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## Book review

### **The Phonological Enterprise, Mark Hale, Charles Reiss. Oxford University Press, Oxford, (2008)**

As the title suggests this is a programmatic book, promising to discuss the big picture in the field of phonology. What is the empirical scope of phonology? What do phonologists set out to explain? What should a (phonological) theory look like, i.e., by which criteria do we measure theoretical explanations? It keeps this promise by discussing all these issues and thus has to be regarded as one of these rare books that merit highest praise.

The book does a very good job in addressing some of the most fundamental questions that have to be discussed. The authors' conclusions range from stating the plain obvious via thought provoking to premature. The reader already familiar with Hale's and Reiss' work should not expect any surprises, since the book is by and large a compilation of previously published work by the authors.

In the following I will first give an overview and summary of the book and then move on to discuss a few of Hale & Reiss' positions in more detail. In this latter part I will concentrate most on the points which I regard as premature conclusions or simply plain wrong, which does not mean that I disagree with the authors on all fronts, as I hope will become clear already in the summary.

The main body of the book is divided into four major parts, of which the first argues for an approach to acquisition and a resulting conception of Universal Grammar (UG) and phonological analysis in particular that is different from current mainstream thinking in substantial respects, which I will discuss further below. The second part is concerned with the contemporary trend of functional motivation or grounding of phonological principles and categories and argues that this methodology is fundamentally misguided. Neither phonetic nor typological grounding should be regarded as evidence for theoretical argumentation in phonology since both find adequate explanation outside the language faculty. The same holds for frequency effects, which are also explainable by extra-linguistic factors rather than grammar.

The third part is basically a polemic against Optimality Theory (though Feature Geometry and the OCP get their fair share as well). Though, as they state at the beginning of part four (p. 257), their "target is not a particular theory of phonology, but a particular *practice*." The final part of the book reveals a few more details of Hale & Reiss' approach to phonology.

Hale & Reiss see phonology as an abstract module of computation and representations. The interface with the physical world is cared for by what they call a transducer. A transducer maps abstract phonological representations, the output of the phonological computation, to muscular activity, or, say, the neurological equivalent of commands that effect physical activity resulting in sound production. In the reverse, parsing of language, the transducer maps physical/acoustic events to phonological representations, that then can be used in the phonological computation.

Since phonology is an abstract module functional considerations such as ease of articulation, perceptual biases, etc. are a priori excluded as motivations for phonological representational primitives or grammatical mechanisms, such as positional or paradigmatic faithfulness. Anything that can be motivated on extra-linguistic grounds has to be excluded from the scope of phonological explanation. From the same line of reasoning it follows that production data from language acquisition, typological frequency effects, typological implicational relations (such as 'every language that has fricatives also has stops') and typological observations in general are neither motivation for aspects of phonological theories nor does typology pose the limit to the structures and patterns a grammar should be able to generate. Because of these extralinguistic limits of typological observation Hale & Reiss also do not accept typological data, and neither phonetic data, as evidence for a theory of markedness. Hale & Reiss reject Optimality Theory's (OT)

Richness of the Base Hypothesis (RotB) and redefine the subset problem on similar grounds. The RotB, a central axiom of OT, can be seen as a guideline for theory construction. An OT grammar assumed for a language  $x$  should only be capable of producing those outputs that are legal in this language, regardless of – and this is important – which input is fed into the grammar. Ultimately, however, the hypothesis is relevant as the major evaluation metric for theory construction, i.e., falsifiability. Hale & Reiss bring up two arguments against this hypothesis. First, a speaker of language  $x$  is never confronted with highly deviant inputs. In the cases s/he has to deal with non-language-conform inputs s/he deals with loanwords and very often languages have developed strategies to deal with loanwords that are different to their core grammar. The second argument against RotB is performance. Structures that are alien to a language are difficult to perceive and to produce because they are not practised.

This is not the only aspect of OT they criticise. By and large they spent a lot of space arguing against OT rather than for their proposal. Their general claim that grammars should not be collections of statements of what is not allowed is dressed as an argument against OT constraints (though, for completeness' sake, they argue against the most widely accepted pre- or non-OT constraint, the Obligatory Contour Principle, as well). The problem with negative statements, as conceived by Hale & Reiss, is that, once you get started, you should be exhaustive, which leads to absurd constraints, such as their favourite NoBANANA (roughly: 'linguistic expressions don't contain real bananas.').

