

Approaching UG from Below

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The problem that has virtually defined the serious study of language since its ancient origins, if only implicitly, is to identify the specific nature of this distinctive human possession. Within the “biolinguistic perspective” that began to take shape fifty years ago, the concern is transmuted into the effort to determine the genetic endowment of the faculty of language FL, understood to be a “cognitive organ,” in this case virtually shared among humans and in crucial respects unique to them, hence a kind of species property. So construed, language is I-language, a state of FL, and universal grammar (UG) is reinterpreted as the theory of the initial state of FL. The term “biolinguistics” itself was coined in 1974 by Massimo Piattelli-Palmerini as the topic for an international conference he organized for the Royaumont Institute in Paris and MIT, bringing together evolutionary biologists, neuroscientists, linguists, and others concerned with language and biology, one of many such initiatives, before and since, which sought to explore the extent to which apparent principles of language are unique to this cognitive system, one of “the basic questions to be asked from the biological point of view,” as discussed there, and crucial for the study of development of language in the individual and its evolution in the species.¹

Within the biolinguistic framework, methodological considerations of simplicity, elegance, etc., can often be reframed as empirical theses concerning organic systems generally. For example, Morris Halle’s classical argument against postulating a linguistic level of structuralist phonemics was that it required unmotivated redundancy of rules, taken to be a violation of natural methodological assumptions. Similarly conclusions about ordering and cyclicity of phonological and syntactic rule systems from the 1950s were justified on the methodological grounds that they reduce descriptive complexity and eliminate stipulations. In such cases, the issues can be recast as metaphysical rather than epistemological: Is that how the world works? The issues can then be subjected to comparative analysis and related to principles of biology more generally, and perhaps even more fundamental principles

¹Piattelli-Palmerini, ed., *A Debate on Bio-Linguistics*, Endicott House, Dedham Mass, May 20-21, 1974.

about the natural world; clearly a step forward, if feasible. Such options become open, in principle at least, if the inquiry is taken to be the study of a real object, a biological organ, comparable to the visual or immune systems, the systems of motor organization and planning, and many other subcomponents of the organism that interact to yield the full complexity of thought and action, abstracted for special investigation because of their apparent internal integrity and special properties. From the earliest days there have been efforts to explore closer links between general biology and the biolinguistic perspective. Insofar as methodological arguments in linguistics can be reframed as empirical ones about general operative principles, the analogies may become more substantive.

At the time of the 1974 discussions, it seemed that FL must be rich, highly structured, and substantially unique. In particular, that conclusion was drawn from considerations of language acquisition. The only plausible idea seemed to be that the process is a form of theory construction. Somehow, the child reflexively categorizes certain sensory data as linguistic, not a trivial achievement in itself, and then uses the constructed linguistic experience as evidence for a theory that generates an infinite variety of expressions, each of which contains the information about sound, meaning, and structure that is relevant for the myriad varieties of language use. It was well understood that construction of theories must be guided by what Charles Sanders Peirce had called an “abductive principle” that “puts a limit upon admissible hypotheses,” so that the mind is capable of “imagining correct theories of some kind” while discarding infinitely many others consistent with the evidence. Peirce was considering theory construction in the sciences, but the same general observation holds for growth/acquisition of language.² In this case, it appeared that the format that limits admissible hypotheses must be highly restrictive, given the empirical facts of acquisition and convergence. The conclusions about the specificity and richness of the language faculty seemed to follow directly. Plainly such conclusions pose serious problems for dealing with the diversity of languages: the well-known tension between descriptive and explanatory adequacy. The conclusions also raise barriers to inquiry into how the faculty might have evolved, since any property specific to language calls for an evolutionary explanation. These matters were discussed repeatedly, and inconclusively, at the 1974 conference.

The crystallization of the Principles & Parameters program a few years later suggested ways to reduce the tension between descriptive and explana-

²Though not, to be sure, the notion of convergence to the correct theory, as in Peirce’s concerns.

tory adequacy, as is familiar. It also removed a major conceptual barrier to the study of evolution of language. With the divorce of principles of language from acquisition, now understood to be a matter of parameter setting, it no longer follows that the format of UG that “limits admissible hypotheses” must be rich and highly structured to satisfy the empirical conditions of rapid convergence on generative systems of the kind required to determine meaning and external manifestation. That might turn out to be the case, but it is no longer an apparent conceptual necessity.

The P&P approach largely emerged from intensive study of a range of languages, but it was also suggested by major developments in general biology, specifically François Jacob’s account of how slight changes in the hierarchy and timing of regulatory mechanisms might yield great superficial differences – a butterfly or an elephant, and so on. The model seemed natural for language as well: slight changes in parameter settings might yield superficial variety, through interaction of invariant principles with parameter choices (Chomsky 1980, p.67). The P&P framework also made it possible to pursue more seriously the recognition, from the earliest days of generative grammar, that acquisition of language involves not only a few years of experience and millions of years of evolution, but also “principles of neural organization that may be even more deeply grounded in physical law” (Chomsky 1965, p.59). Again, somewhat parallel developments were proceeding in general biology, now sometimes called the “evo-devo revolution.”³

Evidently, development of language in the individual must involve three factors: (1) genetic endowment, which sets limits on the attainable languages, thereby making language acquisition possible; (2) external data, converted to the experience that selects one or another language within a narrow range; (3) principles not specific to FL. Some of the third factor principles have the flavor of the constraints that enter into all facets of growth and evolution, and that are now being explored intensively in the “evo-devo revolution.”⁴ Among these are principles of efficient computation, which would be expected to be of particular significance for generative systems such as I-language. Insofar as the third factor can be shown to be operative in the design of FL, explanation can proceed “beyond explanatory adequacy” in the technical sense,

³Sean Carroll (2005). More generally, see Gould (2002, Part II).

