Model Abstracts

The LSA Program Committee has prepared model abstracts which members may wish to consult before preparing submissions. Thanks to the authors of these abstracts for their agreement to make their abstracts available and for annotating them. Each abstract appears with annotations which are indented and boldfaced.

Matthew Adams. Poetic correspondence in Welsh

Whether poetic forms are constrained by the same principles as prose and speech is a long-standing question in metrics. I present evidence from Welsh poetry that bears directly on this question, and argue that Long-Distance Consonantal Agreement (LDCA) is instantiated by the alliterative *cynghanedd* verse form. LDCA is a non-local phonological process visible in many languages in the form of consonantal harmony (Rose & Walker 2004, Hansson 2001, 2004) or vowel harmony (Rhodes 2009). I demonstrate that Agreement by Correspondence (ABC, Rose & Walker 2004), a formalization of LDCA, can be extended to analyze Welsh *cynghanedd* meter. ABC introduces output-output and input- output identity constraints that enforce long-distance similarity between segments sharing a specified subset of phonological features.

In the first two sentences of the introduction, the author introduces the theoretical context of the discussion and the language-specific phenomenon that will address an aspect of the relationship between prose and speech. The rest of the paragraph is devoted to spelling out in more detail the method by which the author will examine the relationship, primarily by providing citations of work within the broader theoretical context. The last sentence clearly states why the chosen framework, Agreement by Correspondence, is relevant to alliterative poetry: because it has been used to analyze cases of long-distance agreement in spoken language production, it also has value in providing an analysis for constrained, conventionalized language use. In sum, the paragraph makes clear the theoretical issue at hand, the literature that bears on this, and the way the author proposes to approach the issue.

The following example illustrates salient characteristics of this verse form:

l**f**o**r**, au**r** o | **f**ae**r**w**r**iaethlfor, a fair golden stewardship
De**g**, yw'**r f**au, | **d**ie**gr** o **f**ae*th*is mine, sweet nurture
(Lake et al. 2007)

For expository clarity, a bar (|) marks the division into half-lines. Within each line, the bolded consonants to the left of the bar correspond *absolutely* with the bolded consonants to the right of the bar; their linear order and segmental identity are preserved. Any consonants following the last vowel of each half-line (au and ae, in the second line) are not repeated. Thus, the final italicized 'th' sequence has no correspondent in the left-hand side of its line. Line division is determined by a pitch-accent prominence that falls on the ultima of some non-final word (indicated by underlining). *Cynghanedd* does not require that consonants in the left half occupy the same syllabic position as their counterparts in the right half (viz., the 'r' segments in the first line are first codas, then onsets).

This paragraph clearly but succinctly demonstrates how the alliterative form works and looks. A single example is provided, with each line translated. Consistent typographical conventions are used to highlight important, relevant aspects of the alliterative form, in particular, the strict linear order in which the consonants appear on both sides of the poetic half-line by using boldface type. Needlessly jargony language is intentionally avoided: the word "half-line" is used instead of "hemistich," since it largely means the same thing in this context and doesn't risk impeding communication for non-specialists in the metrics literature.

My analysis makes two theoretical claims. First, following work by Griffen (1996) and Albright (1996), I argue that pitch accent, not lexical stress, defines the domain of ABC: consonants appearing *before* the final pitch accent of each half-line are required to be identical, while those

following the pitch accent are metrically invisible. This simple generalization contrasts with the highly intricate traditional analysis based on stress (Rowlands 1976).

Second, I argue for the utility of an *inventory grammar* (Hayes 2008, Kiparsky 2006), which generates licit metrical forms in optimality-theoretic fashion. Constraints ranked above a strategically placed faithfulness constraint, which treats every output as its own input, are active in restricting the inventory of metrical forms. Constraints below the faithfulness constraint are irrelevant. With respect to *cynghanedd*, I show that a faithfulness constraint must rank below ABC correspondence and identity constraints, which compel LDCA, but that it ranks above constraints that demand the preservation of syllabic place between correspondents.

The preceding two paragraphs establish the concrete theoretical points the author wishes to make. In each case, the implication of the theoretical move is explicitly spelled out, either in contrast to an existing account (the first paragraph) or in agreement with a theoretical device (the second paragraph).

