

Squibs and Discussion

“SUBSTANCE ABUSE” AND
“DYSFUNCTIONALISM”:
CURRENT TRENDS IN
PHONOLOGY

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1 Form and Substance in Phonology

A coherent theory of the relationship between form and substance in linguistics has yet to be proposed for either phonology or syntax. In this squib we attempt to contribute to this necessary inquiry in the domain of phonology by first defining *form* and *substance*, then critiquing recent work that implicitly or explicitly touches on the relationship between the two. We will argue that the current trends in phonology are incompatible both with coherent definitions of form and substance and with basic principles of science. Since we are not proposing a complete alternative model of phonology, we invite the reader to reflect on how our proposals could be implemented or on how our assumptions (which we believe are widely shared in principle, if not in practice) should be modified.

We propose that it is useful to conceive of a grammar as a relationship between (a) a set of symbols (entities like features and variables; constituents like syllables, feet, NPs) and (b) a set of computations (operations whose operands are drawn from the set of symbols, such as concatenation and deletion). The issue of substance arises only with respect to the set of symbols, and for the sake of simplicity we restrict ourselves to the set of phonological primitives known as distinctive features and to the representations that can be defined as combinations of distinctive features. *Form* refers to the characteristics of phonological computations: Are they rules and/or constraints? Do they apply serially or in parallel? Are there limits on the number of operands they can refer to?

In this squib we concentrate on the issue of substance in phonology. In brief, the question we are interested in is the following:

- (1) Do the phonetic correlates (i.e., the substance) of a particular distinctive feature or feature bundle have any nonarbitrary

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bearing on how that feature or feature bundle is treated by the computational system?

It is trivial to show that languages differ in that their computational systems treat specific features or feature bundles differently—for example, Standard German has coda obstruent devoicing and English does not. From this we can conclude that languages *can* treat the same symbols differently. A more challenging problem arises when we find an apparent example of crosslinguistically universal, seemingly non-arbitrary treatment of a feature or feature bundle. In such cases we must ask ourselves the following question:

- (2) Is the observed pattern a reflection of substantive constraints on the computational system, or is the pattern due to other causes?

As we will show, other a priori plausible causes include the process of language change, the nature of the language acquisition device, and sampling errors. From the standpoint of grammatical theory, factors such as sampling errors are obviously uninteresting. However, language change and the nature of the learning path are also, strictly speaking, not part of grammatical theory. The modular approach to linguistics, and to science in general, requires that we both model the interactions between related domains and sharply delineate one domain from another. Occam's razor demands that, in doing so, we avoid redundancy and the postulation of unnecessary factors.

Before proceeding to our argument that generalizations that bear on patterns of phonetic substance are not relevant to phonological theory strictly defined, we can suggest that there is potentially much to gain from this approach in that it posits that universal phonology can be studied not just across languages, but also across modalities. What is shared by the phonologies of signed and spoken languages? We believe that there must be a core of formal properties (e.g., organization into syllables and feet, feature-spreading processes) that are modality independent and thus not based on phonetic substance. The goal of phonological theory should be to discover this formal core.¹

2 Three Examples of "Substance Abuse"

2.1 Positional Faithfulness in Beckman 1997

Beckman (1997) proposes the constraints in (3a–b) as members of the universal constraint set.

- (3) a. *IDENT- σ_I (hi)*

A segment in the root-initial syllable in the output and its correspondent in the input must have identical values for the feature [high].

¹ Thanks to Bill Idsardi, Chris Miller, and other participants in the Montreal-Ottawa-Toronto Phonology Workshop 1998 for pointing this out to us.

b. *IDENT*(hi)

Correspondent segments in output and input have identical values for the feature [high].

As Beckman explains, this set of constraints allows faithfulness to a feature, like [high], to be maintained in some contexts, but not others, since the context-sensitive constraint (3a) can be ranked above a markedness constraint that is violated by, say, the presence of high vowels, *HIGH, which in turn is ranked above the general constraint in (3b). In other words, the ranking in (4) will allow surface high vowels only in root-initial syllables.

$$(4) \text{IDENT-}\sigma_1(\text{hi}) \gg *HIGH \gg \text{IDENT}(\text{hi})$$

This is assumed to be a welcome result.

The high ranking of positional faithfulness constraints, relative to both the more general *IDENT* constraints and markedness constraints, yields the result that features and/or contrasts in *just those positions which are psycholinguistically or perceptually salient* are less susceptible to neutralisation than in other locations which are not protected. (Beckman 1997:8; emphasis in original)

Beckman (p. 5) cites more than ten psycholinguistic studies to support her claim that word-initial material is more salient than medial or final material. It is unclear, however, whether this generalization would hold, say, in a language with noninitial stress. It is also unclear whether Beckman's extension of psycholinguistic findings concerning word-initial syllables to *root*-initial syllables is justified. However, let us assume that Beckman has stated the relevant generalizations correctly. The more important point is that we believe the correct conclusion is the *opposite* of the one Beckman draws.² We reject Beckman's decision to encode the findings of psycholinguistic experimentation in the grammar because we think it is possible to derive the same result without positing new mechanisms like positional faithfulness.³ Consider the following alternative theory.

We know that children acquire spoken language primarily on the basis of acoustic input from speakers in their environment, with Universal Grammar (UG) providing constraints on the hypothesis space.⁴ We also know that phonological contrasts are best distin-

² We wish to stress that we are not singling Beckman out for any reason except that her article appeared recently in a widely read journal and is well written and clear in its arguments and assumptions.

³ For other arguments against context-sensitive faithfulness see Reiss 1996:315.

⁴ It may be a useful idealization to assume that UG does not just constrain the learning path, but completely determines it. We suspect that such a position will prove most fruitful in sketching an explicit theory of acquisition, but justification for this goes beyond the scope of this squib.

guished and recalled when occurring in certain positions. Imagine a child exposed to a language $\mathcal{L}_1$ that allows high vowels in all syllables—initial, medial, and final. Imagine further that $\mathcal{L}_1$ has initial stress and that stress is realized as relatively increased duration and intensity. Given this scenario, it would not be surprising to find that a child constructing $\mathcal{L}_2$ on the basis of output from $\mathcal{L}_1$ consistently fails to acquire a contrast between mid and high vowels in relatively short, quiet syllables (those that are noninitial and thus unstressed), but succeeds in acquiring it in initial syllables, which are stressed and thus longer and louder. This mapping from $\mathcal{L}_1$ to $\mathcal{L}_2$ is an example of “sound change”—in particular, what is known as a “conditioned merger” in traditional historical linguistics.

On the other hand, it is highly implausible that a child would consistently fail to correctly analyze the mid/high contrast in longer, louder (stressed) syllables, yet successfully analyze the contrast in relatively short, quiet syllables.

We see therefore that the existence of positional faithfulness phenomena can be understood as merely reflecting the nature of the learning situation.⁵

- (5) If the acoustic cues of a given contrast in the target language are correctly analyzed by the acquirer in a context where they are relatively weak, they will also be analyzed correctly in a context where they are relatively strong.

