised in terms of speaking rate or intelligibility is not independent from speech). As a result, such third-variable explanations tend to be untestable.

[6]For example, Ziegler (2003a: 29) refers to 'integration of visual information with a subject's body image' as a non-motoric task aspect that differs between speech and imitating oral movements.

[7]Caviness et al. (2006) explicitly define speech broadly as tasks involving simultaneous phonation and articulation, which includes sustained vowel production and reiterant speech, as well as two connected speech (reading) tasks. They reported differences between the two connected speech tasks.

In terms of neurological dissociations, two studies by Ziegler and colleagues provide support for the dissociability of syllable sequencing and initiation and assembling multisyllabic sequences into a single program (Deger & Ziegler, 2002; Ziegler, Kilian, & Deger, 1997). In a simple (delayed) reaction time (RT) paradigm, speakers were asked to produce syllable strings such as 'dada', 'dadada', and 'daba'. A length effect (RT for 'dadada' > RT for 'dada') was interpreted as reflecting the additional time needed to initiate and 'unpack' an additional syllable motor subprogram from an articulatory buffer. A complexity effect (RT for 'daba' > RT for 'dada') was taken to reflect difficulty in assembling two syllables into a single motor program. Ziegler et al. (1997) reported a patient with supplementary motor cortex damage who presented with dysfluent speech but who produced no segmental substitutions or distortions. This patient demonstrated a length effect but not a complexity effect (unlike unimpaired speakers, who showed neither), suggesting that her impairment was in initiating or unpacking a sequence of syllables (regardless of the specific content of those syllables, i.e. intact assembly). In contrast, Deger and Ziegler (2002) reported that speakers with AOS demonstrated the opposite pattern: a complexity effect but not a length effect, suggesting that their impairment was in combining multiple syllables into a single program but not in initiating syllables within a sequence. In other words, together these two studies suggest a double dissociation between aspects of speech motor control, derived from the same task – a within-speech dissocation.

As another example, both behavioural and neural evidence suggests that the speech motor programming routines for producing low- versus high-frequency syllables are qualitatively different (Aichert & Ziegler, 2004; Bürki, Cheneval, & Laganaro, 2015; Cholin, Dell, & Levelt, 2011). Yet both types of routines are considered part of the speech system.

In this light, a dissociation reported by Ziegler (2002), one of the strongest pieces of evidence raised in support of a TDM, becomes less clear-cut. Ziegler reported a dissociation in speech rate between repeating a sentence containing a nonword (without rate instructions) and an alternating motion rate DDK task (with maximal rate instructions). Ziegler compared unimpaired speakers, speakers with AOS, and speakers with ataxic dysarthria matched on duration of a target syllable in the sentence repetition task. Unimpaired speakers and speakers with AOS had shorter syllable durations in DDK compared to sentence repetition, and speakers with ataxic dysarthria showed the reverse. Ziegler explained this pattern by suggesting that cerebellar pathology (as in ataxic dysarthria) affects the ability to use sensory feedback to form predictive, feedforward commands to perform a relatively novel syllable repetition task, whereas sentence repetition relies more on established feedforward commands and is thus less affected by cerebellar damage. Although Ziegler cast this dissociation in terms of different motor control systems (one speech and one nonspeech), another interpretation is in terms of feedback- versus feedforward-based control mechanisms, both of which play a role in speech motor control (e.g. Guenther et al., 2006; Hickok et al., 2011) – in other words, a within-speech dissociation.

The question here is essentially *What is a system?* and becomes one of granularity: At what grain size does a dissociation or difference indicate distinct systems, as opposed to components within a system (see also Folkins et al., 1995)? Ziegler (2003b) argues that although macroscopically there may appear to be overlap in behaviour and neural substrates underlying speech and nonspeech oral motor behaviours, this is merely a matter of low resolution: a more microscopic view reveals differences.[8] However, it is not clear why a broad (and ill-defined) concept such as "speech" is the right grain size of microscopic resolution. Why do differences within speech tasks (an even higher resolution) *not* lead to stipulation of multiple speech motor control systems? Surely, this cannot be based on current methodological limitations (e.g. spatial resolution of neuroimaging techniques) but requires a principled justification.

