Yes, we have NoBanana?: Arguments against Richness of the Base

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Constraint satisfaction approaches have been developed in recent decades for application to problems like solving Sudoku and other puzzles, development of scheduling software, and bin packing and other optimization problems. In each case, constraint satisfaction constitutes an engineering solution applied to an *artifact* created by humans. For science, not engineering, Wolfram (2002, 351) is of the opinion that "whenever there is behavior of significant complexity its most plausible explanation tends to be some explicit process of evolution, not the implicit satisfaction of constraints." If we accept that language is a natural object (Chomsky, 2000), and that linguistics should use the methods of the natural sciences, constraint satisfaction approaches appear to be contraindicated for our field. In light of such foundational concerns, it is useful to re-evaluate the claimed results of constraint-based approaches to linguistics. In this squib, I focus narrowly on Richness of the Base (RotB), which is widely recognized in the literature as a fundamental principle of Optimality Theory:

(1) Richness of the Base (Smolensky, 1996a): The source of all systematic cross-linguistic variation is constraint reranking. In particular, the set of inputs to the grammars of all languages is the same. The grammatical inventories of a language are the outputs which emerge from the grammar when it is fed the universal set of all possible inputs.

RotB is supposed to reflect the idea that the surface inventory (segments and sequences of segments, for example) of a language is fully determined by the constraint ranking. To illustrate in simple terms, if we could somehow feed a morpheme containing a click into an English-type grammar, the constraint ranking would guarantee that the click would never surface, and the optimal candidate would contain, say, [k], a segment which the ranking does allow to surface. The grammar 'knows' to prohibit clicks from surfacing, even though there has been no direct evidence concerning clicks during the acquisition process. This looks like a Poverty of the Stimulus situation: the input alone appears to underdetermine the achieved knowledge state.

One problem with RotB is that it is predicated on a claim that is patently false. Contrary to suggestions in the literature, it is obvious that borrowed

words sometimes *are* adopted with 'new' segments or new phonotactic patterns. In the history of English the [ʒ] sound of *pleasure* and *treasure*, was adopted from French; the initial [ʃm] sequence of *shmuck* and *shmooze* occurs only in recent borrowing from Yiddish (and in the non-standard pronunciation of *smorgasbord* from a Swedish source with [sm]!); and formerly absent initial [sr] sequences occur in contemporary pronunciation of many speakers for *Sri Lanka* and *Sriracha sauce*. The literature is full of hedges like this: "while loanwords do not necessarily respect all the constraints of the receiving language, there is no doubt that the correct generalization is that they are strongly reshaped by those constraints" (Smolensky, 1996a). This just means that RotB does not in fact prevent speakers from acquiring and outputting sounds that were absent from their L1 input—English speakers could conceivably end up pronouncing clicks in loanwords, just as they have ended up saying *shlep* with initial [ʃl].

More interesting than this straightforward empirical problem with RotB is a logical argument based on *modus tollens*: **If P then Q. Not-Q. Therefore Not-P**. In his discussion of RotB Smolensky (1996a) proposes that at the initial state of the grammar, the beginning of acquisition, all structural (or markedness, or wellformedness) constraints outrank all faithfulness constraints: "The initial ranking is $\mathcal{H}_0$: Struc-H >> Faithfulness." Smolensky goes on to demonstrate that the claim that the initial state is in fact $\mathcal{H}_0$ "is explained as a learnability consequence of a fundamental OT principle, richness of the base". In other words, according to Smolensky, RotB implies $\mathcal{H}_0$ as a logical consequence: if RotB then $\mathcal{H}_0$. Here's the idea: If we assume RotB then no matter what input we feed into GEN, the (adult's) ranking of CON assures an output that is surface consistent with everything else the person outputs based on the ranking learned during acquisition—like the case of the input with click fed into an English grammar. This is because all markedness constraints for which there was no evidence for demotion must have remained highly ranked (undominated by a relevant faithfulness constraint): A constraint like *Click must have started out ranked above Faith-Click, and that relative ranking persists in the absence of evidence to the contrary. In general, this is guaranteed to happen only if $\mathcal{H}_0$ was the initial state, with all markedness constraints ranked higher than all faithfulness constraints. Thus, RotB solves the (putative) Poverty of the Stimulus problem alluded to above—RotB explains how the grammar "knows" to exclude clicks in the absence of click-relevant data in the course of acquisition.