Note that (5) is essentially definitional, since the strength, or acoustic salience, of a contrast is just a measure of how easy it is to perceive. What is most important to understand is that the theory proposed here is not meant to *replace* a synchronic account of the data. So, the best synchronic analysis must somehow be able to generate vowel neutralization in noninitial syllables. (5) is meant to guide us in choosing a theory of grammar in which to couch that synchronic account, but (5) is not part of the grammar. Whatever theory of phonology one adopts must be able to synchronically generate the type of pattern that Beckman describes, but the predictions generated by the correct theory, qua phonological theory, should not overlap with the predictions derivable from (5).

The view of sound change proposed here suggests that many phonological tendencies, or markedness patterns, are actually emergent properties. “Positional faithfulness” is due, not to the nature of phonology, but to the “sifting effect” of acquisition on the incidental, arbitrary nature of the phonetic substance associated with phonological symbols. Since effects such as those observed by Beckman already have a coherent extragrammatical account within acquisition theory (and it is necessary, in any event, to have an acquisition theory), building positional faithfulness into a theory of universal phonology is a misuse, or abuse, of phonetic substance in theory construction.

⁵ This idea is discussed more thoroughly in Hale, forthcoming.

2.2 *r*-Insertion in McCarthy 1993

McCarthy's (1993) discussion of intervocalic *r*-insertion in Massachusetts English is fairly well known, so an example should suffice for illustration. In this dialect an underlying sequence like *Wanda arrived* is realized with a "linking" [r]: *Wanda[r] arrived*. As McCarthy himself notes (and as discussed by LaCharité and Paradis (1993) and Halle and Idsardi (1997)), "r is demonstrably not the default consonant in English" (p. 189). That is, it is not the maximally unmarked consonant that an Optimality Theory (OT) account predicts would emerge in such a situation. In order to account for the insertion of [r], McCarthy proposes a special *rule* of *r*-insertion: "a phonologically arbitrary stipulation, one that is outside the system of Optimality" (p. 190). There are several problems with this proposal, many of which are insightfully discussed by Halle and Idsardi. However, we propose that one of their criticisms requires elaboration. Halle and Idsardi rightly point out that "reliance on an arbitrary stipulation that is outside the system of Optimality is equivalent to giving up on the enterprise" (p. 337), but they do not discuss what we consider to be a most important observation: grammars contain arbitrary processes. McCarthy's grammar has an arbitrary component (containing rules like *r*-insertion) and a nonarbitrary component (containing the substantive OT constraints). Such a theory is empirically nondistinct from the theory we propose below, which posits that *all* grammatical computations are arbitrary with respect to phonetic substance. The set of phenomena predicted to exist by our theory (with only arbitrary processes) is identical to the set of phenomena predicted to exist by McCarthy's theory (with both nonarbitrary and arbitrary processes). Since McCarthy must adopt a model that allows arbitrary phenomena (like *r*-insertion), the addition of a special subcomponent to account for alleged "nonarbitrary" phenomena violates Occam's razor.

The diachronic source of *r*-insertion is transparent: the relevant dialects also exhibit *r*-deletion in codas, so insertion reflects rule inversion triggered by hypercorrection. Again, the diachronic facts do not make a synchronic account unnecessary, but they show that basically idiosyncratic historical events affect specific grammars—and, in part, how they may do so.

2.3 *Structural Constraints on Nonstructures*

Perhaps one of the most problematic cases of "substance abuse" we have come across is McCarthy's (1996) appeal to parameterized constraints to account for opacity effects in Hebrew spirantization. McCarthy invokes a constraint schema with parameters specifying the level at which conditions must hold. He then describes the specific instantiation of the constraint for Hebrew: "In correspondence terms, the meaning of this constraint is as follows: the constraint is violated if a surface stop β or its underlying correspondent is immediately preceded by a vowel" (p. 224; emphasis in the original). As pointed out in Reiss

1997, this powerful constraint type has two problems. It compromises the OT notion of a universal constraint set by allowing parameterized constraints. It also undermines McCarthy's implicit and explicit appeal to phonetic grounding for well-formedness constraints,⁶ since the structures that violate this constraint need not be potential surface structure strings. In fact, they need not exist as strings at any level of representation.

3 Neo-Saussureanism

The conclusion we wish to draw from the above examples and many others like them is that the best way to gain an understanding of the computational system of phonology is to assume that the substance of phonological entities is *never* relevant to how they are treated by the computational system, except in *arbitrary, stipulative* ways. What this means is that many of the so-called *phonological universals* (often discussed under the rubric of markedness) are in fact epiphenomena deriving from the interaction of extragrammatical factors like acoustic salience and the nature of language change. It is not surprising that even among their proponents, markedness "universals" are usually stated as "tendencies." If our goal as generative linguists is to define the set of *computationally possible* human grammars, "universal tendencies" are irrelevant to that enterprise. We propose extending the Saussurean notion of the arbitrary nature of linguistic signs to the treatment of phonological representations by the phonological computational system. Phonology is not and should not be grounded in phonetics since the facts that phonetic grounding is meant to explain can be derived without reference to *phonology*. Duplication of the principles of acoustics and acquisition inside the grammar violates Occam's razor and thus must be avoided. Only in this way will we be able to correctly characterize the universal aspects of phonological computation.

John Ohala has done the most to demonstrate that many so-called markedness tendencies can be explained on phonetic grounds and thus should not be explained by universal cognitive principles. Examples discussed by Ohala include patterns of assimilation and the contents of phonemic inventories. For an extensive bibliography on this topic see Ohala 1998. We differ from Ohala in our use of the term *phonology* (which for him covers all aspects of the sound systems of human language) but wholeheartedly endorse his approach.

It is obvious that our proposal runs contrary to most of the discussion in chapter 9 of *SPE* (Chomsky and Halle 1968). This chapter starts out with an "admission" that the theory developed in the earlier chapters is seriously flawed.

⁶ For example, McCarthy and Prince (1995:88) refer to a constraint *VgV as the "phonologization of Boyle's Law."

The problem is that our approach to features, to rules, and to evaluation has been overly formal. Suppose, for example, that we were systematically to interchange features or to replace $[\alpha F]$ by $[-\alpha F]$ (where α is $+$, and F is a feature) throughout our description of English structure. There is nothing in our account of linguistic theory to indicate that the result would be the description of a system that violates certain principles governing human languages. To the extent that this is true, we have failed to formulate the principles of linguistic theory, of universal grammar, in a satisfactory manner. In particular, we have not made use of the fact that the features have intrinsic content. (p. 400)

Later in the chapter Chomsky and Halle themselves acknowledge that they are on the wrong track.