By showing that ABC generates the expected metrical forms, I provide further evidence that the linguistic principles underlying verse, prose, and speech can be analyzed similarly. This intersects with previous research on the relationship between alliteration, rhyme, and non-iconic reduplication (Yip 1999) and promises to illuminate further the dynamic relationship between conventionalized and spontaenous language production.

The final paragraph returns to the theme introduced at the start of the abstract and explicitly states why the proposed theoretical framework, Agreement by Correspondence, succeeds in capturing generalizations across different types of language use (verse, prose, and speech). The author also mentions a set of discussions with which the research interfaces, suggesting that the work is part of a larger project to understand the relationship between conventionalized language acts and spontaneous language production. This has the effect of giving the abstract some momentum; it offers a partial answer to an ongoing, yet unresolved, question in linguistic research.

Geoffrey Pullum, Arnold Zwicky. Licensing of prosodic features by syntactic rules: The key to auxiliary reduction

Auxiliary reduction (e.g. *she's* for *she is*) is well known to be blocked before sites of VP ellipsis (**She's usually home when he's*), pseudogapping (**It's doing more for me than it's for you*), *wh*-movement (**I wonder where he's now?*), etc. Most analyses connect this to empty categories. We show that this is incorrect.

The phenomenon to be examined is made clear, with examples of three of the major constructions in which reduction is blocked. The examples simultaneously illustrate blocking of auxiliary reduction and remind the

reader of what, say, pseudogapping is. The last two sentences of the paragraph alert the reader to previous accounts of the phenomenon and the alternative direction to be taken here. (A general comment about the abstract as a whole: The authors do not coin acronyms for the phenomena under discussion (e.g. 'AR' for auxiliary reduction). This greatly improves the readability of the abstract!)

Selkirk (*Phonology and syntax*, 1984:366) proposes a syntactic condition on prosody: Deaccenting is necessary for reduction, and a phrase-final monosyllable cannot be deaccented. Inkelas & Zec (1993) place the condition on prosodic (not syntactic) phrases, assuming the mapping principle that a dislocated syntactic phrase begins a new phonological phrase. Such accounts fail to predict correctly on comparative subdeletion (**She's a better scientist than he's* [NP *an* [QP Ø]*engineer*]) or examples with subject-auxiliary inversion (**He's taller than's his friend* [AP Ø]). Here the empty category or extraction or ellipsis site does not abut the auxiliary, yet still it cannot reduce. Inkelas & Zec posit (on rather weak arguments) dislocation in subdeletion and pseudogapping and thus predict the lack of deaccenting; but they must allow reduction in subject-auxiliary inversion sentences to get *Who's your friend?*, so they apparently cannot block **He's taller than's his friend* [AP Ø].

This second paragraph cites two of the previous works on the topic, briefly stating their solutions to the problem. Constructions which are problematic for the previous accounts are mentioned, again with an illustration of each construction. (In other words, even if one assumes that the conditions on auxiliary reduction make reference to empty categories, the previous accounts fail to block reduction in some constructions.)

Previously unnoticed is the relevance of rejoinder emphasis with *too/so*, as in *I am TOO gonna fix it!*. Reduction is blocked (**I'm TOO gonna fix it!*)--but here THERE IS NO DISPLACED OR ELIDED CONSTITUENT. This is the key to the constraints on auxiliary reduction. A syntactic condition of rejoinder emphasis calls for light accent on the auxiliary verb and heavy accent on *too/so* (prosodic conditions of this sort on syntactic constructions are not uncommon). But since (as noted by Selkirk) an auxiliary can reduce only when completely stressless, the requirements of rejoinder emphasis and auxiliary reduction clash irresolvably.

Here the authors introduce new data into the discussion: a construction with no empty category/extraction site in which auxiliary reduction is blocked. This construction suggests that any account of auxiliary reduction which appeals to empty categories is misguided. Instead, the authors offer a sketch of the prosodic requirements of this particular syntactic construction, which clash with the conditions required for auxiliary reduction. Note that the authors have signaled the importance of this new data by presenting it in a separate paragraph, highlighting the clause in which they point out there is no empty

category, and stating explicitly that this (in their view) is the key to understanding the problem. These stylistic points not only make it easier for the abstract readers to appreciate the point of the argument, but they also suggest that the authors will succeed in making the oral presentation of this materials clear even to nonspecialists.