⁴There are other third factor elements as well, among them properties of the human brain that determine what cognitive systems can exist, though too little is yet known about these to draw specific conclusions about the design of FL. It also might turn out that general cognitive principles that enter into language acquisition pose conditions on FL design. On the role of such principles in acquisition, see particularly Yang (2002).

raising new questions: not only asking what mechanisms suffice to determine I-language from data available, but why these mechanisms should exist, and whether they are real or just dispensable descriptive technology. The task of accounting for the evolution of language would also be correspondingly eased, for the same reasons that hold for inquiry into evolution generally: the less attributed to genetic information (in our case, the topic of UG) for determining the development of an organism, the more feasible the study of its evolution.

Recent inquiry into these questions in the case of language has come to be called “the minimalist program” MP, but there has been so much misunderstanding, even within professional circles, that it is perhaps worth reiterating that it is a *program*, not a *theory*, and a program that is both traditional in its general flavor and pretty much theory-neutral, insofar as the biolinguistic framework is adopted. Traditional efforts to identify what is distinctive to FL have implicitly abstracted from third factor effects (and from generative processes as well, for the most part). And whatever one’s beliefs about design of language may be, the questions of the research program arise. It may also be worth mentioning that the program can only be pursued, whatever theoretical framework one adopts, insofar as some descriptive account of the phenomena to be explained is reasonably unproblematic, often not the case of course, as expected with any system of at least apparent intricacy.

Throughout the modern history of generative grammar, the problem of determining the character of FL has been approached “from top down”: How much must be attributed to UG to account for language acquisition? The MP seeks to approach the problem “from bottom up”: How little can be attributed to UG while still accounting for the variety of I-languages attained, relying on third factor principles? The two approaches should, of course, converge, and should interact in the course of pursuing a common goal.

One useful way to approach the problem from below is to entertain the strong minimalist thesis SMT, which holds that FL is “perfectly designed.” The first task would then be to formulate SMT coherently. The next would be to determine how close it is to true. Naturally, neither task is well-defined a priori, and each is sure to be modified in the course of inquiry. There are various ways to construe SMT, and any specific choice allows various paths that might be followed to investigate its reach. I would like to review where I think we stand after a few years of serious engagement with these issues adopting some choices that seem reasonable though certainly not logically necessary, to suggest a few refinements, and to indicate some of the manifold problems that arise in seeking to close the gap between SMT and the true

nature of FL.⁵ UG is what remains when the gap has been reduced to the minimum, when all third factor effects have been identified. UG consists of the mechanisms specific to FL, arising somehow in the course of evolution of language.

An I-language is a computational system that generates infinitely many internal expressions, each of which can be regarded as an array of instructions to the interface systems, sensorimotor (SM) and conceptual-intentional (CI). To the extent that third factor conditions function, the language will be efficiently designed to satisfy conditions imposed at the interface; one can imagine more radical theses, to which I will briefly return. We can regard an account of some linguistic phenomena as *principled* insofar as it derives them by efficient computation satisfying interface conditions. We can therefore formulate SMT as the thesis that all phenomena of language have a principled account in this sense, that language is a perfect solution to interface conditions, the conditions it must at least partially satisfy if it is to be usable at all.

In its most elementary form, a generative system is based on an operation that takes structures already formed and combines them into a new structure.⁶ Call it *Merge*. Operating without bounds, Merge yields a discrete infinity of structured expressions. Hence Merge, and the condition that it can apply without bound, fall within UG.

A Merge-based system will be compositional in general character: the interpretation of larger units at the interfaces will depend on the interpretation of their parts, a familiar observation in the study of every aspect of language. If the system is computationally efficient, once the interpretation of small units is determined it will not be modified by later operations – the general property of strict cyclicity that has repeatedly been found. Operations will also typically yield nested rather than crossing dependencies, also a familiar observation (and where crossing dependencies are found, it is commonly, and plausibly, taken to be the result of more complex processes). Thus in “The men who John V₁ V₂ ...,” agreement universally holds between *John* and V₁ and between *the men* and V₂, not conversely. There is no obvious reason for this in terms of communicative or parsing efficiency; as is well-known,

⁵I will assume here the general framework of my “On Phases,” (to appear); and sources cited there.

⁶There are more complex possibilities, some familiar: e.g., phrase structure grammars, which fall within Emil Post’s version of recursive function theory, abandoned for well-known reasons, empirical and conceptual. Another complication beyond pure Merge is adding the principles of associativity and ordering, suppressing hierarchy and yielding sequences.

dependencies quickly overflow memory, so that language use adopts various methods that give it a paratactic flavor. But these familiar properties are an automatic consequence of generation relying on Merge with appropriate compositional conditions. One task of MP is to clarify and test these general ideas, and place them in a broader setting.