It does not seem likely that an elaboration of the theory along the lines just reviewed will allow us to dispense with phonological processes that change features fairly freely. The second stage of the Velar Softening Rule of English (40) and of the Second Velar Palatalization (34) of Slavic strongly suggests that the phonological component requires wide latitude in the freedom to change features, along the lines of the rules discussed in the body of this book. (p. 428)

We propose that switching the feature coefficients as described in the first quotation might lead to the description of systems that are *diachronically* impossible human languages (ones that could never arise because of the nature of language change), but not to ones that are *computationally* impossible. The goal of phonological theory, as a branch of cognitive science, is to categorize what is a computationally possible phonology, given the computational nature of the phonological component of UG.⁷

4 Explanatory Inadequacy

What are the implications of our view that phonology should be all form and no substance? In particular, does this conclusion about the nature of phonological operands have any positive implications for phonological theory? We think that there is one clear conclusion to be drawn. Since we have argued that languages appear to vary in some arbitrary ways (e.g., inserting $[r]$ and not, say, $[t]$), it is necessary to develop a theory that allows for such variation. In other words, the child should be equipped with a universal computational system and a set of primitives that can be modified upon exposure to positive evidence. For this reason, we believe that current versions of OT,

⁷ This argument, as well as other ideas in this squib, was anticipated by Hellberg (1980) and Kaye (1989). More recently, Ploch (1997, 1999) argues that the phonology of nasals is grounded in cognitive, not phonetic, principles.

which assume a universal set of substantive constraints (*VOICEDCODA, LAZY, etc.), are not likely to prove enlightening. A set of constraint templates, with principles of modification from which the learner can construct the necessary constraint inventory for the target language, may prove to be more useful. Similarly, a rule-based theory equipped with a set of principles for defining possible rules would also allow for the type of stipulative, crosslinguistic variation we have argued is necessary. Note that, given an explicit theory of acquisition, such a “nativism-cum-constructivism” view of phonology is well constrained: UG delimits the set of possible rules or constraints; the data determine which rules or constraints are actually constructed.

In order to appreciate the fact that positing the type of substantive constraint found in the OT literature adds nothing to the explanatory power of phonological theory, consider the situation in which learners find themselves. Equipped with an OT-type UG, a child born into a Standard German-speaking environment “knows” that voiced coda obstruents are “marked.” However, this child never needs to call upon this knowledge to evaluate voiced coda obstruents, since there are none in the ambient target language. In any case, by making use of positive evidence the child successfully acquires a language like German. Born into an English-speaking environment, the child again knows that voiced coda obstruents are marked. However, the ambient language provides ample positive evidence that such sounds are present, and the child must override the supposed innate bias against voiced coda obstruents in order to learn English. So, this purported UG-given gift of knowledge is either irrelevant or misleading for what needs to be learned. Our substance-free theory of phonology shares with OT-type theories a reliance on positive evidence. The two theories have the same empirical coverage, since we also assume that both English and German are acquired. The difference is that we leave out of the genetic inheritance “hints” that are irrelevant or misleading. We find our solution to be more elegant. Once again, note that this argument is equally applicable to markedness theories of all types, not just those couched within OT. Since markedness cannot have any bearing on learnability, it is probably irrelevant to any explanatorily adequate theory of grammar. We thus propose banishing markedness from consideration in future linguistic theorizing.⁸

⁸ In fact, there are two distinct types of markedness in the phonological literature. This squib is concerned with substantive markedness. Simplicity or evaluation metrics of the *SPE* symbol-counting type can be seen as measuring “formal” markedness. We believe that the best approach to such formal requirements is to build them into the language acquisition device (LAD). Under this view learners never compare extensionally equivalent grammars for simplicity or economy; they simply construct the one that is determined by the LAD. There is, then, no reason to introduce the terms *simplicity* and *economy* into the theory since they are contentless labels for arbitrary (i.e., not derivable) aspects of the LAD. For a concrete example of how we think the characterization of the LAD should be approached, see Hale and Reiss, forthcoming.

5 Discussion

The “substance abuse” approach has been criticized for cognitive science in general by Pylyshyn (1984:205ff.). Pylyshyn describes a box emitting certain recurrent patterns of signals. He then asks what we can conclude about the nature of the computational mechanism inside the box, given the observed pattern of output. The answer is that we can conclude nothing, since the observed patterns may reflect the nature of what is being computed (in his example, the output is a Morse code rendering of English text, and the observed regularity is the “*i before e, except after c*” rule), not the nature of the computer. In Pylyshyn’s words, “[*T*]he observed constraint on [the system’s] behavior is due not to its intrinsic capability but to what its states represent” (p. 207; emphasis in the original). If we are interested in studying the phonology “computer,” then we need to distinguish a possible phonological computation from an impossible one. The set of attested phonological patterns and their distribution may be somewhat skewed by the sifting effect of language change. Real explanation of the nature of phonological computation requires us to see beyond such epiphenomena as “markedness tendencies.”

We believe that the current impregnation of the architecture of the phonological “virtual machine” with phonetic substance represents a step backward for phonological theory. Phonologists should now call upon their impressive success in amassing descriptions of individual phonological “programs” and aim for a more abstract, but deeper, understanding of phonological computation.

6 The Mirage of Enhancement

A particularly illustrative combination of functionalism and what we consider to be the misuse of substantive considerations can be found in the literature on phonetic enhancement and the maximization of contrast (e.g., Stevens, Keyser, and Kawasaki 1986). For example, the tendency of three-vowel systems to contain the maximally distinct set /i, u, a/ is taken as a reflection of a phonological principle demanding the “best” use of the available acoustic space. Like other claims concerning markedness and UG, this pattern is no more than a tendency. However, we can show that the view of markedness as an emergent property, outlined above, can give insight into this statistical pattern. Imagine a language $\mathcal{L}_1$ that had the four vowels /i, u, e, a/. Now we know that merger of acoustically similar vowels (like /i/ and /e/) is a common diachronic process. It would not be surprising if a learner constructing $\mathcal{L}_2$ on the basis of data from speakers of $\mathcal{L}_1$ were to fail to acquire a slight distinction and end up with a three-vowel system containing /i, u, a/. However, it is much less likely that the learner would fail to acquire an acoustically more robust distinction like /u/ versus /a/ and end up with an inventory containing, say, /i, u, e/.⁹ So,

⁹ Note that “phonetic substance” may itself indicate how weak the rea-

vowels that are close together in the acoustic space are likely to merge diachronically. Vowels that are acoustically distant are not likely to merge diachronically. The observed pattern of maximal contrast is thus not built into the phonology, but is an emergent property of the set of observed phonological systems due to the nature of diachronic sound change.

7 Functionalism and “Dysfunctionalism”

The rise of OT has been accompanied by a revival of functionalism in phonology. In fact, there is no necessary connection between OT as a theory of computation and functionalist reasoning, and an OT proponent might invoke what we call the “NRA defense” (“Guns don’t kill people; people kill people”): Computational theories aren’t inherently functionalist; people are functionalist. However, the ease with which functionalist ideas can be implemented in OT has clearly invited this “functionalist” explosion and may bear on the question of whether or not the theory is sufficiently constrained or even constrainable. Note also that the “logic” of functionalism (namely, that *all* phenomena are explicable by reference to competition between universal, but violable, principles) is identical to the logic of OT. In this section we briefly show that the “substance” orientation of functionalism can be turned on its head to yield a theory that we will dub “*dysfunctionalism*.”