All the other constructions mentioned above similarly require lightly accented auxiliaries. For example, the VP ellipsis construction could be described as one in which a VP contains nothing but a lightly accented head (applied semantically to a free variable over VP meanings). What this means is the distribution of reduced auxiliaries can be completely accounted for by Selkirk's stresslessness condition--except that there are certain left context conditions on cliticization (noted by Kaisse 1983), these being the only remaining syntactic conditions on reduction: Auxiliaries cliticize only to (1) subjects, (2) subordinators (*than, that*), (3) proadverbial *so*, or (4) *wh*-words.

Having argued that rejoinder emphasis shows that the conditions on auxiliary reduction cannot crucially depend upon empty categories, the authors now reconsider the constructions containing empty categories, giving as an example the prosodic requirements associated with VP ellipsis.

Our analysis needs no special rule for auxiliary reduction at all. As a matter of morphology, the auxiliaries have (at least) two shapes, one when completely deaccented and one when accented, and the syntax of certain constitutuent types determines light accent on head verbs (something that has to be stated anyway). This analysis offers no support for traces; in fact if traces exist, then Selkirk's condition has to be modified rather awkwardly to say not just 'if it ends a constituent' but 'if it ends a constituent or has as its complement a case-marked trace.'

In the concluding paragraph the authors sum up their general proposal and touch upon a larger theoretical question: the existence of traces.

Joshua Jensen. Jarai wh-questions. Pseudo-clefting, wh-raising, and clause structure

Overview & Goals: This study investigates wh-questions in Jarai (Austronesian, Vietnam; SVO). Using diagnostics in Potsdam & Polinsky (2009), I argue that some clause-initial wh-phrases in Jarai originate as the predicate of a pseudo-cleft structure, while others move from a subcategorized argument position.

The abstract starts right off by stating the topic and the main conclusions that will be argued for.

Evidence: I present four arguments that Jarai has two routes to clause-initial wh-phrases. The

first three pieces of evidence show that in questions containing the particle *pô* (2), the remainder is a headless relative clause, unlike the remainder in questions without *pô* (1). First, the particle *pô* occurs as a dummy head in headless relative clauses outside of wh-questions (3). Second, Jarai uses a special negative form, *ƀu-djŏ' 'neg'*, for negating a DP in copular clauses; this negator can be used in *pô*-questions but not in questions without *pô*. Third, the postnominal demonstrative *anŭn* 'that' can occur at the right edge of a *pô*-question (4a), indicating that the remainder is a DP; but its occurrence at the end of a question without *pô* is unacceptable (4b). The final piece of evidence shows that the wh-phrase in *pô*-questions originates as the predicate. As (5a) shows, *hlơi* can appear clause-finally in a *pô*-question as the complement of an optional copula. However, *hlơi* in non-*pô*-questions cannot be clause-final (5b).

This paragraph fleshes out the details of how the main conclusion will be argued for in a succinct and organized fashion (leaving little doubt that the talk itself will be well-structured and clear—and presentable within a 20-minute format). The prose is transparent enough that, taken in conjunction with the examples provided, the reader can easily understand the arguments so as to see their relevance to the main conclusion that was introduced in the first paragraph.

Further Issues & Analysis: Having argued that the wh-phrase in a *pô*-question is the predicate in a pseduo- cleft structure, the word order in (2) (compared to (5a)) needs to be explained given that canonical word order in Jarai is subject-initial and predicate-final. I propose that (5a) shows the base-generated structure of pseudo- cleft questions, with *hlơi* merged in the predicate. In (2), the wh-phrase has undergone wh-fronting out of the predicate.

The third paragraph addresses an analytical problem that is likely to be spotted by close readers of this abstract, and states how the analytical problem will be addressed.

Theoretical Implications: Cole & Hermon (2008) have argued that SVO clauses in Toba Batak (Austrone- sian; VOS) are derived by VP-fronting followed by raising of the subject. Because Jarai's SVO word order is atypical for western Malayo-Polynesian languages, it would be attractive to show that SVO in Jarai, too, is VP-raising with subsequent subject-raising. Nevertheless, the presence of wh-argument fronting rules out this route to Jarai SVO word order, assuming with Oda (2005) and Potsdam (2009) that VP-raising is inconsistent with wh-raising. Thus, the presence of wh-fronting in Jarai offers a useful diagnostic for adjudicating between different plausible accounts of Jarai SVO clause structure (see Chung (2008) for other arguments).