According to the TDM, control processes are organised around task goals (Bunton, 2008; Weismer, 2006; Ziegler, 2003a). Thus, one might argue that high- and low-frequency syllables, or fast and slow speaking rates, share a similar goal (e.g. to produce an acoustic signal to communicate). However, this solution hinges on a rather vague definition of "goal" (see previous section) because at a finer grain size there are numerous differences in goals between tasks that ostensibly constitute speech. For example, the motor goals for syllables with fricatives are different from those for syllables with stops. Shaiman and Gracco (2002) reported that the compensatory response to unexpected perturbations differed depending on the target consonant, supporting the notion of task-specific functional synergies at a finer grain size. As another example, the task of consciously controlling speaking rate has a different goal than speaking for the purpose of communication, and may result in qualitative differences (e.g. Adams, Weismer, & Kent, 1993; Van Lancker Sidtis et al., 2012). Finally, recent neuroimaging research indicates that planning of syllable structure and planning of syllable sequences rely in part on distinct neural regions (Bohland & Guenther, 2006; see also Ziegler et al., 1997), that vowels and consonants, and different types of consonants, have different neural representations (Bouchard et al., 2013), and that high-frequency and novel syllables recruit different neural circuitry (Bürki et al., 2015). Thus, each sound, in each context, has a different goal, or represents a different task. A radical consequence of TDM logic would be that each sound in each context – each utterance – is controlled by a different motor system, resulting in a potentially infinite number of systems (Gick & Stavness, 2013; Riecker et al., 2005). Yet all such motor actions are nevertheless considered part of a single speech motor network (e.g. Ziegler & Ackermann, 2013). If motor control systems are defined by common goals, then one must define these common goals. Why are vowels and consonants, or fricatives and stops, or high- and low-frequency syllables, or speaking at a habitual versus maximal rate, controlled by one system (despite many differences in goals, kinematic patterns, acoustic consequences, neural underpinnings), and shushing someone or DDK tasks by a fundamentally distinct system? The delineation of a system appears to depend on the granularity of the definition of "goal".

Another way to define a system might be to consider *mechanisms* that encompass a range of (micro-level) goals. For example, in some recent models (e.g. Guenther et al., 2006), vowels and consonants are produced by the same mechanisms (feedforward and feedback control), but the exact combination and micro-level goals may vary by target sound (e.g. greater

[8]Ziegler (2003b) wrote: 'Thus, macroscopically overlapping functions are, on closer examination, broken up into specialised and segregated functions which are optimally tuned to their behavioural goals.' (p. 101), and 'At a low level of resolution the usual suspects, motor cortex, basal ganglia, cerebellum, and brainstem nuclei are implicated in most if not all of the behaviours at stake […]. Yet, at a higher level of resolution, the neural networks controlling motor functions turn out to be organised in a task-specific manner […]' (p. 102).

contribution of feedforward control for rapid consonant gestures). Thus, a TDM might define goals at a grain size larger than individual sounds or syllables. Even here however, the different components (feedforward and feedback systems), each associated with different neural circuitry, could dissociate (see Maas, Mailend, & Guenther, 2015, for a single dissociation in AOS; Smith & Shadmehr, 2005, for double dissociation in limb motor function).

The point here is that it is not clear how and where, at what grain size, to draw a line between different systems, and where to reject such lines despite differences and dissociations in kinematic, neural, or other aspects. If some dissociations merely reflect different *components* (or *strategies,* Adams et al., 1993; or *modes,* Tasko & McClean, 2004) within a single control system (e.g. fricatives vs. stops, frequent vs. infrequent syllables, habitual vs. maximal rate), then two implications follow. First, dissociations are compatible with the IM, according to which speech is decomposable and dissociations are best understood in terms of task properties. Second, the notion of a separate speech system does not rest on the logic and presence of dissociations, but on *stipulation* of tasks as reflecting speech or nonspeech. That is, the TDM disregards dissociations within “speech” tasks as evidence for distinct systems, yet assumes that dissociations with tasks designated as “nonspeech” indicate distinct control systems, even when those tasks share many properties with speech, such as DDK tasks (see also Ballard et al., 2003).