Many functionalist theories of grammar can be summarized in almost Manichean terms as consisting of a struggle between the “competing forces” of ease of articulation (what is presumed to be “good for” the speaker) and avoidance of ambiguity (what is presumed to be “good for” the hearer). As an example of the former, consider Kirchner’s (1997) constraint “*LAZY—Minimize articulatory effort*” (p. 104). For avoidance of ambiguity, consider Flemming’s (forthcoming) *MAINTAIN CONTRAST* constraints, which are violated by surface merger of underlying contrasts.

The interplay of what is “good for” the speaker and what is “good for” the hearer supposedly gives rise to the patterns we see in language: sometimes mergers occur and the speaker’s output is “simplified,” potentially creating a difficulty for the hearer; sometimes the speaker maintains distinctions, perhaps producing a more “complex” output, thus avoiding ambiguity for the hearer.¹⁰

soning is in this case: English [i], as well as the other front vowels, is significantly lower than Danish [i]. Why is the “maximization of contrast” not active at the phonetic level—precisely the level that provides the alleged “substance” (perceptual distinctness, in this case) for the functionalist claim?

¹⁰ Further evidence for the incoherence of the functionalist position is the fact that “careless” speech often can lead to supposedly complex outputs such as the stop cluster in [pt]ato for *potato*. Onset stop clusters are not found in careful speech, so it is surprising, from a functionalist perspective, that they should be found precisely when the speaker is not putting forth greater articulatory effort.

The problem with this theory is that functionalist principles can be replaced by their opposites, which we will call “dysfunctionalist” principles, with no significant change in the set of grammars predicted to exist. Consider the following principles, proposed by a linguist with a different view of human nature than the functionalists have:

(6) *Principles of “dysfunctionalism”*

OBFUSCATE: Merge contrasts, use a small inventory of distinctive sounds, and so on.

NO PAIN—NO GAIN: Maintain contrasts, use a large inventory, generate allomorphy, and so on.

Merger (as well as the oft-proclaimed diachronic principle “Change is simplification”), well attested in the languages of the world, will be accounted for by the (dys)functional requirement that one should OBFUSCATE. The failure of merger (and the generally ignored diachronic process of “complexification”), equally widely attested, will be attributed to the effects of NO PAIN—NO GAIN. The competition of these two “dysfunctionalist” principles will thus lead to the same results as the usually cited functionalist principles. Although the ultimate question of whether human beings are fundamentally lazy, but helpful, or something seemingly more perverse is intriguing, it hardly seems that investigation into such matters should form the foundation of a theory of phonological computation.¹¹ We propose, therefore, that functionalism provides no insight into the nature of grammar. Again, we propose leaching all substance out of phonology in order to better observe the abstract computational system.

The alternative—which seems to be the focus of many current developments in phonological theory—seems clear. Given a sufficiently rich and explicit theory of the human personality (giving us principles such as “Be lazy” and “Be helpful to the listener”) and the human articulatory and perceptual systems (“phonetic” substance), phonology itself will turn out to be epiphenomenal. Although this seems considerably less promising to us, it has clear implications for the research strategy that phonologists should adopt. Phonologists, under such a view, should focus their energies in two domains: phonetics and the empirical explication of fundamental features of the human personality (“laziness,” “helpfulness,” etc.).

8 Conclusions

We are advocating that phonologists, qua phonologists, attempt to explain less, but in a deeper way. As we hope to have indicated, empirical results provided by phoneticians and psycholinguists contribute to the development of a substance-free phonology, and we look forward to important cooperation with scholars in these fields. We

¹¹ We would be happy to provide examples—drawn from the history of linguistic theory—of the evolutionary advantages of self-interested effort (NO PAIN—NO GAIN) and OBFUSCATE. We refrain for reasons of space, fully confident that readers will have no difficulty generating ample evidence on their own.

recognize that only they can provide explanation for many (E-language) generalizations that are striking in their statistical regularity.¹² Since we believe that the focus of phonological theory should be on the cognitive architecture of the computational system, we also believe that the nonsubstantive aspects of OT have been tremendously important for the development of the field. The best of the OT literature is far more explicit about the nature of the assumed computational system than its predecessors often were. The mere existence of such a well-developed alternative to rule-based phonology is valuable, regardless of specific formal problems (e.g., synchronic “chainshifts”) or the “substance abuse” found in any particular implementation.

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¹² But see Engstrand 1997a,b for arguments that the statistics may be misleading. For example, the purported markedness of /p/, as evidenced by its relative rarity in voiced stop inventories, vis-à-vis /t/ and /k/, is probably illusory. The overwhelming majority of the languages in a database like UPSID (Maddieson 1984, Maddieson and Precoda 1989) lacking a /p/ are found in Africa. Similarly, the languages of Africa do not “avoid” voiced velar stops, which are also commonly assumed to be marked (see footnote 6). “Thus, it cannot be concluded that velars and bilabials constitute universally underrepresented members of the respective voiced and voiceless stop series. Although this pattern is to be expected from proposed production and perception constraints, it is largely overridden by areal biases” (Engstrand 1997a:187).

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SUBJECT HONORIFICATION IN

JAPANESE

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1 Introduction

In the generative grammar literature, Japanese, which does not seem to exhibit overt agreement (e.g., Shibatani 1990), is nevertheless taken to show the same kind of agreement that is explicitly marked in many European languages, which is now characteristically explained by way of feature checking (Chomsky 1993, 1995); let us informally call this type of agreement *syntactic agreement*. One phenomenon in Japanese that directly suggests the existence of syntactic agreement is *subject honorification* (e.g., Harada 1976), examples of which are given in (1) (from Kuno 1973:20).

- (1) a. Gakusei-ga Mary-o matu.
 student-NOM Mary-ACC wait
 'The student waits for Mary.'
 b. Sensei-ga Mary-o o-mati ni naru.
 teacher-NOM Mary-ACC honorific-wait-ing -ly become
 'The teacher waits for Mary.'

This phenomenon "involves the conversion of the verb into the *o V-ni naru* form" (Shibatani 1990:283),¹ and it is said to take place only when the "subject is an SSS [a person socially superior to the speaker]" (Harada 1976:514). Therefore, the verb *matu* 'wait' is realized as *o-mati ni naru* in (1b), since the subject *sensei* 'teacher' is an SSS; it remains as is in (1a), where the subject *gakusei* 'student' is a noun whose referent is not usually entitled to special respect.²

Harada (1976:517) tries to capture this by positing a "syntactic feature . . . [+Subject Honorific]" or [+SH], clearly assuming that subject honorification is a syntactic agreement phenomenon between a subject with [+SH] and its predicate. Translating this analysis into minimalist terms, Ura (1993:387), following the insight of Toribio (1990), specifically claims that "[subject] honorification in Japanese is a solid mark of feature-checking between AgrS and 'subject.'"

However, a closer look at this phenomenon leads us to suspect that it is not the same as the syntactic agreement that plays a crucial role in the current generative literature. In this squib, drawing on several empirical facts about subject honorification in Japanese, I will show that it is implausible to analyze this phenomenon as a kind of syntactic agreement.