A Merge-based system of derivation involves parallel operations. Thus if X and Y are merged, each has to be available, possibly constructed by (sometimes) iterated Merge. The process has a loose resemblance to early theories of generalized transformations, abandoned in the early 1960s for good reasons, now resurrected in a far simpler form for better reasons. But a generative system involves no temporal dimension. In this respect, generation of expressions is similar to other recursive processes such as construction of formal proofs. Intuitively, the proof “begins” with axioms and each line is added to earlier lines by rules of inference or additional axioms. But this implies no temporal ordering. It is simply a description of the structural properties of the geometrical object “proof.” The actual construction of a proof may well begin with its last line, involve independently generated lemmas, etc. The choice of axioms might come last. The same is true of generation vs production of an expression, a familiar competence-performance distinction. But even if one were to take the intuitive interpretation literally, generation of an expression is not strictly “bottom-up,” because of the parallelism of operations. A strict “bottom-up” interpretation is, for example, compatible in principle with the assumption that in performance, the first XP (say a noun phrase) is produced or perceived first, even if later merged into some ultimately embedded expression (as internal or external argument, for example). Or many other assumptions about use of language.⁷ In addition to Merge applicable without bounds, UG must at least provide atomic elements, lexical items LI, each a structured array of properties (*features*) to which Merge and other operations apply to form expressions. These features contain information relevant to the way their arrangements are interpreted at the interfaces: all information insofar as I-language satisfies the Inclusiveness Condition, a natural principle of efficient computation.⁸ A particular language is identified at least by val-

⁷E.g., perception models based on Bradley Pritchett’s “bottom-up” theta-attachment model. For discussion, and exploration of new ideas and empirical results highly relevant to considerations here, see Reinhart (2006).

⁸The condition is radically violated in the mapping to the SM interface, even more so in strong versions of Distributed Morphology that take all phonological features of LIs to be inserted in this mapping. It is also violated in standard versions of mapping to CI, but that raises non-trivial questions about the architecture of cognitive systems, difficult to examine because of limited information about their language-independent nature.

uation of parameters and selection from the store of features made available by UG, and a listing of combinations of these features in LIs (the lexicon), satisfying further conditions that we put aside here. There is substantial evidence that human LIs are crucially distinct from the symbolic elements of other animals at both interfaces. At the CI interface, they lack the kind of relation to mind-independent elements that appears to be a general property of animal communication systems; something similar is taken for granted for phonological elements. If so, there is no *reference*-like relation for human language, hence no semantics in the technical sense of Frege, Peirce, Tarski, Carnap, Quine, and others, or contemporary “externalist” theorists of reference. The reasons have been discussed elsewhere and I will put them aside here, but if so, these are further genetically determined components of FL (or the conceptual resources on which it draws), and a problem to be addressed in study of evolution of language (or of the pre-linguistic conceptual resources available to humans).

In addition to such properties as these, UG must contain the principles that map external data to linguistic experience, providing the basis for language acquisition. The extent to which these properties and their organizing principles are unique to FL could be clarified by comparative studies, but there is little doubt that it is substantial.

The conclusion that Merge falls within UG holds whether such recursive generation is unique to FL or is appropriated from other systems. If the latter, there still must be a genetic instruction to use Merge to form structured linguistic expressions satisfying the interface conditions. Nonetheless, it is interesting to ask whether this operation is language-specific. We know that it is not. The classic illustration is “the mathematical capacity,” which troubled Alfred Russel Wallace 125 years ago because it “is wholly unexplained by the theory of natural selection, and must be due to some altogether distinct cause,” if only because it remained unused. One possibility is that it is derivative from language. If the lexicon is reduced to a single element, then Merge can yield arithmetic in various ways. Speculations about the origin of the mathematical capacity as an abstraction from linguistic operations are familiar, as are criticisms, including apparent dissociation with lesions and diversity of localization. The significance of such phenomena, however, is far from clear. They relate to use of the capacity, not its possession; to performance, not competence. For similar reasons, dissociations do not show that the capacity to read is not parasitic on the language faculty, as Luigi Rizzi points out.

Suppose the single item in the lexicon is a complex object, say some visual

array. Then Merge will yield a discrete infinity of visual patterns, but this is simply a special case of arithmetic and tells us nothing about recursion in the visual system. The same would be true if we add a recursive operation – another instance of Merge – to form an infinite lexicon on the model of some actual (if rather elementary) lexical rules of natural language, say an infinite array of visual patterns as “lexical items.” Again that introduces nothing new, beyond FL. Similar questions might be asked about the planning systems investigated by George Miller and associates 45 years ago. If these and other cases fall under the same general rubric, then unbounded Merge is not only a genetically determined property of language, but also unique to it. Either way, it falls within UG as one of the organizing principles of recursive generation of expressions.