In short, dissociations and differences between tasks exist, but they do not require postulation of distinct motor control systems, nor that such a distinction is best cast in terms of a (poorly defined) speech/nonspeech difference. To make this case using the dissociation method, one first needs an explicit, consistent, and principled definition of *speech*, and a principled approach for deciding which dissociations matter. To my knowledge, no such definition or criteria have (yet) been articulated that can address the complications discussed above. Once such a definition and criteria are available, it will become possible to identify the neural regions involved in speech versus nonspeech tasks, and perhaps even to induce double dissociations with virtual neural lesions (e.g. with transcranial magnetic stimulation). Even in that case however, proper controls are needed to rule out that the dissociation is indeed best characterised as a categorical speech/nonspeech task distinction rather than as a task property distinction.

Emergence of dedicated systems

An important theoretical issue relevant to this debate relates to the emergence of a dedicated speech system, which has been argued to reside in the massive overlearning of speech skill (Bunton, 2008; Ziegler, 2003a,b; Ziegler & Ackermann, 2013). However, this raises a number of heretofore relatively ignored questions about how people acquire speech motor skill: Which system do speakers use before they reach the level of skill (in either a native or foreign language)? Initial attempts at speaking must be supported by a different motor control system.[9] How much experience or practice triggers emergence of the speech motor system, given that speech motor control develops over a protracted period (Hoit, Hixon,

[9]I assume that initial attempts at speech are in fact considered speech. If the system that supports initial attempts is the speech system, then the origin of this system cannot be based on experience-dependent plasticity.

Watson, & Morgan, 1990; Smith & Zelaznik, 2004)? Which system controls speech at intermediate skill levels? Are novel or infrequent syllables (which by definition have not been overlearned) controlled by a novel volitional motor control system? Can there be experience-dependent improvements in skill *within* a system? If so, this would obviate the need to postulate a shift toward a fundamentally distinct system. It is well-established that increases in skill are associated with changes in underlying neural substrates (e.g. Kleim et al., 1998; Sakai et al., 2004). However, such findings do not necessitate postulation of a distinct system, or at least require criteria to distinguish between- versus within-system changes. These issues must be addressed if the notion of experience-dependent plasticity is to have explanatory value for the TDM.

Reference to principles of practice-dependent neural plasticity derived from research on motor skill learning (Bunton, 2008; Ziegler & Ackermann, 2013) implies a belief that speech motor control shares fundamental organising principles with other motor skills, rather than speech motor control being subject to its own unique organising principles. Of note, many of the ideas in current models of speech motor control are similar to, or derived from, nonspeech motor domains, and thus provide continuity with a wider scientific literature (Grimme et al., 2011; Hickok, 2014). For instance, the notions of a hybrid feedforward/feedback control architecture, internal models, motor planning in sensory space, competitive queuing for sequencing actions, self-organisation via a babbling stage (Bohland, Bullock, & Guenther, 2010; Guenther et al., 2006; Hickok et al., 2011) are not specific to speech but derive from the motor control literature (Bullock, 2004; Bullock, Grossberg, & Guenther, 1993; Wolpert, Ghahramani, & Flanagan, 2001). Similarly, contrary to claims in the literature (Ziegler, 2003b),[10] motor equivalence and trading relations are not speech-specific phenomena (Todorov & Jordan, 2002), nor are coarticulation (Jordan, 1990), rhythmic organisation of sequential movements (Sakai et al., 2004), or the notion of content-specific motor "chunks" that develop with practice (Sakai et al., 2004; Sternberg et al., 1978; Verwey, 1996).[11]

Of course, the fact that speech motor control may share principles with nonspeech motor control does not mean that speech and nonspeech motor control rely on the same system or overlapping systems. Stronger tests of whether speech and nonspeech tasks rely in part on shared control systems would require demonstration of an influence of one task on another, for example dual-task interference (e.g. Bailey & Dromey, 2015), priming/facilitation of one task by another, or transfer of learning across tasks (Bunton, 2008; Weismer, 2006).[12] The logic behind the transfer-of-learning approach is that transfer would indicate improvement in some common, shared task component. For example, treatment of speech sounds can

[10]In discussing adaptive trading relations in producing rounded back vowels, Ziegler (2003b: 101) states: 'Co-ordination here is clearly in the service of producing intelligible speech. […] the described organisational principle is speech-specific and is not useful for any other behaviour.' I argue that the *organisational principle* is not speech-specific, only its *application* to a specific-speech pattern (rounded back vowel).

