How Not to Undermine the Goal of an Acquisition Theory

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Much of Drozd's penetrating review of Inverstigations in Universal Grammar (IUG), beyond noting countless lamentable misattributions of ideas and inconsistencies. evaluates its methodology. The standardization of methodology in IUG can, I believe, undermine our understanding of acquisition more than it supports it. It is a pity if we lose the real value of their useful experimental refinements of existing experiments (and several novel ones in semantics).

To rescue the valuable contributions, we need to ask again the classic question: what is the acquisition problem? It remains: how does a child select the grammar of his community from a set of biologically defined possible grammars.? Logic, acquisition data, intuitive evidence, and experimental evidence jointly help us to answer this question. The goal is not to explain actual or apparent stages in acquisition, which are probably illusory and inconsistent, but rather how purely UG factors intersect (interface) other mental abilities to make the choice of one grammar among many an inevitable mechanical process. No evidence is guaranteed to be pertinent, nor direct proof of a claim, just like relativity is supported by evanescent observations. In sum: no evidence is completely reliable, and no evidence should be ignored.

If we assume Continuity of UG, then all properties of all grammars are within the mental power of all children (with the possible exception of SLI). IUG claims that children should therefore rarely deviate from English. The opposite consequence follows: children should, hundreds of times, be momentarily misled into non-English grammars by the diverse and fragmentary properties of actual dialogue. "Errors" often provide direct insight into UG mechanisms.

Therefore we expect that the child would pass through numerous foreign grammars, and they do (just outlined here), from Italian the familiar pro-drop (Hyams (1986)) to German medial wh- (deVilliers et al (1990) to Asian no AGR "where go" (Roeper and Rohrbacher (2000), to German V-

2 “what calls that” to West African perfectivity “eat berry” Terry (2002, In prep)), to Walbiri quantifier-spreading Bach et al (1989) Philip (1995),, to Salish definite article (Matthewson, Bryant, and Roeper (2001)), to interesting interlanguage bilingual examples (Tracy (1991). The inversion, wh-errors, and quantification behavior in IUG offer further support.

The outcome will be a language where subgrammars remain in syntax and semantics (Lebeaux (2003)) with for instance V-2 for quotation, just as the lexicon retains Latinate, Anglo-Saxon, and Greek morphology. All people including children are speakers of Multiple Grammars (Roeper (2003), Yang (2003), Chomsky (2003), Müller (2003), Ionin, Ho and Wexler (2003)).

Hundreds of other instances where children make a leap into another grammar may be very short-lived and invisible, yet reveal the essence of how new grammars are projected. Acquisition is still in the *field linguist stage* where we seek the basic perimeters of child grammar---even anecdotes may reveal a grammar radically different from the trarget.

What methodology identifies these phenomena? The strongest arguments arise when there is “independent” evidence. No statistic measures the power of “independent evidence” though it is a cornerstone of real science.
For instance if a child says “Don’tickle me, I’m laughable”and there is experimental evidence that children take “the elephant is pushable” to mean “the elephant pushes” (see Johnson (2001)), then the combination makes the argument strong that the child analyzes –able as [+potential] but not with the passive property: subject = object. (By comparison, the interesting Principle B errors in comprehension (John washed him) are not buttressed by any naturalistic data) We learn from child –able that the acquisition mechanism can look for a minimal feature to justify an affix, [+potential] but without [+subject = object], so “passive” can be acquired in parts.

Are there any impossible features? No language (to my knowledge) has an affix [+contradictory] or many other features. Is there confirmation for this view of the micro-structure of acquisition. One child just adjoins:
"are you want one"/"are this is broke"/"are you don't know Sharon's name is” (Akmajian pc) where are is briefly an abstract yes/no marker without tense, number or agreement. Examples like “was this is the boat I saw” (Adam-Brown Corpus (Childes)) show that inversion can be combined with long-distance copying of Tense in UG.

Each of these examples involves Merge—just the combining of two elements with one as Head instead of Move (to match (“Check”) a Feature). Thus acquisition data gives us vital information about the projection powers of an acquisition device. Van der Lely (2003) shows SLI cases like

deVilliers (1991) where Merge without Move exists in wh-movement. Acquisition poses the question: what feature bundles are recognizeable objects for Merge ?

To dismiss these examples as "performance" or because they are not reproduced experimentally fails to acknowledge the value of "independent evidence" and would eliminate all work based on CHILDES (e.g. root infinitives). UIG's use of typical psychological methodology is heavy-handed because that methodology was developed to explore areas with no deductive theory. Experimental research within a deductive theory allows a much less restrictive approach, since the theory itself has independent sources.

Every field develops via greater magnification of the phenomena itself (not much by the surrounding numbers). Acquisition research will languish if only a few structures are looked at over and over, and if the stunningly brief moments of creativity do not come under serious examination.

What defines the core productive grammar? Snyder and Roeper (2003) suggest that a primary feature is recursivity. Recursive structures (like pronominal adjectives in English and post-nominal adjectives in French) separates, for the child, the core from the periphery of each grammar.

How does UG apply? Roeper (1982) argued that UG itself is an input and output filter on all child grammar. Chomsky commented "minimalist conditions hold for all states of the language faculty, including the initial state". If true, then real grammatical decisions are virtually never misled by experimental context. Children who are faced with a sentence like:

2) Was the policeman eaten by the hamburger

with a Plausible Assent scenario [a policeman eats a hamburger] may say "yes". Have they been misled by context? The same children will say "no" if asked:

3) Did the hamburger eat the policeman?

For (2) they are misled by context because their passive is insecure allowing a reduction to an active. So misleading contexts only mislead if the child has a grammar that allows it. A secure grammar is not dislodged by context for (3), just as adults are not for (2). Ignoring Plausibile Deniability, if it reveals an interesting grammar, may be <u>exactly</u> what we should do to elicit unusual hypotheses that the child is entertaining. IUG has come around to my view (in general, not in particular, as Drozd shows) that performance factors do not explain children's deviations from target grammars---UG explains them. IUG has however moved that style of "performance"

criticism over to the claim that experimental contexts mislead children. But the argument is just as wrong there as it was for the claim that "performance factors" created the disparity between child and adult.

What of quantifier-spreading (Philip (1995))? Children's treat every as if it were an adverbial "always" (via Event semantics). It best fits spreading to an unmentioned extra pair: Does every dog have a bone "Not this rabbit and carrot". Independent evidence should arises to show the robustness we seek (most totally ignored by IUG):

1) No grammar lacks adverbial quantification. Many lack DP quantification. (see cross-linguistic evidence Bach et al).
2) Strong quantifers (most) entail reference to the object, which is a kind of adult spreading: Many Scandinavians won the nobel prize = many with respect to prize-winners, not Scandinavians.

These facts alone should lead to the acquisition hypothesis even if there were no acquisition evidence.

3) Spreading occurs in the acquisition of at least six languages.
4) Spreading occurs where no question is asked, therefore deniability is irrelevant. Children directed to mark some of the circles are black mark some of the circles have some black (Roeper and Matthei (1975)).
5) Natualistics data (Stickney (2003) allows "most" to mean "mostly" (See Drozd (2000)).
6) Spreading appears with other quantifiers (all, some, most).

Even if children show some knowledge of DP-every, their deviations are surely not arbitrary (Meroni et al (to appear)), but reflect knowledge of a UG-relation (probably Weak quantification).

It is the collective force of these arguments that is important, We cannot neglect any methods in the linguists' toolkit. Building a successful acquisition theory is more like assembling a skeleton in archeology than manipulating statistics in psychology.

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