$\text{Merge}(X_1, \dots, X_n) = Z$, some new object. In the simplest case, $n = 2$, and there is evidence that this may be the only case (Richard Kayne’s “unambiguous paths”). Let us assume so. Suppose X and Y are merged. Evidently, efficient computation will leave X and Y unchanged (the No-Tampering Condition NTC). We therefore assume that NTC holds unless empirical evidence requires a departure from SMT in this regard, hence increasing the complexity of UG. Accordingly, we can take $\text{Merge}(X, Y) = \{X, Y\}$. Notice that NTC entails nothing about whether X and Y can be modified after Merge.⁹

Suppose X is merged to Y (introducing the asymmetry only for expository reasons). Trivially, either X is external to Y or is part of Y: external and internal Merge, respectively; EM and IM (Move). In the latter case, X is not only a part of Y but necessarily a *term* of Y in the technical sense. Without further complication, Merge cannot create objects in which some object O is shared by the merged elements X, Y. It has been argued that such objects exist. If so, that is a departure from SMT, hence a complication of UG.¹⁰

If an element Z (lexical or constructed) enters into further computations, then some information about it is relevant to this option: at the very least, a property that states that Z can be merged, but presumably more, it is commonly assumed.¹¹ The optimal assumption is that this information is provided by a designated minimal element of Z, a lexical item W (Z itself, if it is an LI), which is detectable by a simple algorithm; the *label* of Z, the head

⁹Let us put aside here the question whether in addition to “set-Merge” there is also an operation “pair-Merge,” as discussed in my “Beyond Explanatory Adequacy,” (Chomsky 2004).

¹⁰See Citko (2005). Also Svenonius (2005), on “banyan trees.” Citko argues that parallel Merge is “predicted” as IM is, but that is not quite accurate. It requires new operations and conditions on what counts as a copy, hence additional properties of UG.

¹¹Actually, a delicate question, having to do with interpretation of deviance at the interface.

projected in X-bar theories – possibly a dispensable notion, as discussed below. The label *W* of *Z* enters into EM in selection in various ways as well as into interpretation of *Z*. Since *W* contains all information relevant to further computation involving *Z*, *W* is also necessarily the *probe* that selects a *goal* in any internal modification of *Z*. Minimal search conditions limit the goal of the probe to its complement, the smallest searchable domain. It is impossible, for example, for IM to move a SPEC of *W* (or one of its terms) to a higher SPEC position, or for an agreement relation to be established between *W* and an element within its SPEC. Or conversely, unless the SPEC is itself a head, an option barred under narrower phase-theoretic conceptions of the role of the probe in controlling operations.

Restricted to heads (probes), *c-command* reduces to minimal search. The standard broader notion can be defined in terms of dominance and sisterhood, and a choice has to be made between immediate dominance and some higher projection. But it is not clear that this extension beyond minimal search – a natural computational principle – is necessary. There seems no clear independent reason to believe that sisterhood is a relevant relation. Furthermore, to capture the intended asymmetry, both for EM and IM, choice of projection (labeling) must also be introduced into the definition. For such reasons, the broader notion departs from SMT, on reasonable assumptions, and requires empirical motivation. It has been assumed to be relevant to binding theory, hence presumably to the CI interface, but that may be unnecessary,¹² leaving scopal relations as possible instances of *c-command* in the broader sense. I know of no other evidence that it plays a role in narrow syntax or the mapping to CI. At the the SM interface side, the idea that the broader notion of *c-command* determines linearization is the core principle of Kayne's LCA and the very fruitful work it has inspired, and if the foregoing is correct, LCA can plausibly be interpreted as part of the mapping to the SM interface. That requires some device to deal with ordering of merged LIs, either (as in Kayne's work) a further elaboration of Merge and *c-command* to allow non-branching nodes, or some other departure from SMT, non-trivial it appears. Fukui and Takano review other stipulations that seem necessary, and argue in favor of a head parameter (for which they cite additional evidence, bearing on linear ordering in narrow syntax but not broader *c-command*). They do note one residue of LCA that is unaccounted for by a head parameter: the near universal SPEC-H ordering – which is narrowed to subject-H ordering unless second-Merge (hence SPEC) is banned within complex VPs and other

¹²See Chomsky (to appear), and below. See also Hasegawa (2005), analyzing reflexivization in terms of multiple-agree.

such structures, a conclusion that is by no means obvious.¹³

It is, however, not clear that the SPEC-H residue would qualify as support for LCA, because of the array of stipulations required to yield the result, some just reviewed. One might want to explore other directions. To mention one, it would not be implausible to seek a parsing account for properties of ordering; often justified (e.g., rightward displacement of complex phrases). One thought that might be pursued, for example, is that for a Pritchett-style parser based on theta-attachment (see note 7), if the external argument precedes the theta-assigner α , then all theta roles are available in the immediate projection of α when it is reached in linear search, simplifying the operations. Similar ideas might extend to the left periphery, on Rizzi-style assumption that the head carries the information about the status of the SPEC. Speculations aside, one general conclusion that seems clear is that LCA involves significant departures from SMT, and therefore must be supported by sufficient empirical evidence.

NTC has always been assumed without comment for EM: there is, for example, no proposal that if V and NP are merged to form VP, then V is merged inside NP. Under SMT, it should hold for IM as well. Assuming so, then an application of IM yields two *copies* of X.¹⁴ There is no rule of formation of copies or remerge, as has sometimes been supposed; just IM applying in the optimal way, satisfying NTC. Repeated IM yields many copies. There must be a procedure to distinguish copies from independent repetitions; that is easily stated with a proper notion of cyclicity in terms of *phases*, to which we return: all and only repetitions formed by IM within a phase are copies.