[11]Hickok (2014: 53): 'To a first approximation, what may primarily distinguish between domains then – what distinguishes a linguistic system from a manual control system – is the representational bits that are plugged into those computational architectures.'

[12]Although practice on a nonspeech task is unlikely to produce changes in speech intelligibility (e.g. Bunton, 2008), for proof-of-concept of an integrative system, it would be sufficient to show a change in (for example) a particular kinematic or acoustic parameter observed in a speech task, following practice on that parameter embedded in a nonspeech task. For example, does practice of a particular rhythmic pattern in the context of a human beatbox task result in greater accuracy/stability of that same rhythmic pattern in a speech task (e.g. sentence repetition)?

transfer to other instances of those sounds in untrained utterances, and to other similar speech sounds (e.g. Ballard et al., 2007). However, lack of transfer does not necessarily mean that the tasks rely on fundamentally distinct control systems, unless one accepts the notion of multiple speech control systems, because treatment of speech sounds does not transfer to all other speech sounds (e.g. Ballard et al., 2007, showed transfer was constrained by manner class), or in some cases even to the same sound in different contexts (e.g. Rochet-Capellan et al., 2012).

On the whole, evidence of nonspeech-speech task influences is limited one way or the other, and as argued above, this enterprise requires clear and consistent task definitions. One interesting hypothetical example was offered by Aichert and Ziegler (2013), who argued that overlearned nonverbal expressions (e.g. *mmm*, *shhh*) 'can perhaps be used as overlearned oral movements to facilitate consonantal gestures' (p. 1194). This suggests that transfer from nonspeech tasks to speech may be possible (in essence an endorsement of the IM), although no mechanisms for such transfer are articulated, nor easily conceived, for a TDM.

IMPLICATIONS

This debate has clear theoretical interest. However, there are also practical implications that follow from each view and one's definition of speech. In a way, this debate is about the kinds of generalisations we can make (Tasko & McClean, 2004), and how to study speech motor control. Below I discuss some methodological and clinical implications.

Methodological Implications

Even if there is a speech system that developed for, and is primarily used for, producing "typical" communicative speech, a legitimate question is whether speakers engage such a system in tasks that deviate in some respects from typical speech – and thus, whether we can study this system with tasks that are not typical communicative speech. Can or do people use (parts of) this system to perform other oral motor tasks, such as producing syllable-sized sounds with the vocal tract, with or without communicative function (e.g. *m-hm*; DDK)?

Proponents of a TDM express skepticism in this regard (e.g. Bunton, 2008; Weismer, 2006; Ziegler & Ackermann, 2013).[13] The question *Why not just study speech?* has been posed multiple times in response to the potential infinite regress of making nonspeech tasks speech-like (Bunton, 2008; Weismer, 2006). Although intended rhetorically, the question presumes that we know what speech is, and what it is not. As argued above, it is not clear that we do. Thus, a reasonable answer is *Because we do not know what to study, or how*. A boundary must be defined to establish tasks and methods from which generalisations about speech can be made.

What is the legitimate object of study? Naturalistic conversational speech is an obvious option (see Staiger & Ziegler, 2008, for an excellent example). But limiting study to naturalistic conversation restricts options for controlled experimentation (Xu, 2010). Is any

[13]'There is no other natural motor activity except speech and song which utilizes the specific layout of this neural circuitry, and it is also *hard to imagine any artificially designed nonspeech assessment or training task in the clinic which would specifically engage this particular network*.' (Ziegler & Ackermann, 2013: 59; italics mine).

experimentation a sufficient departure from typical speech to engage a fundamentally different system, and thus uninformative about *speech* motor control? What is the guiding principle that delineates speech from nonspeech motor control? If the goal is to fully understand speech motor control, then some experimentation will be required, which may involve tasks that some might consider nonspeech.