I thank the anonymous *LI* reviewers for their comments and suggestions on an earlier version of this squib.

¹ But the *ni naru* part is a "further complication" (Harada 1976), and it becomes relevant only when the predicate is a verb phrase. When the predicate is not VP, only the honorific prefix *o-* comes into play. See Harada 1976: 503–505 for more explanation.

² As a reviewer points out, whether or not a given subject counts as an SSS is pragmatically determined. However, once the subject is considered as an SSS, it is said to syntactically trigger on its predicate the subject-honorific agreement in question. Harada's [+SH] feature account, to which we now turn, concerns itself with this second part of the phenomenon of subject honorification.

2 Arguments against the Syntactic Analysis

In this section I present four arguments revealing weaknesses in the analysis that treats subject honorification as a syntactic agreement phenomenon.

2.1 Adverbials

First, subject honorification can trigger agreement even on an adverbial. In (2), for example, the subject *sensei* ‘teacher’ is assumed to have the syntactic[+ SH] feature; accordingly, the verb *yameta* ‘resigned’ is realized as *o-yame ni natta* in this example.

- (2) Sensei-ga go-seisiki-ni o-yame ni natta.
 teacher-NOM officially resigned
 ‘The teacher officially resigned.’

Notice here that the adverbial *seisiki-ni* ‘officially’ also gets an honorific mark *go-*, which is a variant of *o-* (Harada 1976:504). To capture this in the minimalist framework, we would have to posit feature checking between the adverbial and the subject noun phrase. However, in light of the fact that an adverbial and a noun do not combine,³ this step would be illegitimate, a fact that weakens the basic motivation for regarding subject honorification as a syntactic agreement phenomenon.

2.2 Failure to Check [+SH]: Evidence from Reciprocals

Second, the allegedly syntactic feature [+SH] of a subject does not seem to cause derivational crash even when the predicate does not get the honorific mark and hence fails to perform the relevant feature checking with the subject.

Following Chomsky (1986), let us assume that anaphors are like NP-traces in that they share all agreement features with their antecedents. Then, the grammaticality of (3) follows straightforwardly.

- (3) [Osamu to Akira]_i-ga [otagai_i-ga kyougisita
 Osamu and Akira-NOM each.other-NOM discussed
 koto]-o mitometa.
 fact-ACC admitted
 ‘Lit. Osamu and Akira admitted that each other discussed.
 (i.e., Osamu and Akira admitted that they’d talked.)’

³ In fact, what is official in (2) is not the teacher, but the teacher’s resignation. Hence, the kind of analysis suggested by a reviewer in (i), which contains a bracketed small clause, seems semantically unjustified.

(i) Sensei_i-ga [PRO_i go-seisiki-ni] o-yame-ni natta.
 official

I thank the reviewer for drawing my attention to this hypothetical analysis, which led to refinement of the example sentence.

In this example the reciprocal *otagai* must share with the subject *Osamu to Akira* all agreement features, including the (plural) number feature. This is clear from the contrast between (4a) and (4b).

- (4) a. Osamu to Akira-ga kyougisita.
 Osamu and Akira-NOM discussed
 'Lit. Osamu and Akira discussed. (i.e., Osamu and Akira talked.)'
 b. *Osamu-ga kyougisita.
 'Lit. Osamu discussed. (i.e., *Osamu talked to each other.)'

The verb *kyougisuru* 'discuss' is a verb of "symmetry" (Masuoka and Takubo 1992) or "reciprocation" (Levin 1993) and requires a plural subject when it is used intransitively. Hence, (4a), where the subject is plural, is fine, whereas (4b), where the subject is singular, is ill formed under the intended intransitive reading.⁴ Therefore, we have a solid reason to follow Chomsky in assuming that the reciprocal in the grammatical sentence in (3) must share with *Osamu to Akira* all features of the matrix subject, including the number feature.

Now, consider the following example:

- (5) [A sensei to B sensei]_i-ga [otagai_i-ga
 A teacher and B teacher-NOM each.other-NOM
 kyougisita koto]-o o-mitome ni natta.
 discussed fact-ACC admitted
 'Lit. Teacher A and Teacher B admitted that each other
 discussed. (i.e., Teacher A and Teacher B admitted that
 they'd talked.)'

Notice in this fully grammatical sentence that the verb in the embedded clause does not have the honorific mark although the verb in the matrix clause does. This means that unlike the [+SH] feature of the matrix subject, that of the embedded reciprocal subject fails to be checked, which then should lead the derivation to crash, contrary to fact. This observation thus constitutes an empirical difficulty for the feature-checking analysis of subject honorification, again suggesting the implausibility of the syntactic analysis.⁵

⁴ The sentence is grammatical with an implicit interlocutor.

(i) Osamu-ga pro kyougisita.
 Osamu-NOM pro(-with) discussed
 'Osamu talked (with somebody).'

⁵ One cannot argue that subject honorification is a phenomenon observed only in matrix clauses, since the following sentence, where honorification takes place even in the embedded clause, is perfectly grammatical as well:

(i) [A sensei to B sensei]_i-ga [otagai_i-ga go-kyougi ni natta
 A teacher and B teacher-NOM each.other-NOM discussed
 koto]-o o-mitome ni natta.
 fact-ACC admitted

2.3 Coordination

Third, consider the coordination structure in (6).

- (6) C sensei-ga wakaku-te utukusii.
 C teacher-NOM young-and beautiful
 'Teacher C is young and beautiful.'

In the predicate of this sentence, two adjectives are coordinated.⁶ What is interesting about this sentence is that when subject honorification takes place, the honorific mark need not appear on the two adjectives simultaneously.

- (7) a. C sensei-ga wakaku-te o-utukusii.
 b. C sensei-ga o-wakaku-te utukusii.
 c. C sensei-ga o-wakaku-te o-utukusii.

If the presence of the honorific prefix were to be explained by feature checking, only (7c), where both adjectives are prefixed by *o-*, would be considered grammatical; however, (7a–b) are equally grammatical, where only one adjective checks [+SH], the other one failing to do so. This should cause derivational crash, but the resulting sentences are

'Lit. Teacher A and Teacher B admitted that each other discussed. (i.e., Teacher A and Teacher B admitted that they'd talked to each other.)'

A reviewer claims, however, that what is needed for sentences like (i) and (5) may be "a syntactic mechanism that optionally deletes a [+SH] marking on an embedded verb if its subject is coreferential with the main clause [+SH] subject." What is postulated to delete by this mechanism is a syntactic [+SH] marking, but not the semantic feature associated with it; thus, it is theoretically possible to interpret the embedded subject in (5) with the intended honorification.

But resorting to this kind of optionality seems ad hoc at best, since it does not appear to follow from anything in the grammar of Japanese, nor does it seem to be a linguistic primitive in itself. Besides, honorific marking in only the embedded clause is also possible, as shown in (ii).

- (ii) [A sensei to B sensei]_i-ga [otagai_i-ga go-kyougi ni natta koto]-o mitometa.