In a well-designed FL, lacking arbitrary stipulations, both EM and IM should be permitted, and the two kinds of Merge should be expected to yield different interface properties. That is obviously true at the SM interface – the ubiquitous property of “displacement” – and appears to be true at CI as well. The two types of Merge correlate well with the duality of semantics that has been studied from various points of view over the years. EM yields generalized argument structure, and IM all other semantic properties: discourse-related and scopal properties. The correlation is close, and might turn out to be perfect if enough were understood. If so, the conclusions so far conform

¹³Fukui and Takano (1998). The head-parameter approach they adopt is developed in earlier work of Naoki Fukui, and Fukui and Mamoru Saito. Their primary example is head-final Japanese. For indirect but intriguing empirical evidence supporting LCA for Japanese, see Kayne (2004).

¹⁴Traces, indices, etc., are barred by NTC and Inclusiveness. Hence carry a considerable empirical burden.

to SMT.

It also follows that it was a mistake – mine in particular – to suppose that displacement is an “imperfection” of language that has to be assigned to UG or somehow explained in terms of its special functions. On the contrary, its absence would have to be accounted for by a UG stipulation barring IM. It therefore follows that some form of transformational grammar – by now a radically stripped-down version of early proposals – essentially “comes free.” If some other device is developed to account for the empirical phenomena of displacement and associated interpretations, it will require a stipulation barring IM and further stipulation of the additional mechanisms, therefore facing a considerable empirical burden.

Unless an element Z is an isolated element (an interjection, or frozen expression), hence of no interest here, its label W must have a feature indicating that Z can be merged. Under NTC, merge will always be to the edge of Z, so we can call this an *edge feature* EF of W.¹⁵ If EF is always deleted when satisfied, then all expressions will be of the form LI-complement; in intuitive terms, they branch unidirectionally. If EF is not deletable, then the elements of expressions can have indefinitely many specifiers (*complement* and *specifier* mean nothing more in this framework than *first-merged* and *later-merged*). Variation among LIs with regard to deletability of EF would be a departure from SMT, so we assume that for all LIs, one or the other property holds. Empirical evidence reveals that SPECs exist, that is, that EF is undeletable. That leaves the question why. SM interface conditions seem to be irrelevant, so we have to look to third factor effects and CI conditions. Both support the choice of undeletability. Only that choice permits IM, which comes free, so if expressive potential is to be used, EF must be undeletable. As for CI, undeletability provides for the duality of semantics.¹⁶

The property of unbounded Merge reduces to the statement that LIs have EF. The property has to be stated somehow, and this seems an optimal way. So far, then, the only syntactic properties of UG are that it contains Merge and LIs with undeletable EF, and that expressions generated must satisfy interface conditions – in a principled way, insofar as SMT holds.

¹⁵There are several interpretations of “merge to the edge,” including a version of *tucking-in* in Norvin Richards’s sense. I will put the matter aside here.

¹⁶As an uninterpretable feature, EF cannot reach the interface, so presumably deletion of EF is an automatic part of the operations of transfer. Note that the same cannot be assumed for the standard uninterpretable features, which can be deleted only when certain structural conditions are satisfied, and will crash the derivation otherwise. A reviewer points out that if EF is always deleted when satisfied, then one case of IM is permitted: self-merge of an LI, which is enough to yield the basic elements of arithmetic.

CI clearly permits interpretation of quantification in some manner. Language should provide such a device if expressive potential is to be adequately utilized. There are various logically equivalent devices, among them variable-free logics. The most familiar notation is operator-variable constructions. But that device virtually comes free, given EM and IM expressing the duality of semantics at CI – which may be why it is the most commonly used formal device, and the easiest to learn. In the simplest case, the copy merged to the edge by IM is the operator taking scope over the copy that had previously been merged by EM, the latter understood as the variable; the full structure of the two copies provides the interpretation as a restricted variable, hence yields the options for reconstruction along lines that have been pursued very productively in recent years. These considerations take us a step towards establishing the A/A'-distinction as a property of language with a principled explanation in terms of SMT.

In the cases just mentioned, the apparent optimization of design is relative to the CI interface. That raises the question whether the examples are idiosyncratic in this respect or whether the property holds generally. If the latter, then the relation of the generative procedure to the interfaces is asymmetrical, CI taking precedence: optimization is primarily to the CI interface. The question can be approached on empirical grounds, from various directions. One is along the lines just illustrated: by investigating language design. The ways language deals with IM provide additional evidence of priority of the CI interface. As noted, NTC requires that all copies should be retained under IM: the initial copy is introduced by EM, and all others are introduced by IM. At the CI interface the conclusion is correct, at least to good approximation, as illustrated by reconstruction. It is, however, radically false at the SM interface, where all copies other than the final occurrence generated are deleted, with fairly systematic exceptions not relevant here. Here conditions of computational efficiency and of ease of communication are in conflict. Computational efficiency yields the universally attested facts: only the final position of IM is pronounced, dispensing with the need for multiple applications of the generally complex and language-variable operations of morphology-phonology (and whatever else may be part of the mapping to the SM interface).¹⁷ But that leads to comprehension problems. For perception, major problems, fa-

¹⁷Note that the issue does not arise in the mapping to CI if, as generally assumed (and plausibly so, on “poverty of stimulus” grounds), it is universal, hence in effect instantaneous and costless. It must be the highest copy that remains or there will be no detectable evidence that IM applied overtly. The observations here refer to overt movement, but they generalize if we adopt the approach to overt/covert movement by Jon Nissenbaum (2000).

miliar from parsing programs, are to locate the “gaps” associated with the element that is pronounced, problems that would largely be overcome if all occurrences were pronounced. The conflict between computational efficiency and ease of communication appears to be resolved, universally, in favor of computational efficiency to satisfy the semantic (CI) interface, lending further support to speculations about its primacy in language design.