Although perhaps somewhat tongue-in-cheek, this is not a trivial issue, because much of what *we think* we know about speech motor control and its neural underpinnings comes from tasks that are very different from naturalistic conversational speech. For instance, articulating words to a computer in response to pictures or written words lacks communicative intent (even a conversational partner). If this is not speech, then a large body of research on speech motor control and its disorders must be rejected as fundamentally uninformative about speech production. The literature on behavioural and neural aspects of speech motor control has relied extensively on tasks involving production small sets of phrases or words – or nonwords – elicited through picture naming (Maas, Gutiérrez, & Ballard, 2014; Mailend & Maas, 2013; Wunderlich & Ziegler, 2011), imitation of auditory models (Aichert & Ziegler, 2004; Kim, Weismer, Kent, & Duffy, 2009; Smith & Zelaznik, 2004; Ziegler, 2002), reading (Bunton & Weismer, 1994; Tsao & Weismer, 1997), memory recall (Bohland & Guenther, 2006; Cholin et al., 2011; Deger & Ziegler, 2002; Maas, Robin, Wright, & Ballard, 2008; Sternberg et al., 1978), or rapid shadowing (Peschke, Ziegler, Kappes, & Baumgaertner, 2009). Some experimental paradigms to study speech motor control involve learning novel, non-native sound sequences (Moser et al., 2009; Segawa et al., 2015). In all these cases, the task is explicitly *not* to communicate but rather to produce the sound sequences requested (sometimes modeled) by the examiner. Do such tasks engage the speech motor control system or a novel oral motor control system? That is, can we draw conclusions about speech motor control from such tasks (cf. Staiger & Ziegler, 2008)?

Moreover, experimental tasks often involve instructions or demands that deviate from typical speaking situations, such as speaking with a bite block or transducer (Bunton & Weismer, 1994; Jacks, 2008), with instructions to be clear/loud/slow/fast (Darling & Huber, 2011; Ghosh et al., 2010; Tsao & Weismer, 1997), imitating accents or individuals (McGettigan et al., 2013), speaking with a focus on fast reaction time (Deger & Ziegler, 2002; Mailend & Maas, 2013), speaking with experimentally altered feedback (Houde & Jordan, 1998; Maas et al., 2015; Tremblay, Shiller, & Ostry, 2003; Villacorta, Perkell, & Guenther, 2007), repeating syllables without prosodic modulation in synchrony with a metronome (Riecker et al., 2005), or speaking without sound (Wildgruber et al., 1996, 2001). There may or may not be differences between tasks with and without these demands (Tasko & McClean, 2004), but absence of differences does not imply a shared control system (or that this is the *speech* motor control system) – nor do differences imply that people engage fundamentally different systems.

On the whole, most tasks used in speech production research are quite removed from their naturalistic communicative context and often involve specific instructions that induce a task goal different from typical speech. If such tasks engage different oral motor control systems, then they cannot in principle elucidate speech motor control. The rather sobering message in this case would be that we know very little about *speech* production at all. All current models of speech motor control are built on data from tasks that may not qualify as speech, and such models may therefore be considered models of nonspeech oral motor behaviour.

To be fair, proponents of a TDM utilise, and draw inferences about speech motor control from, decontextualised tasks (Bunton, 2008; Bunton & Weismer, 1994; Deger & Ziegler, 2002; Tsao & Weismer, 1997; Wildgruber et al., 2001; Ziegler, 2002), suggesting that such tasks are in fact considered speech (although the relation to conversational speech is rarely addressed; see Tasko & McClean, 2004, and Staiger & Ziegler, 2008, for exceptions). However, notice that this implies acceptance of the decomposability of speech: communicative intent, semantic meaning, or acoustic signal are not necessary; maximal rate tasks and repetitive syllable production tasks can still be speech, etc.. If such deviations from conversational speech are insufficient to posit separate control systems, then why are other tasks that involve some but not all components of typical speech, such as DDK, designated 'nonspeech' (Bunton, 2008: 275; Ziegler, 2003: 20) or 'quasi-speech' (Weismer, 2006: 319)? Again, the distinction appears arbitrary and inconsistent.