If we explained this by devising another ad hoc mechanism that would optionally delete the [+SH] marking on the matrix verb on condition that a coreferential relation held between the subjects of the matrix and embedded clauses, what would happen when the two mechanisms optionally deleted the [+SH] marking on both matrix and embedded verbs simultaneously, as in (iii)?

- (iii) [A sensei to B sensei]_i-ga [otagai_i-ga kyougisita koto]-o mitometa.

The semantic features associated with honorification would theoretically survive to LF, but (iii) is never interpreted as an honorific sentence. If one were to posit that simultaneous deletion was somehow blocked, one would then have to ask why and how, further complicating the explanation and quite possibly making it even more ad hoc. Hence, the suggested optional [+SH] deletion analysis seems too problematic to be adopted.

⁶Regarding the *te* attached to the first adjective, I assume, following Morita (1980), that it is equivalent to the coordinative conjunction *sosite* 'and'.

well formed. This state of affairs clearly contrasts with the following English examples:

- (8) a. *Money come and goes.
- b. *Money comes and go.
- c. Money comes and goes.

Here only (8c), where both verbs have the agreement mark *-s*, is grammatical.

Thus, coordination facts also cast doubt on the validity of the feature-checking analysis of subject honorification.

2.4 Lexical Gaps

Finally, if subject honorification were a syntactic agreement phenomenon, we would expect to see its mark on any predicate whose subject is assumed to have the [+SH] feature. However, this expectation is not met.

First, observe that the following sentences with adjectival predicates are well formed:

- (9) a. Sensei-ga o-hade/go-kenkou/o-zyoubu da.
teacher-NOM flashy/healthy/tough be
'The teacher is flashy/healthy/tough.'
- b. Sensei-ga o-katai/o-isogasai/o-yasasii.
teacher-NOM strict/busy/gentle
'The teacher is strict/busy/gentle.'

(9a) contains so-called *na*-adjectives, and (9b) *i*-adjectives; all the adjectival predicates receive the honorific prefix, seemingly realizing the syntactic subject honorification agreement. However, quite a few adjectives cannot receive this "agreement" mark.

- (10) a. Sensei-ga *o-yuumei/*o-kodoku/*o-muti da.
teacher-NOM famous/lonely/ignorant be
'The teacher is famous/lonely/ignorant.'
- b. Sensei-ga *o-tadasii/*o-okuyukasi/*o-awatadasii.
teacher-NOM right/graceful/hurried
'The teacher is right/graceful/hurried.'

In fact, the list of the adjectives that do not take the honorific prefix is very long.⁷ Since there do not seem to be any "suppletive forms"

⁷ Examples of the *na*-adjectives and *i*-adjectives that do not seem to take the honorific prefix are given in (i) and (ii).

- (i) *na*-adjectives: aho 'foolish', aimai 'ambiguous', gouin 'forcible', idai 'great', nonki 'carefree', oogesa 'exaggerated', rokotu 'blunt', uchouten 'overjoyed', youti 'childish', yutaka 'wealthy', etc.
- (ii) *i*-adjectives: abunai 'dangerous', hanabanasii 'glorious', hitosii 'equivalent', ikenai 'bad', itizirusii 'remarkable', monosugoi 'awful', namamekasii 'voluptuous', nayamasii 'sexy', oisii 'delicious', omosiroi 'funny', etc.

(Harada 1976:519–522) for adjectives that indicate honorification,⁸ adjectives like the ones in (10) raise a difficult question for the syntactic analysis of subject honorification. That is, if subject honorification were truly a syntactic agreement, why wouldn't it be realized in sentences like the ones in (10), which seem to be semantically faultless?⁹ Thus, these lexical gaps constitute yet another empirical difficulty for the syntactic analysis of subject honorification.

3 Conclusion

As we have seen, facts about adverbials, reciprocals, coordination, and lexical gaps all point to the difficulty of maintaining the syntactic feature-checking analysis of subject honorification. I thus conclude that we should drop this analysis and look for a better one from a different perspective.¹⁰

⁸ As for verbs, Harada (1976:520) gives *ide* (as in *o-ide ni naru*) and *ossyaru* as suppletive forms of *iku* 'go' and *iu* 'say', respectively.

⁹ In fact, the sentences in question are all fine without the honorific prefix.

- (i) a. Sensei-ga yuumei/kodoku/muti da.
teacher-NOM famous/lonely/ignorant be
'The teacher is famous/lonely/ignorant.'
- b. Sensei-ga tadasii/okuyukasii/awatadasii.
teacher-NOM right/graceful/hurried
'The teacher is right/graceful/hurried.'

However, without the honorific prefix, the subject *sensei* 'teacher' is not honored in (i). In order to show respect in these semantic contexts, the only thing the speaker can do is to use different lexical items that may take the prefix. For example:

- (ii) Sensei-ga go-chomei da.
teacher-NOM famous be
'The teacher is famous.'

¹⁰ From the lexical gaps observed in section 2.4, we may be led to suspect that the attachment of the honorific prefix is a morphological process that takes place entirely in the lexicon, being totally independent of what happens in syntax. Indeed, lexical gaps of this type have been reported to be very common in word formation processes, and they seem to constitute "non-existent but possible words" (Kageyama 1993:9–10). If so, the choice between, say, *o-isogasii* 'busy (honorified)' and *isogasii* 'busy (neutral)' would be purely lexical, just like the choice between, say, *pass away* and *die* in English.

- (i) a. Dr. Gold passed away.
- b. That stupid bastard died.

This lexical choice is by no means determined by syntactic feature checking of any kind; in fact, an example like (ii) is semantically odd, but syntactically well formed.

- (ii) #That stupid bastard passed away.

This oddity seems to parallel that of (iii).

- (iii) #Ano tonma-ga o-isogasii.
that fool-NOM busy (honorified)
'That fool is busy.'

As a reviewer points out, however, this is not the only alternative; nor can it be adequately argued for in the confines of a squib. Hence, I would like to suggest it only as one possibility at this time, leaving solid argumentation in its favor for future research.

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A TYPE OF HEAD-IN-SITU
CONSTRUCTION IN ENGLISH
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1 Syntactic Amalgams

The pattern of *syntactic amalgams* (SAs) in (1) has gone largely undiscussed since Lakoff 1974.

- (1) John invited you'll never guess how many people to his party.

Lakoff proposes to derive SAs by combining two independent clauses as in (2).¹

We wish to thank Chris Collins and audiences at Cornell University and Kanda University of Foreign Studies for discussion and comments.

¹ In this squib we omit discussion of the pragmatic restrictions on SAs proposed by Lakoff. Lakoff's actual derivation of (1) posits an underlying sentence of the form *John invited [a lot of people] to his party*; S₁ in (2b) is attached subject to the restriction that S₁ conversationally entails 'John invited

- (2) a. John invited [indefinite NP] to his party.
 b. [_{S₁} You'll never guess [_{S₂} how many people
 [_{S₃} ~~John invited to his party~~]]].