After brushing over phonology as it should not be and what it should not be about according to Hale & Reiss I try a synopsis of what phonology should be about and what it should look like in their view.

Hale & Reiss argue that for a learner to be able to learn anything at all s/he has to have a set of categories available to start with. Thus, they assume that all phonological features are universally present from the beginning of the learning process, that is, they are innate (as opposed to learned or maturing). Children start out with full specifications and have to decide in the course of the learning process which features are underspecified in which segments and which features can be discarded altogether for the language they are acquiring. The second assumption, though, does not necessarily follow from the first. As far as I can see, a learner could likewise be equipped with a universal set of features and use them when the need arises, i.e., when she finds evidence in the input.<sup>1</sup>

Related to learning is also the conception of phonological rules. According to Hale & Reiss a learner postulates rules that are as restrictive as possible. This is in stark contrast to the generally held view on phonological rules. These are usually formulated in the most economic fashion, thus, restricting the use of features in a rule to a minimum, thereby maximising its generality. So, if a learner detects an alternation like final devoicing in a German word like *Lob* 'praise', the general way of postulating the rule would produce a rule that refers to all segments contrastively specified for the laryngeal feature. Hale & Reiss' mechanism instead restricts the scope of the rule to underlyingly voiced labial stops. In summary, phonological representations and phonological rules should be as specific as possible. The claim about the scope of phonological rules should be easily testable by examination of overgeneralisations in child speech. In general one can say that, if children really postulated rules with the smallest scope possible a typical child language phenomenon should be undergeneralisation rather than overgeneralisation.

Since Hale & Reiss reject most traditional types of evidence and postulate that typological observations should not be the evidence that restricts the desired scope of a phonological theory the question arises in which ways phonology actually is restricted. Hale & Reiss formulate a distinction between attested phonological grammars, attestable grammars, processible/transducible/acquirable grammars, computable grammars and finally statable grammars. The attested grammars are of course a subset of the attestable grammars. This latter set is the intersection of the processible/etc. set and the computable set, which are both subsets of the set of statable grammars.

I now outline briefly what Hale & Reiss understand each of these sets to be and which of them is the realm of phonological theory. Attested grammars are trivially those grammars we know, i.e., that of English or Pashto, while attestable grammars are grammars for which we do not have any records, the grammars of languages that are no longer spoken or future states of currently existing languages, future languages or existing languages that have not been described and analysed yet, for example the current author's idiolect of English (assuming there is no systematic description of 'Krämerean' English yet).

The set of humanly computable grammars includes grammars that might be unattestable for extra-linguistic reasons. Hale & Reiss' favourite phonological rule ( $p$  is realised as  $s$  before  $r$ ) is such a case. The rule is not attested

<sup>1</sup> To illustrate that think of what you do when you want to have food and then decide to fry some eggs. Most people would not get all the food out of the fridge first and then pick the eggs and put the rest back into the fridge or throw it away.

and it might never be found in any language. However, Hale & Reiss assume that this is the case for extralinguistic reasons, such as the nature of acquisition or of language change. Though, it does not become clear why these factors should exclude this rule. First, it looks perfectly learnable. On the other hand there are many languages that show an alternation between /p/ and [h]. Given current assumptions of especially rule-based phonological theories, a grammar that changes /p/ to /f/ in intersonorant contexts and subsequently neutralises the place of articulation to coronal would not be too surprising. Given Hale & Reiss' view on language acquisition children could just detect the relevant alternation in the context before /r/ first and generalise the rule. Incomplete acquisition (children simply ignore the rest of the data) then leads to the rule being established in the grammar.

On the other hand they give as an example for a storable but not humanly computable rule the lengthening of vowels in prime-numbered syllables. The reason why this should not be computable is not really clear to me. Hale & Reiss' prime argument here is that languages usually do not refer to numbers. This sounds suspiciously like a typological argument. However, one could easily characterise the stress placement algorithm of some languages as referring to odd numbered syllables or moras. One such algorithm places word stress on the last odd-numbered mora, counting from the beginning of the word, that is not in the final syllable (which could describe Cairene Arabic stress placement, see among others Halle and Vergnaud, 1987 for a discussion of Cairene Arabic). So, if grammars can refer to uneven numbers, why not prime numbers? The reason that numbers are usually not assumed to play a role in grammars is Occam's razor (do not introduce more machinery if you can state the same generalisation by other means that are independently motivated). Thus, reference to prime numbers is perfectly computable in my view, but there might be extralinguistic reasons or other aspects of grammar responsible for the absence of such patterns from the languages we know. This brief discussion already gave us a foretaste that Hale & Reiss' arguments are very inspiring, but not always exhaustively discussed.