Considering speech to involve a specialised system a priori may limit exploration of potentially relevant generalisations. As an example, Peter and Stoel-Gammon (2008) hypothesised that childhood apraxia of speech (CAS) might involve a central underlying timing deficit. They reported similar timing difficulties in matched speech and nonspeech (manual) tasks in children with CAS. Furthermore, timing accuracy was negatively correlated with the number of CAS diagnostic features. Although such correlational designs are suggestive rather than definitive, the point is that such possible generalised impairments may not come to light unless one looks for them beyond a predetermined narrow (ill-defined) task range.

In short, stipulation of ill-defined and inconsistent task categories complicates empirical study, as it is not clear which tasks are appropriate to study speech without veering into nonspeech territory, and may limit exploration of common underlying mechanisms. In contrast, the IM suggests that by examining systematic differences *and similarities* between a range of tasks with similar properties (regardless of whether they are designated "speech" tasks), we may begin to fully understand the many facets of speech motor control (Ballard et al., 2003, 2009; Tasko & McClean, 2004). That is, we ought to study both the parts and their interaction within the whole, in various combinations (including "typical" speech).

Clinical Implications

Assessment—The two claims embodied in the TDM also have important clinical implications, both for assessment and for treatment. Regarding assessment, the TDM implies that no useful information about a motor speech impairment can be derived from using nonspeech tasks such as visuomotor tracking or DDK (Ziegler, 2002, 2003a), as such tasks engage a different oral motor system. Proponents of a TDM do not deny the potential diagnostic value of tasks such as DDK for neurological purposes (e.g. cranial nerve examinations; Ziegler, 2003a), but rather claim that such tasks do not have value for diagnosis or understanding of *speech* impairments (Weismer, 2006; Ziegler, 2002). In other words, whatever function is affected by damage to such neural tissue (e.g. timing), this function is not relevant in the context of a speech task. According to the TDM, there is no overlap between the system that controls conversational speech and the system that controls articulation of speech sound sequences in a DDK task. In contrast, the IM suggests that carefully designed tasks with shared properties (e.g. DDK) can shed light on the nature of motor speech impairments, by examining the abilities and limitations of the oral motor system independent from linguistic input to this system (Ballard et al., 2009).

Interestingly, DDK tasks are common in assessment protocols for motor speech disorders (Duffy, 2005; Thoonen et al., 1999). In addition, much research continues to be conducted on DDK tasks (Hurkmans et al., 2012; Icht & Ben-David, 2014). This may reflect in part 'political considerations' (e.g. the ease with which such tasks can be studied; Weismer, 2006: 343), but often also a belief that such tasks are informative about speech (Riecker et al., 2005). They allow for systematic, controlled manipulation of complexity (Hurkmans et al., 2012) and relatively language-independent assessment of articulation abilities (Icht & Ben-David, 2014), which may be important when assessing bilingual speakers or making cross-linguistic comparisons.

As an example, DDK tasks may be informative about the source of slowed speech rate (e.g. Wang, Kent, Duffy, Thomas, & Weismer, 2004). In comparing alternating motion rate (AMR) and conversational speech rate in speakers with dysarthria, Wang et al. (2004: 79) noted that 'For more severe subjects, the AMR syllable rate was quite similar to conversational syllable rate, perhaps indicating that *speech motor* capability was the limiting factor' (italics mine). This quote suggests that the DDK task *does* capture some shared aspect, and that conversational speech rate is slowed because of speech motor control limitations rather than (for example) cognitive or linguistic limitations. If one were to only examine conversational speech rate, such alternative possible sources of slowing would be more difficult to disentangle.