The abstract concludes by placing the work in a broader theoretical context (but not too broad), by pointing out non-obvious connections with recent proposals for deriving SVO word order. While the conclusion here is perhaps somewhat more tentative, the contribution

that this work offers to these other theoretical issues is made very clear.

Examples:

Examples

Tara McAllister. Patterns of gestural overlap account for positional neutraliation in child phonology

Previous work in developmental phonology (Edwards, 1996; Dinnsen, 1996; Marshall & Chiat, 2003; Altvater-Mackensen & Fikkert, 2010) has described a pattern whereby onset fricatives are replaced with stops or glides, but coda fricatives are realized faithfully.

1. Positional Fricative Neutralization-Stopping (PFN-S):
    1. [tɪk], “sick”

2. [haʊs], “house”

2. Positional Fricative Neutralization-Gliding (PFN-G):
    1. [jak], “sock”
    2. [bʌs], “bus”

PFN is a child-specific phenomenon; it reverses a well-documented bias in adult phonological typology whereby featural contrasts, including manner contrasts, are realized preferentially in syllable- initial position (cf. Beckman, 1997). PFN thus resists modeling with adult formalism. Here it will be argued that PFN is driven by a child-specific articulatory limitation that bans overlap between vowel and fricative gestures. The positional asymmetry then emerges as the consequence of differing degrees of gestural overlap permitted in syllable-initial versus syllable-final position, as encoded in the framework of Articulatory Phonology (Browman & Goldstein, 1985).

The topic of the study is stated and examples are provided to familiarize the reader with the phenomenon under investigation. Background and references establish that this is an unexpected pattern that is of theoretical interest. A general statement of the direction the analysis will take is then provided. For ease of processing, the proposed analysis is divided into two parts that will be addressed in separate paragraphs below.

In a typical, coarticulated fricative-vowel transition, the jaw lowers in anticipation of the vowel while the tongue remains high to maintain the correct aperture for frication (Mooshammer, Hoole & Geumann, 2006). An extensive literature suggests that such dissociated movements of the tongue and jaw are problematic for child speakers, who favor ballistic movements of the tongue-jaw complex (MacNeilage & Davis, 1990; Kent, 1992; Green, Moore, & Reilly, 2002). It has been proposed that this preference for unitary movements of the tongue and jaw can be encoded in a child-specific constraint Move-As-Unit (McAllister, 2009). Crucially, articulatorily simpler segments (stops, glides) can overlap with the vowel without violating Move-As-Unit.

This paragraph briefly explains the hypothesis that articulatory factors lie at the root of child phonological patterns of stopping and gliding. Evidence from studies of speech-motor development and adult articulation is cited to justify the claim. The phonological constraint that will be invoked is introduced with a plain-language explanation.

To avoid violating Move-As-Unit, a speaker might use a non-overlapping transition between fricative and vowel gestures (Figure 1(a,c)). However, articulatory studies have demonstrated that speakers adhere to characteristic patterns of intergestural timing, formalized by Gafos (2002) with the constraints CV-Coord and VC-Coord. Non-overlapping transitions violate these constraints. A key finding in articulatory phonology is that gestural timing patterns differ across onset and coda positions (Krakow, 1999). Experimental evidence reveals that the timing of the

onset-vowel transition is tightly constrained, while the vowel-coda transition is more flexible (Tuller & Kelso, 1990, 1991). This suggests that that CV-Coord >> VC-Coord. Here it is proposed that PFN occurs when a faithfulness constraint such as IDENT-Continuant is ranked between CV-Coord and VC-Coord. In onset position, normal coarticulation is blocked by Move-As-Unit, while the non-overlapped candidate is ruled out by CV-Coord; the winning candidate features substitution of a stop or glide for the fricative target. In coda position, VC-Coord is sufficiently low-ranked that the candidate with non-overlapping gestures is selected, preserving fricative manner. This analysis is supported by acoustic evidence from child speech, with examples of coda fricatives separated from the preceding vowel by more than 300 ms of silence/aspiration. In sum, a problematic child-specific pattern can now be understood as the consequence of a previously established limitation on child speech-motor control, interacting with independently motivated principles of gestural organization.

Figure 1.

Diagram.

This paragraph explains the second part of the analysis, in which it is claimed that the positional nature of neutralization is a reflection of constraints on gestural coordination. Diagrams help the reader visualize the different patterns of gestural overlap under consideration. Evidence from previous literature describing positional influences on gestural timing is presented to justify a claim about constraint ranking (CV-Coord >> VC-Coord). The formal phonological analysis to be pursued is briefly laid out, again with more prose than phonological formalism. Finally, the author describes new acoustic data that will be presented in support of the hypothesis that some child speakers use non-overlapping transitions from a vowel to a coda fricative.