S₁ undergoes sluicing of S₃ and is attached to (2a) by adjunction to the indefinite NP, which is subsequently deleted.² As Lakoff notes, this derivation basically instantiates a generalized-transformation analysis of clausal embedding.³

Lakoff's analysis accounts for the placement of S₁ in the position of the gap corresponding to the complement of *invited* in (1). However, it leaves open major questions, including these:

- (3) Why is the gap in the matrix clause interpreted in relation to the *wh*-expression in S₂? That is, why is the object of *invited* in (1) interpreted as *a number of people n such that you will never guess n*?⁴
 (4) Are there syntactic restrictions on the relationship between the *wh*-expression in S₂ and the matrix clause?

a lot of people to his party'. This restriction is intended to account for the contrast between (ia) and (ib).

- (i) a. Babe Ruth hit how could anyone forget how many home runs?
 b. *Babe Ruth hit Sam forgot how many home runs. (Lakoff's judgment)

We find (ib) perfectly acceptable, but this does not disprove the validity of the proposed pragmatic restriction, since it is not difficult to imagine a context where 'Sam forgot how many home runs Babe Ruth hit' conversationally entails that Babe Ruth hit a lot of home runs. The problem is that given two Ss of the form [S₁ . . . [*wh* S₂]] and S₂, where S₂ contains an indefinite expression in the place of *wh*, it seems to be generally possible to establish a relationship of conversational entailment between S₁ and S₂. But this is just the general context for sluicing.

² This particular derivation is attributed by Lakoff to William Cantrall; Lakoff earlier discusses a derivation in which S₁ is substituted for the indefinite NP.

³ The generalized-transformation approach raises another important issue. Sluicing (of S₃ in (2b)) emerges as an instance of antecedent-contained deletion (ACD) once (2a) and (2b) are combined as in (1). Although not discussed by Lakoff, this approach suggests a solution to the ACD problem: deletion (or copying) in ACD contexts may take place *before* the ACD context and its host clause are combined (rather than *after* they are separated, as in the quantifier raising analysis of May 1985 or the extraposition analysis of Baltin 1987). Hornstein (1994) hints at the possibility of such an approach but rejects it on the basis that deletion/copying should be restricted to the LF interface level; but the basis for this restriction is not obvious.

⁴ The pragmatic restriction mentioned in footnote 1 does not by itself account for this interpretation. Consider an example such as *John is going to marry one of his friends guessed who*, which is acceptable to us. In this sentence *Someone guessed who John is going to marry* (corresponding to (2b)) conversationally entails 'John is going to marry someone' (corresponding to (2a)), but this does not explain why the sentence receives the interpretation 'John is going to marry some person *p* such that one of John's friends guessed *p*'s identity'.

We investigate these questions by contrasting SAs of the type in (1), which we label *wh*-SAs, with a second subtype introduced by Lakoff. We show that whereas the second type observes locality conditions suggestive of a derivation involving syntactic movement, the *wh*-SA type in (1) does not.

2 Horn's Cases (Cleft-SAs)

Lakoff attributes examples of the following type to Larry Horn:

- (5) John is going to, I think it's Chicago on Sunday.

We label the subvariety in (5) *cleft*-SAs. Like *wh*-SAs, Lakoff proposes to derive cleft-SAs by ellipsis followed by clausal adjunction to an empty NP. The presupposition S_3 in the embedded cleft S_2 in (6b) undergoes ellipsis, and the resulting truncated version of S_1 is adjoined to [NP] in (6a).

- (6) a. John is going to [NP] on Sunday.
 b. [S_1 I think [S_2 it is Chicago [S_3 ~~John is going to~~
~~on Sunday~~]]].

The same questions that arise regarding *wh*-SAs also arise regarding cleft-SAs: why is the gap in the matrix clause obligatorily related to the focus of the cleft (*Chicago* in (5)), and what, if any, are the syntactic restrictions on the pattern? The cleft-SA pattern as analyzed by Lakoff superficially resembles an internally headed relative clause (IHR): the notional head *Chicago* is internal to the modifying clause, and an influential approach to IHRs has been to analyze them as modifiers of an empty head (Cole 1987, Tsubomoto 1985). However, analyzing the cleft-SA pattern as an IHR faces numerous problems. The notional head in (5) is a referring expression, whereas the internal heads of IHRs have been claimed to be indefinite (Watanabe 1992). More importantly, such an analysis must explain why English lacks IHRs corresponding to the general pattern exemplified by Japanese (7).

- (7) Taroo wa [Hanako ga ringo o sara no ue ni
 Taroo TOP Hanako NOM apple ACC plate GEN top on
 oita no] o totta.
 placed COMP ACC picked up
 'Tarô picked up an apple which Hanako had (just) put on
 a plate.' (Kuroda 1975–6:87)

For these reasons we explore the possibility that the cleft-SA pattern actually has something closer to the derivation of a conventional relative clause. The immediate problem faced by such a derivation is how to relate *Chicago* and the empty NP head in (5). Consider, for concreteness, Safir's (1986) analysis of relatives, which requires that the head noun bind an element in Comp ([Spec, CP], in current terms). This requirement appears to be violated in cleft-SAs, given a representation as in (8).

- (8) John is going to $[[_{NP} e] [_{CP_1} I \text{ think } [_{CP_2} \text{ it is Chicago } [_{CP_3} Op \text{ (John is going to } t_{Op} \text{ on Sunday)}]]]]]$ on Sunday

(8) represents (5) prior to ellipsis. Regardless of how the elements $[_{NP} e]$, *Chicago*, *Op*, t_{Op} are related (e.g., through indexation), (8) violates the requirement that the head of the relative structure bind an element in $[Spec, CP]$. The same problem holds for other treatments of the relationship between head and relative clause (e.g., Williams's (1980) and Browning's (1987) analyses of the head-relative relationship based on predication): the relative head is too distant from the other components of the construction.

The solution that suggests itself here is that one of the elements in (8) moves to a position near the head. Evidence that such movement occurs comes from the fact that the acceptability of the cleft-SA pattern is degraded in island contexts.⁵

- (9) a. John is going to $[_{NP} e]$ it's obvious that it's Chicago on Sunday.
 b. ?*John is going to $[_{NP} e]$ that it's Chicago is obvious on Sunday.
 c. ?*John is going to $[_{NP} e]$ I got angry because it was Chicago on Sunday.
 d. ??John is going to $[_{NP} e]$ I believe the claim that it is Chicago on Sunday.

In (9a) a bridge predicate intervenes between the embedded cleft and the gap in the position of the complement of *going to*; in (9b) the embedded cleft is contained in a subject island, in (9c) in an adjunct island environment, and in (9d) in a complex NP. (9b–d) are all less acceptable than (9a).

The question now is what moves in the pattern shown in (9). The focus element *Chicago* is clearly in situ in the syntax; although it is conceivable that *Chicago* might be moved at a postsyntactic level of representation,⁶ the island effects in (9) are characteristic of syntactic, not abstract, movement. This leaves the possibility that what moves is the empty presupposition CP_3 in the embedded cleft structure.⁷ This movement gives (8) the following representation:

- (10) John is going to $[[_{NP} e] [_{CP_1} [_{CP_3} Op \text{ (John is going to } t_{Op} \text{ on Sunday)}]] [I \text{ think } [_{CP_2} \text{ it is Chicago } t_{CP_3}]]]]]$ on Sunday

⁵ The acceptability judgments regarding the island contexts in (9) and (11) are Whitman's. These judgments were confirmed by students in two elementary linguistics classes at Cornell University.