Empirically, there is support for the utility of DDK tasks in differential diagnosis of speech disorders, in particular with respect to CAS. For example, to date the only prospectively validated diagnostic marker with adequate diagnostic sensitivity and specificity is a score derived from maximal performance tasks (Thoonen et al., 1999). Murray et al. (2015) recently showed that CAS can be differentiated with high accuracy from other pediatric speech disorders using four measures obtained from two tasks, one of which a DDK task. Thus, DDK tasks emerge across studies as among the most discriminative. From the TDM perspective, the interpretation would be that CAS also involves impairment of nonspeech oral motor control, which has nothing to do with their speech impairment – and therefore cannot be used as part of the justification for (particular) clinical services. In contrast, from the IM perspective this finding might suggest that the speech difficulties in CAS also surface in DDK tasks, and performance on these tasks may help make the case for specific interventions for CAS.[14]

[14]To be clear, I am not advocating for relying exclusively on DDK-type tasks (or on any other single task) in assessment and diagnosis of speech disorders. See also Ballard et al. (2000: 979–980): '*Although it is necessary to consider the impairment of AOS in the context of speech production tasks*, also studying nonspeech behaviours has the potential to disambiguate which characteristics are a result of the underlying motor impairment and which are related to the interaction between the motor and linguistic systems.' (italics mine)

More generally, the strong claims embodied by the TDM require criteria that delineate "speech" to devise an assessment protocol with tasks that allow conclusions about speech impairments. The issues above are relevant in the clinical context as well: Is communicative intent necessary? Are imitative tasks sufficiently speech-like? Is production of nonwords informative about speech? Do instructions to alter rate change the task into a nonspeech task? These questions illustrate that each theoretical perspective has important implications for assessment, and that indeed 'the details make all the difference' (Weismer, 2006: 315).

Treatment—Similar considerations arise for treatment. For example, if nonwords are not speech, then treatment for speech disorders should only use real word targets, since no transfer would be expected from nonwords, based on the specificity of learning (Rochet-Capellan et al., 2012; Segawa et al., 2015). Yet some evidence suggests that generalisation from nonwords to real words occurs (Maas et al., 2002; Schneider & Frens, 2005), and may even be greater than targeting real words for some speakers (Gierut, Morrisette, & Ziemer, 2010). Such findings suggest that semantic meaning and communicative intent are not necessary conditions for speech (and thus can be removed for a somewhat decomposed behaviour that is still speech).

In addition, many therapeutic techniques alter the task from typical conversational speech into a more consciously controlled task, such as rate control (Mauszycki & Wambaugh, 2008; Yorkston et al., 2007), focus on loud speech (Ramig et al., 1995), visual models and mirrors (Brendel & Ziegler, 2008; DeThorne et al., 2009), gestural or tactile cues (Brendel & Ziegler, 2008; Dale & Hayden, 2013), imitation of tone sequences (Brendel & Ziegler, 2008), visual biofeedback (Preston et al., 2014), or implicit practice (without overt articulation; Davis, Farias, & Baynes, 2009). Does this mean that individuals operate in a "nonspeech mode" and therefore do not actually engage their speech motor control system? If so, then the justification for such techniques is unclear, because no transfer is expected to actual speech production (despite evidence of such transfer; Brendel & Ziegler, 2008; Davis et al., 2009; Preston et al., 2014). Perhaps the justification is that it does not matter whether we call the behaviour speech, as long as communication improves (by nonspeech means) and we do not expect improvement in speech production. If the goal is to improve speech production with treatment, and one stipulates that speech is a categorically distinct behaviour controlled by a separate system, then the question is what range of tasks and techniques can be considered legitimate and appropriate for this purpose.

Importantly, the foregoing discussion should not be construed as an endorsement of so-called nonspeech oral motor exercises (e.g. tongue push-ups) to improve speech production. There are many good arguments against this practice (Clark, 2003; McCauley et al., 2009), and rejection of such practice does not require the assumption that speech is controlled by a separate motor control system, or that speech is holistic. Nonspeech oral motor exercises to improve speech function are contraindicated (in most cases) by both views, contrary to occasional suggestions otherwise (Ziegler & Ackermann, 2013). Although the IM predicts that transfer between some nonspeech oral motor tasks and some aspects of speech production may occur, this view still predicts greater transfer from actual speech to speech, given the specificity of learning (Rochet-Capellan al., 2012). While some have argued that nonspeech motor behaviours may be a necessary precursor to speech treatment in some cases (Robin, 1992), this does not necessarily follow from an IM. The claim that speech may share properties with other motor behaviours does not imply that practice on *any* such motor behaviour will therefore necessarily benefit speech production, much less that any such benefits would be *greater than* or *equal to* benefits from practising speech movements. The IM does not claim that a given nonspeech task uses *all* or *only those* components involved in speech production or vice versa. In fact, the central claim is that there is more or less overlap, depending on the degree of similarity between tasks. As such, greater transfer is expected from speech to speech than from nonspeech to speech – because of overlapping or shared properties, not because speech and nonspeech are controlled by categorically distinct systems.