So accepting the truth of RotB has $\mathcal{H}_0$ as a logical consequence: RotB

implies $\mathcal{H}_0$. Now, by *modus tollens*, if the initial state is independently known to be Not-$\mathcal{H}_0$, then RotB must be rejected. It turns out that this is not just a hypothetical situation.

Hale and Reiss (1997b; 1997a; 1998; 2008) demonstrate that $\mathcal{H}_0$ is untenable due to a number of logical and conceptual problems in the standard OT approach to phonological acquisition presented by Smolensky (1996a,b). Most notably Smolensky's model cannot handle the existence of neutralization in phonology, mappings from distinct input forms to identical surface forms. Hale and Reiss argue that much of the trouble arises from the acceptance by Smolensky and the majority of the OT acquisition and learnability literature of the view that the observed output in child speech well reflects the state of children's grammars, rather than reflecting factors in child speech output due to immature performance systems, motor planning and the like. As Prince and Tesar (2004, n.3) colorfully pointed out, Hale and Reiss's argument that only the ranking which is the opposite of $\mathcal{H}_0$ could work for an OT model of acquisition, a ranking where all faithfulness constraints initally dominate all markedness constraints, was not very popular: "Swimming against the tide, Hale & Reiss 199[8] insist on $\mathbf{F} >> \mathbf{M}$ as the default." The tide, however, appears to have changed, with work like Smith (2010) largely acknowledging that the Hale and Reiss view of the competence-performance distinction was correct in rejecting the extremely influential perspective of his own earlier work (Smith, 1973), e.g., "I am content to follow Hale and Reiss's (2008) suggestion that this is a matter of performance" (Smith, 2010, 66). There has been no coherent rejoinder to the logical arguments that Smolenky's $\mathcal{H}_0$ is untenable, or at best, reduces to an inelegant version of the Hale and Reiss view.

So, Smolensky shows that accepting RotB as true logically implies that $\mathcal{H}_0 = \mathbf{M} >> \mathbf{F}$, is the initial ranking. Since we know that $\mathcal{H}_0$ cannot be the initial ranking, we are licensed, via *modus tollens*, to conclude that RotB cannot be a true property of a psychologically plausible OT grammar, one acquired by children.

What does OT lose when *modus tollens* forces us to reject RotB? Not much. Bert Vaux (p.c.) points out that Tesar and Smolensky (1993, 87) propose that their Constraint Demotion Algorithm works even if $\mathcal{H}_0$ does not hold: "Does learnability depend on this assumption about the initial state? The answer is no; the same results can be shown to follow when the initial state is any arbitrary hierarchy." Tesar and Smolensky seem to have decided that the tides don't matter—$\mathcal{H}_0$ is not so important after all.

As pointed out above, RotB makes a false prediction about loanwords, so that's another reason to eliminate it. Interestingly, RotB is sometimes formulated as the "principle" that there are no morpheme structure constraints, no constraints on underlying forms or on the content of lexical entries (e.g., Kager, 1999, sec. 1.5.2). In this odd, negative formulation, OT shares a property with string theory, the theory of evolution, plate tectonics, and the Marxist theory of class struggle, all of which also lack morpheme structure constraints, along with an infinite list of other properties they all lack. Reiss (2008) warned against including in Universal Grammar or in individual grammars such constraints: it is obviously an empirically true generalization that no linguistic representation contains a banana, but we should not therefore build into linguistic theory a universal NoBanana constraint. Instead linguists should follow normal scientific practice and characterize the building blocks of language that constitute Universal Grammar. By insisting on RotB, not only does standard OT lead to the untenable conclusion that the initial ranking of constraints must be "M >> F", but it also entails accepting that yes, we have NoBanana. Proof by *modus tollens* saves us: No, we don't have NoBanana or constraints against having morpheme structure constraints[1]—because we also don't have RotB, since $\mathcal{H}_0$ can't be the initial state. Having clarified the status of this "fundamental principle" of the OT models in the literature, we are in a better position to determine whether the OT framework, or indeed any constraint-satisfaction approach, is worth pursuing as part of naturalistic inquiry into the language faculty.

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[1]Of course this does not mean that we *do* have morpheme structure constraints or bananas in grammar. To clarify further, assuming Minimalist syntax, we end up with only binary branching trees, because of the nature of Merge. There is no need to build into Universal Grammar a constraint against ternary branching trees.

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