Anna Papafragou. Source-goal asymmetries in language acquisition and memory

Recent research has demonstrated an asymmetry between the origins and endpoints of motion events, with preferential attention given to endpoints rather than beginnings of motion. This asymmetry emerges when children and adults are asked to describe or remember motion events (Lakusta & Landau, 2005; Lakusta, 2005; Arnold, 2001; Regier, 1996; Regier & Zheng, 2003), and surfaces even in the non-linguistic representation of motion in infants (Lakusta, Wagner, O'Hearn & Landau, 2007). Here we explore the link between source and goal categories in language and cognition in two further studies with adults and 5-year-olds. Our aim is to investigate whether the *specificity* of encoding source/goal relations differs in both spatial memory and the acquisition of novel spatial vocabulary.

The main phenomenon to be investigated in the paper (the asymmetry between sources and goals) is presented in the introduction to the abstract. Prior literature is reviewed. The goal of the present abstract is clearly stated and situated in the context of prior studies.

Experiment 1 asked whether specific spatial configurations are represented more accurately if they participate in goal rather than source relations. We created GOAL-SOURCE versions of motion events which were identical except for the path reversal (e.g., a ball moved intoGOAL/out ofSOURCEa container). We then created variants of these events (e.g., a ball moved ontoGOAL/off ofSOURCEthe same container). Each event lasted for 2s, with starting-point and endpoint configurations remaining on the screen for an equal amount of time (500ms). Each participant saw either the source or the goal version of each event and the corresponding variant separated by a mask (1s) and had to say whether they were the same or different. To block verbal encoding, adults and children were given a counting task during inspection of the events. Adults saw a total of 64 pairs of motion events and children a reduced set of 16 pairs. We found that goal changes were detected more accurately than source changes by both adults (MG =.83, MS =.72) and children (MG= .70, MS = .55; all p's<.05).

In this and the next paragraph, experimental materials and procedure are described in some detail. A key factor for the readability of the experimental section is whether the reader is able to reconstruct the studies with some degree of specificity. A summary of results (including statistical information) is presented.

Experiment 2 asked whether the observed asymmetries in memory have implications for spatial word learning. The study builds on previous reports that children overgeneralize separation rather than joining words (Bowerman, 1996), and on evidence that languages make finer distinctions within goal rather than source spatial-semantic fields (Regier & Zheng, 2003). Materials were similar to those in Exp.2 but the task now involved identifying the denotation of a novel path verb (1st event:Look! The ball is glorping the toy!2nd event:Is the ball glorping THIS toy?). We chose path verbs as targets because such expressions are restricted in English and thus made good candidates for novel spatial vocabulary. We found that changes in the goal path were more likely to make participants of either age group reject the novel spatial verb than

identical changes in the source path (adults: MG = .64, MS = .50; children: MG = .62, MS = .49; all p's<.05). In other words, goal distinctions were more precisely drawn in language than source distinctions. Taken together, these studies demonstrate that a cognitive-attentional bias in spatial representation and memory affects the specificity of hypotheses about spatial referents that learners build during the acquisition of spatial language.

The second experiment is clearly motivated and its logic follows from Experiment 1. Notice that various experimental choices (e.g., the choice to use path verbs as targets) are justified throughout. This makes the abstract stronger and improves readability. The abstract ends with a clear conclusion which brings together the data from Experiment 1 and 2 and bears on the theoretical points made in the introduction (i.e., it links the present data to the cognitive source-goal asymmetry and its implications for language learning).

Stephanie Shih, Jason Grafmiller. Weighing in on end weight

The Principle of End Weight maintains that constituents will occur in order of increasing weight (Behagel 1909; Quirk et al. 1985), but the precise definition of "weight" has been heavily debated. Previous proposals have defined weight as syntactic complexity (syntactic nodes or words), processing (dependencies), phonological complexity (lexical stresses), or phonological length (syllables). This paper presents a systematic investigation of the predictive value of these measures on constituent ordering in two constructions in spoken English. We show that weight measured as the number of words or lexical stresses most reliably predicts construction choice.