There are other well-known cases where language design is dysfunctional for language use: island phenomena for example, which require circumlocution or special devices (e.g., resort to otherwise-barred resumptive pronouns) to allow expression of simple thoughts. Insofar as island phenomena can be reduced to design efficiency, they would lend further support to theses about primacy of the CI interface.¹⁸

The question can be approached from other directions too. Perhaps relevant are discoveries about sign languages in recent years, which provide substantial evidence that externalization of language is at least partially modality-independent. Among these are striking cases of invention of sign languages by deaf children exposed to no signing and by a community of deaf people who spontaneously developed a sign language. In the known cases, sign languages are structurally very similar to spoken languages, when the modality itself does not require differences. They also are reported to follow the same developmental patterns from the babbling stage to full competence. They are distinguished sharply from the gestural systems of the signers, even when the same gesture is used both iconically and symbolically, as Laura Petitto has shown. She and her colleagues have also studied children raised in bimodal (signing-speaking) homes, and have found no preferences or basic differences. Her own conclusion is that even “sensitivity to phonetic-syllabic contrasts is a fundamentally linguistic (not acoustic) process and part of the baby’s biological endowment,” and that the same holds at higher levels of structure. Imaging studies lend further support to the hypothesis that “there exists tissue in the human brain dedicated to a *function* of human language *structure* independent of speech and sound,” in her words. Studies of brain damage among signers have led to similar conclusions, as has comparative

¹⁸Of interest in this connection is the investigation of interaction of syntactic structure and derivation with principles that facilitate communication, typically neo-Gricean, involving some form of “reference-set computation” (Reinhart 2006); see Chierchia (2004). A question that might be pursued is the extent to which these inquiries presuppose a pragmatic environment based on trust and intent to communicate effectively, as contrasted with one based on intent to deceive and mislead (or others). If the presupposition turns out to play a role, the ideas developed might be reinterpreted within interpretive components of thought, external to language strictly speaking, using its mechanisms in one rather than another way.

work by Tecumseh Fitch and Marc Hauser indicating, they suggest, that the sensorimotor systems of earlier hominids were recruited for language but perhaps with little special adaptation. Similar conclusions about the primacy of the semantic interface have been advanced by prominent evolutionary biologists. The ideas trace back to the cognitive revolution of the 17th century, which in many ways foreshadows developments from the 1950s.¹⁹

Generation of expressions to satisfy the semantic interface yields a “language of thought.” If the assumption of asymmetry is correct, then the earliest stage of language would have been just that: a language of thought, used internally. It has been argued that an independent language of thought must be postulated. I think there are reasons for skepticism, but that would take us too far afield.

These considerations provide a very simple thesis about a core part of the evolution of language, one that has to be assumed at a minimum, so it would seem, by any approach that satisfies the basic empirical requirement of accounting for the fact that the outcome of this process is the shared human property UG. At the minimum, some rewiring of the brain, presumably a small mutation or a by-product of some other change, provided Merge and undeletable EF (unbounded Merge), yielding an infinite range of expressions constituted of LIs (perhaps already available in part at least as conceptual atoms of CI systems), and permitting explosive growth of the capacities of thought, previously restricted to the elementary schemata but now open to elaboration without bounds: perhaps schemata that allowed interpretation of events in terms of categorization by some property (hence predication, once Merge is available), actor-action schemata, and a few others that might well have earlier primate origins. Such change takes place in an individual, not a group. The individual so endowed would have the ability to think, plan, interpret, and so on in new ways, yielding selectional advantages transmitted to offspring, taking over the small breeding group from which we are, it seems, all descended. At some stage modes of externalization were contrived. Insofar as third factor conditions operate, UG would be optimized relative to the CI interface, and the mappings to the SM interface would be the “best possible” way of satisfying the externalization conditions. Any more complex account of the evolution of language would require independent evidence, not easy to come by; and some account is needed for any complication of UG

¹⁹Petitto (2005). Work of Hauser and Fitch cited in Hauser, et al. (2002). See my “Three Factors in Language Design,” (2005), and “Some simple evo-devo theses: how true might they be for language?,” in *Evolution of Human Language: the Morris Symposium*, SUNY at Stony Brook, October 2005, forthcoming.

that resists principled explanation. A common assumption of paleoanthropology is that emergence of language led to the “great leap forward” exhibited in the archaeological record very recently, and the spread of humans all over the world shortly after, all within an eye-blink in evolutionary time.