The domain that should be covered by UG is the set of humanly computable grammars. This set, as just said, potentially includes a large number of unattestable grammars. Hale & Reiss give three arguments for this. Their first argument is the human anatomy and choice of channel for language transmission (i.e., audio-transmission in most cases). Current humans are perfectly capable of perceiving high frequency noise. If the frequency range would by some accident be reduced significantly no language could have fricatives. This hypothetical absence of fricatives, however, does not imply that UG should not contain the means to contrast fricatives. One could likewise turn this around and contemplate the option that the current frequency range limitations to human perception prevent us from using contrasts our language faculty can encode.

Their second argument is a variation on the first, relating to the limits of human memory, life span and attention span. It is generally accepted that human grammars can potentially generate endless sentences. The reasons there is no attested endless sentence lie outside the language faculty. We have only a limited life span, people have a limited attention span, which prevents us from actually parsing too long sentences, etc. Arguably, this argument can be extended to other forms of complexity. Short-term memory limits could simply be the reason for the absence of structures that exceed a certain level of complexity (see Golston, 2007 and references therein).

The third argument Hale & Reiss invoke is slightly more subtle. Linguistic theories (and other theories as well) have a tendency to contain elements that don't have any empirical effects. Their example is Halle and Idsardi's (1995) theory of stress computation, which gives you the option to insert the right edge of a foot at the left edge of a domain and vice versa. While such foot edges have an effect on the placement of stress domain-internally, their presence or absence at the respective opposite domain edge does not make a difference. Removing this option from the theory, however, would result in cumbersome statements that make the theory unnecessarily more complicated.<sup>2</sup>

In the remainder of this review I will discuss those of Hale & Reiss' claims in more detail that struck me as most controversial.

1. Production data from first language acquisition are largely showing us something about performance and not competence, while perception data are revealing children's competence.
2. Implicational relations between segments in language typology (which follow more or less the same pattern as the order of acquisition) are a matter of performance.
3. Functional grounding is discussing performance issues and should not be used to motivate grammatical principles.

<sup>2</sup> In this particular case, though, the allegedly effectless diacritic could have an impact on the organisation at the phrasal level.

4. Mostly because of 1, 2 and 3, markedness is not a matter of phonology.
5. Richness of the Base is irrelevant.
6. Explicit restrictions (or constraints) that outlaw impossible representations are to be rejected as part of grammar.

In the discussion of the theoretical relevance of production data in language acquisition Hale & Reiss argue that phenomena like the *fis-fish* stories (children produce the wrong sound but object against adults doing the same) show that children get the underlying representations right from early on but just cannot produce the right sound as a correspondent to a particular input. The argument usually held is that such patterns show that children have acquired a contrast, but the grammar's mapping mechanism lags behind. A similar argument is given for chain shifts, also known as the puzzle-puddle-pickle problem. Children map the /z/ in *puzzle* to a [d] and the /d/ in *puddle* to a [g] (/pʌzəl/ → [pʌdl];/pʌdl/ → [pʌgl]). Such data show that children do not have articulatory problems with the target output. Hence, the phonology must be responsible. Hale & Reiss acknowledge that such children do not have difficulties articulating the structure in question, but it can still be extra-grammatical factors which lead to the non-target-like output. Children are capable of producing [dl], but they are not capable yet to produce it as a target, that is, when they should. The question remains open though what these factors are and, approaching the dysfunctionism argument from the other side, in the absence of any extra-grammatical explanation, grammar has to be responsible for these mismatches.

Since Jakobson's (1941) seminal work it is widely accepted that the developmental path of child language acquisition, the order of loss of contrast in aphasia and typological implicational relations between segment classes (of the type 'every language that has x also has y') are by and large parallel, that is the sounds or contrasts that are generally acquired early are also the most common cross-linguistically and the last to be lost in aphasia. This observation is usually taken as evidence for theories of markedness that differentiate segment classes as more marked versus less marked to unmarked. Likewise, however, ease of articulation (or the reverse) is often given as a motivation for relative markedness of segments. For example, it is obvious to most phoneticians and phonologists that vocal fold vibration is difficult to maintain during the closure phase of a stop. Therefore, scholars conclude, voicing is the marked state of stops while the unmarked stop is voiceless. If we invoke Occam's Razor, as Hale & Reiss do, the grammar-external explanation (articulatory difficulty, perception problems) obviates the need for a grammatical explanation.