The first paragraph introduces the phenomenon the authors are investigating and the primary research question addressed in the paper. It also presents a succinct description of the study and the results. The authors define at the outset terminology that is used throughout the abstract.

As approximations of weight in studies of syntactic complexity, counts of orthographical words or syntactic nodes have been widely shown to be reliable predictors of constituent ordering (Wasow 2002; Szmrecsányi 2004; Bresnan and Ford 2010; a.o.). Less well-studied are notions of phonological weight, sometimes operationalized as the number of syllables (Benor and Levy 2006; McDonald et al. 1993). Alternatively, Anttila et al. (2010) proposed that weight be measured by the number of lexical stresses (following Selkirk 1984; Zec and Inkelas 1990), arguing that primary stress count supersedes word count as a more accurate predictor of construction choice in the English dative alternation. They, however, failed to control for the influences of other non-phonological predictors of dative construction choice (see esp. Bresnan et al. 2007). Nor did they account for the high degree of correlation between the two weight measures—the number of stresses increases as word count increases—which potentially masks the effects of both predictors.

The second paragraph provides a thorough and focused contextualization and motivation of the study. The authors engage the relevant literature and identify well-defined issues requiring further investigation, which are addressed by their study in the following paragraph.

We examine the effects of phonological weight by distinguishing the independent influences of the various weight measures using multivariate regression analysis of genitive and dative construction choice in a corpus of spoken American English while crucially controlling for other known predictors of genitive and dative construction choice (Shih et al. 2009; a.o.). Four measures of weight were studied: the number of syntactic nodes, words, lexical stresses, and syllables in the possessor and possessum NPs of the genitive and in the recipient and theme arguments of the dative. A model that includes all of the different weight measures demonstrates that their independent effects are robustly indistinguishable due to their severe collinearity, even after the application of de-correlation methods. Tested independently, word count and lexical stress count are marginally better predictors than the number of syllables or syntactic nodes. An analysis of the data where the number of words and the number of lexical stresses do not coincide moreover finds that one is not a significantly better predictor than the other.

A pithy statement of the project is given, followed by an elaboration on the data and methods used in the study and a clear and concrete description of the results. The description of the study is logically organized and avoids overly involved and specialized terminology.

Finally, we note that in theories of processing complexity (e.g., Dependency Length Theory; Gibson 1998, 2000; Hawkins 1994), dependency measures, which count lexical categories introducing new discourse concepts, are near equivalents to phonological measures of weight and thus can predict the same empirical facts (e.g., Comrie 2003). Given our findings, we argue that word count, as commonly used in many studies of construction choice, is a fitting and sufficient proxy for any number of theoretical approaches to end weight, including phonological ones.

In the final paragraph, the authors consider theoretical implications and possible extensions of their study. The abstract concludes with a final reiteration of the results.

Neal Snider, Jeffrey Runner. Phonological structure is reactivated in VP ellipsis

Though the claim that there is linguistic structure at the VP ellipsis site is fairly uncontroversial, it is still relatively unknown exactly what type of linguistic structure is present. Two standard approaches to ellipsis include, (1) PF deletion: syntactic structure is present throughout the derivation (including LF) but is deleted at the level of phonetic form; and (2) LF reconstruction:

syntactic structure is reconstructed in the ellipsis site for interpretation at logical form. And a third possibility exploits theories of late insertion: syntactic structure is present throughout the derivation but at the point of insertion the actual words with their phonetic forms are not inserted (Harley05). The different approaches make different predictions for the types of information we might expect to find in the ellipsis site. LF reconstruction and late insertion approaches predict the presence of morphosyntactic features and structural representations, but do not contain the actual lexical items with their phonetic forms. In contrast, the PF deletion approach assumes that prior to deletion, a fully specified syntactic representation, containing specific lexical items with phonetic forms, are present. In two experiments, we show that while the semantic neighbors of antecedent words were reactivated during processing of both VP ellipsis and VP anaphora, as evidenced by eye movements, only phonological neighbors were reactivated with VP ellipsis, consistent with a PF-deletion-type analysis of VP ellipsis.

The theoretical literature around VP ellipsis is reviewed with an emphasis on the testable predictions. Then, the particular predictions are introduced for the empirical phenomenon being investigated, eye movements. The theories are treated in an objective fashion, and the evidence is discussed as being consistent with some and not with others. There is no mention of evidence "for" a particular theory, because scientific theories cannot be proven, only disproven, so evidence is only consistent or inconsistent.