Various considerations, then, seem to converge rather plausibly on the conclusion that language may be optimized relative to the CI interface, with mapping to the SM interface an ancillary procedure, and complex to the extent that the SM interface has no prior adaptation to these needs. Insofar as SMT holds, generation of structures mapped to CI will be optimal for the CI interface and common to languages apart from parametric and lexical choices (phenomena that require explanation), while phonology, morphology, and whatever else is involved in externalization might be variable and complex and subject to large-scale historical accident, satisfying the linking condition in ways that are as good as possible. That is not a bad first approximation to what the study of language seems to yield.

A more radical conception of the FL-CI interface relation, developed by Wolfram Hinzen, is that “certain empirical properties of thought contents” derive from the structures generated optimally by FL: we are, for example, led to postulate propositions as “intermediate entities between what’s in the head and what’s out there in the physical universe” on the basis of the role of CP in syntactic generation and hence mapping to CI, so that we can “deflate” these mysterious entities “into the notion of a CP”; and the same with other postulated entities of thought. Thus optimally designed FL “provides *forms* that a possible human structured meaning may have, leaving a residue of non-structured meanings (concepts), a substantive amount of which we share with other animals that lack syntax (or at least do not use it, or do not use it for the purposes of language).” These forms are natural objects “that we can study as such, even though we see them, somewhat miraculously, systematically condition properties of linguistic meaning that we can empirically attest,” a novel approach to what has been called “naturalization of meaning.” It is “as if syntax carved the path interpretation must blindly follow” (quoting Juan Uriagereka) (Hinzen 2006, p.179, p.235, p.250). One might extend similar ideas to duality of semantics and other notions of the theory of meaning. From this perspective, propositions and other postulated entities of thought go the way of reference, eliminated from the theory of mind and language. The primacy of CI is reduced, though satisfaction of CI conditions cannot be entirely eliminated: CI must have some range of resources that can exploit the properties of generated expressions, along with whatever is involved in use of language to reason, refer, seek to communicate perspicuously, and

other mental acts. SMT and the concept of principled explanation would be correspondingly simplified.

Returning to the main track, what further properties of language would SMT suggest? One is a case of Occam's razor: linguistic levels should not be multiplied beyond necessity, taking this now to be a principle of nature, not methodology, much as Galileo insisted and a driving theme in the natural sciences ever since. We are assuming that FL provides at least instructions for the CI and SM interfaces, the former having priority (perhaps near-tautologically, insofar as the more radical thesis can be sustained). But postulation of any linguistic levels beyond that departs from SMT, and requires justification. Others are postulated in familiar conceptions of language. Thus in versions of EST (the "Y-model"), three internal levels are postulated, each with its specific properties: D-structure, S-structure, and LF.²⁰ Reliance on Merge as the sole operation dispenses with D- and S-structure, in fact, renders them unformulable (the same with any other notions of underlying and surface structure). It has to be shown that nothing is lost (or better, that something is gained) by this simplification. That appears to be true. If so, we are left only with the internal level LF.

As noted, Merge yields compositional/cyclic properties of the kind that have repeatedly been found. Optimally, there should be only a single cycle of operations. EST postulated five separate cycles: X-bar theory projecting D-structure, overt operations yielding S-structure, covert operations yielding LF, and compositional mappings to the SM and CI interfaces. With the elimination of D- and S-structure, what remains are three cycles: the narrow-syntactic operation Merge (now with overt and covert operations intermingled), and the mappings to the interfaces. As noted earlier, optimal computation requires some version of strict cyclicity. That will follow if at certain stages of generation by repeated Merge, the syntactic object constructed is sent to the two interfaces by an operation Transfer, and what has been transferred is no longer accessible to later mappings to the interfaces (the phase-impenetrability condition PIC). Call such stages *phases*. Optimally, they should be the same for both subcases of Transfer, so until shown otherwise, we assume so (the mapping to the SM interface is sometimes called "Spell-Out"). LF is now eliminated, and there is only a single cycle of operations. The cyclic character of the mappings to the interfaces is largely captured, but not completely: there may be – and almost certainly are – phase-internal

²⁰I am using LF in the sense defined in EST: the output of narrow-syntactic operations and the input to the mapping to CI. Commonly the term has come to be used to refer to the output of that mapping, so that some other term is needed for LF, within this model or others like it.

compositional operations within the mappings to the interfaces. And with phases in place, the problem of distinguishing copies from repetitions is resolved, since all copies are formed by IM at the phase level, hence identifiable for Transfer (the same observation extends to successive-cyclic movement). Whatever phases are, it is clear that PIC is restricted to the complement of the phase head, since specifiers of the phase label/head P can be raised in later phases, P as well.

Still keeping to SMT, all operations are driven by labels. Since at least some operations are restricted (by definition) to the phase head, the simplest assumption would be that all operations apply there. But there must be a departure from this assumption: syntactic objects cannot expand unless EM applies at every stage of derivation. The simplest conclusion, then, seems to be that operations other than EM all apply at the phase level. There is considerable evidence to support that conclusion, which I will adopt here. It follows at once that CP must be a phase, since A'-movement is to its edge, a specifier of the label/probe C (a cover term for the elements of Rizzi's left periphery). If the conclusions about duality of semantics and the IM-EM distinction are correct, then C must be the locus of A'-movement to capture scopal properties, so that the phasal character of CP may follow directly from CI-interface conditions and optimal computation, hence SMT. The general line of argument seems clear enough and plausible, though there are gaps to fill.