At this point it is interesting to consider Hale & Reiss' own model again. An important part of this model is the interface between phonology and the world, the transducer. Transducers convert one type of input into a different type. A spectrograph could be taken as an example for a transducer, which converts auditory input into visual output. A spectrograph is very systematic in translating sound into black-and-white pictures with straightforward correspondences between intensity of shading and auditory intensity and between extension of shading on the vertical axis and frequency, etc.<sup>3</sup>

A spectrograph does not actually manipulate the signal itself, it creates a new representation of this signal that is of a different type altogether. In Hale & Reiss' model it is the job of the phonology to turn an entity into something else of the same type, i.e., transform an underlying representation, as it is stored in the lexicon, into a surface representation. This representation is still a phonological object. The transducer creates a physical correspondent of this phonological representation, or rather, instructions for the articulators. Unfortunately, though Hale & Reiss discuss the difference between transduction and computation in elaborate detail, they do not explain how they envisage the transducer to work.

For the transmission process from one speaker to another to actually work one would assume that the transduction process is governed by some principles that make sure that correspondences between phonological objects and physical events are stable for individuals as well as across speakers. Otherwise the recipient would not be able to decode the physical correspondent of a phonological representation into the same or a similar representation, that is, the phonological representation has to be recoverable. Thus, one would expect that complex phonological structures receive a complex physical correspondent and that complexity relations between representations are mirrored by parallel complexity relations between the respective physical correspondents.

<sup>3</sup> Actually, transduction in this case is a multi-step process, since sound waves, movement of air molecules, is converted into an electrical signal, i.e., variations of electrical voltage, which then is converted into a digital code, which then gets converted further until we arrive at the pixel representation. A loudspeaker is a more direct case of transduction. Variations of electrical voltage get converted into air movements via vibration of a membrane.

So, if we have two abstract segments with their entirely abstract phonological features and they differ only in that in segment S1 the feature A is present (or positively specified) and in S2 it is absent (or negatively specified) this represents a state of relative markedness for segment S1.<sup>4</sup> The transducer now has to map this to a different form that in the ideal case reflects this relation between the two segments in a recoverable way. An obvious way to do this is to transfer representational complexity into articulatory complexity or difficulty. In case S1 and S2 are for some reason realised as stops the presence of feature A in the former contrasting with its absence in the latter can be transduced into an additional articulatory gesture or activity for S1 and in the ideal case one that is difficult to execute. Obvious candidates for implementation would be voicing or aspiration. Thus, the phonetic signal mirrors the phonological structure in the same way the spectrograph mirrors the auditory signal via concentration and distribution of black pixels. In conclusion, I fully agree with Hale & Reiss that phonology is not driven by phonetics and I would like to push this claim a bit further: The relationship between the two domains is the reverse: Phonetics is grounded in phonology (see as well Uffmann, 2008).

Under such premises we have to re-examine the claim that functional motivation is misguided. It is misguided, indeed, since the cause–effect relation between phonological structure and phonetics can only be as just described, while functional motivation usually has it the other way round. For, example, languages show only final devoicing and not initial devoicing (synchronously), because, the functional argument goes, before another stop or a pause voicing is badly cued in stops and hard to perceive (in addition to being hard to execute in a stop anyway).<sup>5</sup> The question now arises whether the transducer exploits phonetic facts to appropriately reproduce a phonological neutralisation or whether it is language-specific phonetics we are dealing with here and no phonology involved at all in final devoicing. Such a book review, however, is not the place to do this at an appropriate level. Suffice it to say, there are good reasons to conclude that many of the phonetic facts we observe are phonologically grounded. On the other hand, this does not exclude the possibility that many of the patterns we find are motivated by extra-linguistic factors. We have to carefully distinguish these two types of grounding. In the latter case they are not relevant for phonological theory.

If we accept that phonetics is phonologically grounded, because of the nature of the transducer, however, a theory of markedness becomes a genuine concern of phonological theory again and acquisition as well as typological data does matter.