CONCLUSIONS

Most researchers agree that speech is a special skill and that nonspeech oral motor exercises to improve speech production are contraindicated in clinical treatment. However, disagreement exists about whether or not a distinct, dedicated motor control system underlies speech production and whether speech is holistic or decomposable into primitives. A common view in the literature is the TDM, which holds that speech is holistic, categorically different from all other oral motor behaviours, and subserved by a special, separate motor control system.

This article highlighted several major challenges for this view, including the lack of an explicit definition of speech, difficulty delineating speech from nonspeech tasks, and inconsistent application of definitions and criteria. In addition, it was argued that dissociations, among the primary sources of evidence for a TDM, do not require interpretation in terms of distinct motor systems and also exist between speech tasks at a finer resolution, highlighting the lack of principled criteria for interpreting dissociations as within- or between-system differences. Further, several questions were raised surrounding the emergence of a dedicated speech motor control system. These are not trivial challenges, and they must be met for the notion of a distinct, speech-specific control system to be meaningful.

Acknowledging a gradient distinction, with overlapping properties between tasks, is not tantamount to the claim that tasks are the same, or controlled by a completely overlapping system, and does not mean that everything about typical conversational speech can be understood by studying simplified or artificial tasks such as DDK. However, it does amount to rejecting a categorical, discrete boundary and a holistic, indecomposable view of speech. Acknowledging the existence of speech-like behaviours (either explicitly or implicitly by using/endorsing certain experimental tasks to draw inferences about speech) suggests decomposability: Speech can be seen as a combination of properties, which may occur in different combinations in different motor tasks. This is the essence of the IM. Dissociations and differences may best be understood in terms of these properties rather than a stipulated, ill-defined distinction between speech and nonspeech. Our understanding of speech motor control, and motor control in general, may be enhanced if we can identify those properties, for example by comparing tasks with and without these properties (e.g. rate requirements, communicative intent; Ballard et al., 2003; Bunton & Weismer, 1994). There may be more agreement than is apparent in the literature, at least when examining the range of tasks used or cited to support a TDM, which include tasks that depart significantly from naturalistic communicative speech (e.g. without communicative intent, semantic content, syntactic structure, or even an acoustic signal).

This philosophical debate has methodological and clinical implications. If one defines speech as including only conversational speech for the *task* of communicating, then our methods and knowledge of speech motor control and its disorders are very limited. To the extent that clinicians and researchers rely on methods that deviate from conversational speech (e.g. word repetition, reading out loud, covert articulation, rate reduction techniques, visual biofeedback, shaping consonantal gestures from "nonspeech" gestures, DDK tasks), this either implies some degree of decomposability of speech or acceptance of multiple "speech" motor control system, thus undermining the foundation of the TDM. Of course, regardless of whether departures from typical speaking situations in experimental or clinical investigations reflect the operation and processes of "the" speech motor control system or an integrative system, a clear justification for the use and interpretation of such task is needed. Finally, Weismer (2006: 331) wrote 'In the absence of a theoretically motivated, clear criterion of when a task is sufficiently speech-like to qualify as representative of control processes in speech production, the concept of "control overlap" has limited scientific, and hence clinical, utility'. I agree, and would add that the same holds for a TDM: In the absence of a theoretically motivated, clear criterion of when a task is sufficiently speech-like to qualify as representative of control processes in speech production, the concept of "task-specific motor control" has limited scientific, and hence clinical, utility.

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