We monitored participants' eye movements (Exp1: 21 participants, 24 items; Exp2: 29, 20) to four pictures on screen (from Yee&Sedivy06) as they listened to one of four ellipsis, anaphora (do-it and pronoun), or control sentences ((1)-(4)). The Target picture was the object of the antecedent clause, the Related was semantically related (Exp1) or a phonological cohort (Exp2) of the target, and the others were distractors. Using mixed-model logistic regression, we model how the likelihood of fixation varies over time, controlling for state dependencies on the previous sample (Frank&al08), during a 450ms window starting 150ms after the offset of the verb.

The experimental materials (with reference to examples), procedure, and analysis are described in some detail, although succinctly.

Results
Exp1: significantly more predictive eye movements in the second clause to the Target and semantically Related in Ellipsis (T: $p<<0.001$, R:$p<.05$), and Do-it conditions (T:$p<0.1$, R:$p<0.05$); no significant interaction between the conditions and time: looks to the Target did not significantly change over time for any condition relative to the Intransitive and Pronominal baselines.

Exp2: eye movements significantly increased over time to the Target in the Ellipsis condition ($p<0.1$), Do-it ($p<0.05$), and pronoun ($p<0.001$) conditions; significantly increasing eye movements to the phonologically Related ONLY in the Ellipsis condition ($p<0.05$); no predictive eye movements.

These two paragraphs describe the results succinctly in terms that can be understood without intimate knowledge of the empirical methodology. The results of the statistical analyses are presented in the course of the description of the effects.

Antecedent nominals and their semantic neighbors were reactivated in both VP ellipsis and anaphora constructions, but phonological neighbors were reactivated only in ellipsis. This strongly supports a model in which linguistic structure—including fine-grained phonological and semantic information—is present in the ellipsis site, and is consistent with a PF-deletion analysis for VP ellipsis.

The main finding is repeated and related back to the theoretical predictions. The main theoretical claim that the evidence is consistent with is emphasized; it can be inferred that the data is inconsistent with the others.

Examples:
The security guard opened the lock, (Antecedent) and the night watchman ...

1. did, too. (Ellipsis)
2. did it, too. (Do-it)
3. dropped it. (Pronoun)
4. slept. (Intransitive control)

Examples demonstrate both the structures under investigation and the experimental materials.

Adam Ussishkin, Jerid Francom. Predicting word frequency effects from corpora

It is clear that language corpora play a critical role in indexing exposure to language, but in practice, psycholinguists rely on word frequency (WF) norms drawn from relatively small, primarily written language samples. Recent evidence, however, suggests that frequency norms from traditional sources (e.g., the Brown corpus (Francis and Kučera, 1967) and CELEX (Baayen et al., 1993)) are inferior to larger, conversationally-based samples in predicting language behavior (e.g., SUBTL; TV/Film subtitles, cf. Brysbaert and New (2009) and HAL; Usenet newsgroup forums, Burgess and Livesay (1998)). Our goal here is to provide quantitative evidence to further explore corpus size and sampling method as factors in producing frequency measures that best predict lexical processing behavior.

The first paragraph clearly introduces the problem addressed by the research, and cites appropriate scholarly work to support the research question. Furthermore, this paragraph points out the problem with older and written-based corpora providing baseline measures for

psycholinguistic experimentation, and clearly states the goal of the work.

We approach this by examining word frequency effects, which have long been detected in psychological measures of word processing: high frequency words are responded to quicker (Scarborough et al., 1977), named faster (Forster and Chambers, 1973) and rated as more familiar (Connine et al., 1990) than low frequency words. Despite the central role of corpora in gauging the effect of exposure on language processing, concern for the representativeness of corpora from which WF norms are extracted has largely been restricted to corpus linguistics Biber (1993), with inadequate evaluation of sampling methods by the psycholinguistic community (Burgess and Livesay, 1998; Gilquin and Gries, 2009). However, data from recent investigations pointing to size and register characteristics as key to indexing lexical processing measures (Burgess and Livesay, 1998; Brysbaert and New, 2009) 1) confound corpus size with language register; the largest corpora tested were entirely (BNC; Burnard (1999)) or partially based on British English samples (CELEX) and 2) confound sampling method; no American corpus tested conforms to standard methods in contemporary corpus linguistics. This weakens the hypothesis that conversationally-based corpora are superior to written-based collections.