The next issue I discuss is the role of Richness of the Base in phonological theory. Richness of the Base (RotB) is a core axiom of Optimality Theory (OT), basically stating that all cross-linguistic variation is due to constraint re-ranking. A corollary of this is that any proposed OT grammar, which is a ranking of a set of universal violable constraints, should generate all and only such forms that are regarded as well-formed by the speakers of the language in question, regardless which input is assumed. A second core principle of OT is that all permutations of the universal set of constraints should describe an attested pattern, usually referred to as factorial typology. Hale & Reiss convincingly challenge the latter by pointing to our ignorance about unknown past, contemporary and future languages and language states. Thus, a theory that describes all (computationally) possible languages should actually be preferred over one that describes only attested languages and the principle of factorial typology has to be regarded as too strong. However, we will see in a moment that there is an argument in favour of factorial typology.

The former derivative from the RotB is particularly important in the current discussion since it is directly connected to the subset problem or subset principle, as Hale & Reiss refer to it. The subset principle or problem basically recognizes that a language learner acquires the most restrictive grammar possible. (This task is generally regarded as complicated by two factors I will not further discuss here, the poverty of the stimulus and the lack of negative evidence.) The challenge for both a theory of a particular language or linguistic pattern and for language learning is thus restrictiveness. A widespread argument drawn on in support of the restrictiveness assumption is the way in which native speakers of a given language deal with novel words (e.g., loanwords) that do not correspond to the language's restrictions. Loanwords assimilate over time adapting to native patterns. OT grammars that adhere to the RotB map inputs that do not match the output criteria to some unfaithful representation that contains repairs. Thus, an OT

<sup>4</sup> Considering features it is noteworthy that Hale & Reiss, arguing for substance-free phonology, use substance-laden features, such as [±continuant]. If the feature set is universally available, i.e., part of UG, as Hale & Reiss argue, and substance does not play any role, these features cannot have a phonetic basis.

<sup>5</sup> Note that one of the very few actual examples Hale & Reiss discuss is final devoicing. If they go through with their own argument, though, they would have to dismiss any phonological analysis of final devoicing as unnecessary, since there are extralinguistic explanations available.

grammar represents the linguistic competence of a naive native speaker, excluding extra-linguistic factors such as level of education or linguistic awareness. Hale & Reiss argue that it has been observed in many studies that loanword phonology actually is not that simple and that loanwords are often modified in ways that deviate from the native phonology.<sup>6</sup>

The way in which speakers realise linguistic inputs can be affected by all sorts of non-linguistic factors. A speaker who knows the source language the loanword comes from will undoubtedly have a tendency to produce it according to the way it is realised in the source language, unless of course there are social reasons not to do this (for example the speaker does not want to make the impression of a know-it-all). Still, such a speaker has an intuition about what is a well-formed surface form in her native language and will recognise a loanword as odd on encounter if it has an ‘exotic’ structure even if she does not know the source language. It is this competence that the core grammar should define. The Richness of the Base provides the methodological background to achieve this: For a given language it divides all linguistic representations into two sets, possible outputs and impossible outputs. The argument that a speaker is never exposed to completely alien inputs is simply not relevant here, since we are concerned with the speaker’s actual competence.

The claim by Hale & Reiss that a grammar of language *x* should not exclude deviant representations completely ignores this and their theory thus has to be regarded as incapable of capturing speaker intuitions.

Hale & Reiss challenge OT in two more central points. First, they argue, even though OT claims to have solved the duplication problem, that it has its own duplication problem. The division of labour in a Generator function and an Evaluation function, Hale & Reiss claim, is an unnecessary complication of the theory if a theory will do that just generates representations. Occam’s Razor would prefer the latter. Second, the use of negative constraints that explicitly exclude representations is a bottomless pit. If we explicitly define the impossible rather than to define the possible the set of objects we have to exclude is infinite and accordingly the constraint set has to be infinite, including ridiculous constraints, such as NOBANANA.

What would happen if a real banana were the input to Hale & Reiss’ grammar consisting exclusively of a set of rules? The banana would just be realised, since no rule refers to it and therefore it would not be transformed into something else.<sup>7</sup> Thus, implicitly Hale & Reiss have to make restrictive assumptions on what an input representation can be, that is, we are back to the original duplication problem (or close to it): there have to be constraints on the lexicon or on possible inputs in addition to the rules that transform an input representation into an output representation.

The set of phonological features is taken as a given by Hale & Reiss, that is, they regard features as universal, as part of UG. In addition, UG has to provide a definition of what a segment is and how features can be combined within and across segments. The same holds for any structure above the segment.