⁶ As in many analyses of IHRs (Cole 1987). Watanabe's (1992) analysis of IHRs is based on syntactic movement of an empty operator from the specifier of the notional head. This analysis resembles the analysis proposed here for cleft-SAs, except that in the latter the source of the moved constituent is somewhat more remote from the notional head, and in Watanabe's analysis the notional head is crucially indefinite (so as to be able to host the relative operator in its specifier).

⁷ Richard Kayne (lectures at CUNY, fall 1996) has suggested that ellipsis structures in general are derived by movement of the ellipted category.

(8) satisfies the requirement that the relative head bind an element in [Spec, CP₃]. Furthermore, the structure in (8) satisfies Safir's (1986) Locality Condition on R-Binding, which stipulates that the element bound by the relative head be the highest element in [Spec, CP]. The same holds for the other above-cited approaches to relating the relative head with an item in the relative clause.

3 *Wh*-Syntactic Amalgams Again

Deriving the cleft-SA pattern in (5) as in (10) explains the occurrence of island effects symptomatic of syntactic movement and the implication of a cleft structure: the cleft structure provides a constituent (CP₃ containing the empty operator of the cleft structure) with the content required to relate the nominal head of the relative clause structure with a position in the relative clause. We now return to the original *wh*-SA pattern of (1). In this pattern the item to be related to the gap in the matrix clause is the *wh*-expression left behind by sluicing. The *wh*-expression is overtly not in a position close to the matrix gap, and unlike what is found in the cleft-SA pattern, there is no other element whose movement might serve to relate the nominal head to a position in the relative clause. This suggests that *wh*-SAs, in contrast to cleft-SAs, are truly a type of in-situ construction. This surmise is supported by the fact that *wh*-SAs do not show island effects, as (11) illustrates (cf. (9) for cleft-SAs).

- (11) a. John invited it is obvious how many people to his party.
- b. John invited how many people is obvious to his party.
- c. John invited I lost the memo that says how many people to his party.
- d. John invited Mary got angry when she heard how many people to his party.

In (11b) the *wh*-expression *how many people* is inside a subject island (assuming that the *wh*-expression occupies the specifier of a sluiced CP); in (11c) it is in a complex NP; and in (11d) it is in an adjunct island. Nevertheless, these examples are not at all degraded in comparison to (11a), where *how many people* is contained in the complement of a bridge predicate.

The analysis of *wh*-SAs as in-situ constructions is further supported by the contrast with (12), where the same *wh*-SA pattern has undergone overt fronting of the constituent containing the *wh*-expression. As expected, this pattern shows island effects.

- (12) a. John invited how many people you'll never guess to his party.
- b. John invited how many people it is obvious to his party.
- c. ?*John invited how many people I lost the memo that says to his party.
- d. ?*John invited how many people Mary got angry when she heard to his party.

Note that what is fronted in (12) is not the *wh*-expression alone, but the CP containing the *wh*-expression and the sluiced S including its trace. This explains why neither the original SA pattern nor its syntactic movement variant in (13) shows “Empty Category Principle–type” effects with adjunct *wh*-expressions.

- (13) a. John went I wonder if anyone knows why to Kathmandu.
 b. John went why I wonder if anyone knows to Kathmandu.

What has been fronted in (13b) is not the bare *wh*-adjunct *why* but the complement CP containing it; this is why extraction is possible over a *wh*-island. An in-situ analysis of (13a) might posit abstract movement deriving the representation in (14).

- (14) John went [_{CP}[_{CP}[_{CP} why [IP]]] [I wonder if anyone knows _{t_{CP}} to Kathmandu]].

Again, the absence of effects expected from (abstract) movement of an adjunct out of a *wh*-island is likewise explained in (14) by the fact that what is moved is not *why* but the CP containing it.

We have suggested that what undergoes movement in cleft-SAs is a null expression corresponding to the presupposition in a cleft, whereas the corresponding constituent in *wh*-SAs is the sluiced CP-in-situ containing the *wh*-expression. The fact that movement in the former is syntactic whereas no syntactic movement occurs in the latter explains the contrast with respect to island effects in the two patterns.⁸

The remaining fact to be explained about the *wh*-SA pattern is how the head of the pattern is related to the *wh*-expression in the sluiced CP. We have noted that although there is evidence that the parallel relation in cleft-SAs is established by movement, in *wh*-SAs the evidence suggests the opposite. Furthermore, there is a strong generalization that a *wh*-operator, once moved to a position where it takes scope, is not available for subsequent movement; the *wh*-element in the *wh*-SA pattern has already moved within the sluiced CP. Although the entire CP containing the *wh*-expression may move, as in (12a–b), it need not.

We suggest that the relationship between the *wh*-expression and the head of the *wh*-SA pattern is established by binding. It has long

⁸ Note that the results of both types of movement satisfy predication requirements under an indexation-through-predication approach.

- (i) John invited [[_{NP} e]_i [_{CP}[_{CP} how many people_i [IP]]
 [you'll never guess <sub>t_i]]] to his party. (= (1))
 (ii) John is going to [[_{NP} e]_i [[_{CP}]_i [_{CP} I think [_{CP} that it is Chicago_i <sub>t_i]]]]
 on Sunday. (= (6a))</sub></sub>

In (i) the index of *how many people* is percolated to the CP that contains it. Movement (abstract or syntactic) of this CP to the specifier of the CP immediately containing it results in association of that index with the higher CP; this is the index required for a predication relationship with the empty matrix NP. In (ii) (syntactic) movement of the empty CP to the specifier of the CP containing it results in transmission of the index to that CP in a similar fashion, as discussed above.

been known that *wh*-interrogative expressions in languages such as Chinese and Japanese have the form of indefinite pronouns (Kuroda 1965), and this fact has been related to their ability to serve as variables in situ (Nishigauchi 1986). More recently Chung, Ladusaw, and McCloskey (1995) have argued that *wh*-expressions “recycled” in the LF representations of sluiced sentences in English likewise function as indefinite pronouns, that is, as potential variables. Under this approach the antecedent for the sluiced IP in (15a) has the form shown in (15b) (Chung, Ladusaw, and McCloskey 1995:257).

- (15) a. Bill wondered how many papers Sandy had read, but he didn't care which ones.
 b. [_{IP} Sandy had read [how many papers]]

In (15b) the indefinite expression *how many papers* is free and thus able to serve as a variable bound by the *wh*-phrase of the sluice (*which ones*) after the antecedent IP (15b) is “recycled” into the position of the sluice in (15a).

A parallel analysis extends to the *wh*-SA pattern as analyzed in (2), given that the *wh*-phrase *how many people* is free in (2b). On an approach where the relative head and the relative clause are related by binding of a position in the latter by the former, *how many people* is available to be bound in the basic structure proposed by Lakoff.

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