In the second paragraph, the focus of the research is provided: to investigate the sampling methods used to obtain word frequency measurements. In addition, this paragraph cites earlier research that raises similar problems, yet highlights a gap in this research that needs to be addressed: a serious confound between size of corpus and register of corpus exists, rendering earlier results unreliable.

The current investigation aims to address these confounds by specifically asking whether norms derived from smaller, conversational-based collections predict lexical behavior better than larger, more rigorously sampled corpora of American English. To this end, we extracted WF norms from five corpora differing in size and sampling method, where the primary contrast was between a large (400+ million), representative corpora (COCA; Davies (2008)) and a smaller (51+ million), register-specific (conversational) corpus (SUBTL). Other corpora were included as baselines to match previous studies (HAL and KF). These WF metrics were compared to scalar subjective frequency estimates solicited from 21 American English speakers using methods developed successfully by Balota et al. (2001). Regression results support the hypothesis that conversational register collections, despite being smaller and specialized, outperform large corpora designed using rigorous sampling practices (SUBTL: r=.45; HAL: r=.35; COCA: r=.34; KF: r=.31; BNC: r=.18).

Here, the methodology of the current research is clearly laid out. The authors use five test corpora, and compare word frequency measures from each to subjective frequency judgments provided by native speakers. Then, the authors provide their results in a very clear, single sentence summarizing the conclusion: conversational corpora do a

better job than larger corpora predicting word frequency effects.

In conclusion, these results suggest that conversational register language samples, even those modest in size, provide superior estimates of language exposure when compared to well-sampled, written collections, even those significantly larger in size. These results also advocate for a closer relationship between corpus linguistics and psycholinguistics, especially with respect to corpus representativeness.

In the final paragraph, the authors restate the conclusion, illustrating once again that corpora drawn from conversational registers correlate better with psycholinguistic results. Additionally, the authors conclude tying these results to broader implications with an appeal to more interaction between corpus linguistics and psycholinguistics.

Unacceptable Abstract Sample: Licensing of prosodic features by syntactic rules: The key to auxiliary reduction

This paper will discuss the phenomenon of auxiliary reduction, a topic which has been treated by many syntacticians and phonologists.

Future tense is a bad sign; is the paper not written yet?
No example given of the construction.
No specific citations of previous work.

We will show that traces do not exist and that any theory assuming traces is gravely flawed and must be abandoned.

Sweeping claim and tendentious, obnoxious tone.
Writer assumes that readers know that many previous accounts appeal to the mechanism of traces, instead of making the connection explicit.
No specific criticisms are made of the earlier accounts.

We will propose that in the morphology, every auxiliary has two shapes, one when the auxiliary is completely deaccented and one when the auxiliary is accented. (There may be more than two shapes for the auxiliaries.)

Solution given here with no preamble.
Hard to see why this solution is justified.
The issue of the number of shapes of each auxiliary is given more prominence than it is worth.
The parenthetical comment is distracting.

Constructions such as VP ellipsis and *wh*-movement in which auxiliary reduction is impossible are ones in which only the accented form of auxiliaries may appear, due to syntactic conditions

on accent patterns and on what may serve as the host for a clitic.

Only vague reference is made to the relevant syntactic conditions; compare the good abstract, where the accent patterns of two constructions are described and the possible hosts listed (with a specific reference to earlier work).

This also handles comparative subdeletion and pseudogapping, which have been claimed to involve dislocation in order to preserve the generalization that when there is an empty category next to the auxiliary it cannot reduce, which is not necessary with our proposal.

No examples of the syntactic constructions referred to.
No specific citations of others' work.
Stylistic problems such as run-on sentences and vague pronominal reference make the abstract more difficult to follow.

It may also be noted that our solution will account for the impossibility of auxiliary reduction before emphatic *too* or *so* in rejoinders and in comparative constructions with subject-auxiliary inversion.

The rejoinder construction is the key point of the paper! In the good abstract it is given central prominence, but here it is buried and could easily be missed.
No example of the construction in question.
Writer should state explicitly that no empty category can be posited as the explanation for this instance of blocked auxiliary reduction.
The inversion facts raise problems for analyses appealing to empty categories, and they also deserve more than an offhand remark in the abstract.

In conclusion, the results of this paper will have profound effects on linguistic theory in general.

Inflated, empty conclusion.

Frequently Asked Questions

Why Major in Linguistics?

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Thought?

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