Even though Hale & Reiss do not grant it that, OT makes the same assumptions.<sup>8</sup> There are universal primitives which combine to complex representations and universal limits on these combinations.<sup>9</sup> Still, most of these possible representations are ungrammatical in most languages and quite a few are ungrammatical in all languages. Furthermore, given the assumption of a universal set of features and categories we have to assume that the constraint set contains constraints referring to all and only these universal features and categories. Anything that is not built of these features and categories is by definition not a linguistic object and neither subject to generation nor to evaluation. Thus, if we accept a finite set of features and categories together with some basic assumptions on their organisation the

<sup>6</sup> Interestingly, Hale & Reiss take a set of data from Turkish crucially involving loanwords of French origin to argue for their conception of feature specifications. Their argument that the French loanwords should be regarded as nativised is the productive use of Turkish affixes with these words, which is a rather weak argument. Furthermore, arguing against the relevance of loanwords because of their special behaviour and then basing a theoretical argument on loanword data that completely ignores their status as loanwords is like begging for comment.

<sup>7</sup> Of course it would not be realised since we don’t have real bananas in our heads and even if we had we could not produce one with our articulators or have it exit the body via the articulators for obvious non-linguistic reasons. Thus, we could actually assume that no theory of phonology has to prohibit real bananas because there are extra-linguistic reasons not to produce them.

<sup>8</sup> There are different camps within OT. The functionalist school holds that features are learned and that only those features are learned that are needed. The work of Paul Boersma is an example of this position.

<sup>9</sup> Which these are depends on the theory of representations adopted. So, in case of Feature Geometry, the feature tree and the constraint against line crossing, for example, would be universal. Any segment consisting of a deviating tree (for example one with the place feature [labial] associated to a laryngeal feature [spread glottis]) and any string of segments with crossing lines could not even be generated by the Generator function and hence do not need to be excluded by Evaluation.

constraint set is most likely finite as well, while the candidate set is not necessarily so, since candidates can contain recursion.

Finally I would like to come back to the claim that the general evaluation metric for linguistic theories (in fact all theories), corresponding to factorial typology in OT, which holds that a given theory of language should account for all and only the attested languages is an unjustified restriction as well as that the methodological restriction that a particular grammar should generate all and only the forms regarded as well-formed in a given language (by and large corresponding to the Richness of the Base Hypothesis in OT) is unnecessary. Whether an unattested pattern predicted by a theory is unattested because the language faculty, i.e., UG, cannot produce such a pattern or whether its absence from the data set is a historical accident or excluded by extralinguistic factors is an important question, though it is difficult to answer. According to generally accepted principles of scientific reasoning anything assumed or predicted to be possible<sup>10</sup> has to be proven to be so. However, one could of course imagine sophisticated experimentation which shows that human brains respond in different ways to unattested linguistic patterns, dividing such patterns into impossible (or not computable) patterns and patterns that are unattested for extra-linguistic reasons.

The restriction on individual grammars is usually phrased less strictly, acknowledging the existence of lexical gaps as opposed to systematically excluded structures. Such a difference can be tested in various ways, for example with nonce-word experiments. Speakers usually have a clear intuition on what kind of words or sentences just sound ‘bad’ and which are just words they do not know but which could perfectly well be part of their language’s inventory. So, at the level of individual languages we have ways to separate the systematically excluded from the accidentally unattested. Again, though, a theory that just assumes as potential inputs the inventory of morphemes known to speakers, stored as abstract representations built from the universal set of features, and a set of transformation rules that change the given inputs into attested outputs does not do justice to the creative nature of language and especially phonology and it does not account for speaker’s intuitions – though it should.

In summary, despite some shortcomings, this book is to be regarded an important contribution to our understanding of the phonological enterprise since it challenges widespread (mis)beliefs on how the language faculty operates and on methods of theoretical argumentation in linguistics and makes some interesting proposals on what the enterprise of phonology should be. It is to be hoped that it sparks a lively discussion in the field.

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<sup>10</sup> The term ‘possible’ here has to be handled with care, since we are differentiating between ‘possible’ as admitted by the linguistic faculty and actually ‘possible’, with the two sets being intersecting. As argued by Hale & Reiss and accepted here something might be possible in the sense that it is linguistically computable but still impossible for non-linguistic reasons (such as the human anatomy).