Phases should be as small as possible, to maximize the effects of strict cyclicity, hence computational efficiency. Let's adopt the (fairly conventional) assumption that verbal phrases are of the form v-VP, where v can be v*, the functional category that heads verb phrases with full argument structure, unlike unaccusatives and passives. Possibly the functional category v determines the verbal character of the root R that is its complement, along lines discussed by Alec Marantz (1997), in which case verbal phrases are of the form v-RP. Problems arise if phases are associated with every operation of Merge – e.g., with VP (or RP). One reason is that at VP, information is not available as to whether the complement of V will be spelled out in situ or raised by IM, or what its structural Case will ultimately be (so that crash at both interfaces is inevitable). Whether similar conclusions hold at the CI level depends on murky questions as to how argument structure is assigned. For example, can additional material (subject, PP, etc.) determine the semantic relation of V-NP (or R-NP)? Take, say, “(the teacher) left the class (with a problem to solve),” “the class left.” Under the most natural mechanisms of argument assignment, it is not obvious that the semantic relation of

“leave” and the two nominal phrases is determined at the V-NP level. And there are approaches to far more intricate cases for which the assumption appears to be radically wrong.²¹ If VP is not transferable to CI, then for unaccusative/passive (and probably many other structures), the smallest domain within which the V-object relation can be assigned its semantic (theta) role is above vP (in fact CP, with TP still awaiting discussion); and for others the smallest domain is v*P.

Another line of argument that reaches the same conclusions is based on uninterpretable features: structural Case and redundant agreement. Since the values of these features are determined by context, the simplest assumption is that they are unvalued in the lexicon, thus properly distinguished from interpretable features, and assigned their values in syntactic configurations, hence necessarily by probe-goal relations. Keeping to structural NOM-ACC, NOM (and associated agreement) is assigned at least as high as TP (in fact, CP, we conclude below), and ACC (and associated agreement) is assigned within v*P, independently of the choice of higher Case (e.g., *(for him) to accept the job, accept the job!, (his, him) accepting jobs*). On reasonable assumptions that have been familiar since Vergnaud’s original ideas on structural Case, valuation always takes place though it is only sometimes manifested. Accordingly, Case-agreement relations are fixed in the configurations v*P and CP, hence by the probes v* and C-T.²² Object agreement is within this configuration.

Valuation of uninterpretable features clearly feeds A'-movement (e.g., in “whom did you see?”). Hence valuation is “abstract,” functioning prior to transfer to the SM interface, as are the uninterpretable features themselves. A-movement is a more subtle case, but the same conclusion appears to hold. Evidence for that conclusion is provided by Eric Reuland’s discovery of locality-bound (hence syntactic) reflexivization in which the antecedent does not c-command the reflexive but both are c-commanded by the head that agrees with the antecedent: structures of the form [T...XP...R], where T and XP agree, XP does not c-command R, both XP and R are in the search domain of C-T, and XP binds R – indirectly via the common probe C-T. Again, this must be prior to transfer to the SM interface, hence “abstract.”²³

If transferred to the interface unvalued, uninterpretable features will cause

²¹For review, analysis, and sources, see Marantz (2005).

²²On generalization to what he calls “stem features,” either agreement or focus, see Miyagawa (2006).

²³Reuland (2005). See also Legate (2005). Principles of phonetic realization of uninterpretable features are a different matter, though not unrelated.

the derivation to crash. Hence both interface conditions require that they cannot be valued after Transfer. Once valued, uninterpretable features may or may not be assigned a phonetic interpretation (and in either case are eliminated before the SM interface), but they still have no semantic interpretation. Therefore they must be removed when transferred to the CI interface. Furthermore, this operation cannot take place after the phase level at which they are valued, because once valued, they are indistinguishable at the next phase level from interpretable features, hence will not be deleted before reaching the CI interface. It follows that they must be valued at the phase level where they are transferred, that is, at the point where all operations within the phase take place and the Transfer operation therefore “knows” that the feature that has just been valued is uninterpretable and has to be erased at (or before) CI. Since all operations take place at the phase level, there is no memory or search problem.²⁴ It follows again that v^*P must be a phase along with CP.

The next question is whether TP is also a phase, as is suggested by surface phenomena of valuation of uninterpretable features and A-movement. From observations of Marc Richards, it follows that the PIC entails that TP cannot be a phase, with operations of valuation and A-movement driven by properties of T.²⁵ Suppose TP were a phase. Then its interior will be transferred by PIC, but the head T will retain its valued uninterpretable features. The derivation will therefore crash at the next phase, for the reasons just given. Hence the relevant phase for these operations must be CP, not TP. It is, therefore, not only unnecessary but incorrect to add an additional phase TP – the preferred outcome on grounds of computational efficiency, obviously.

For the same reason, Richards points out, the uninterpretable features of C must be “inherited” by T. If they remain at C, the derivation will crash at the next phase.²⁶ Note that TP cannot be saved as a phase by the same device: if

²⁴Alternatives can be devised, but all involve additional search and memory, hence are barred unless empirical evidence is provided for this departure from SMT.

²⁵Personal communication, August 2005. See Richards (2006).

²⁶A residue may remain at C for phonetic interpretation at the next phase, as sometimes found. That is worth exploring in detail, in a variety of language types, but appears to require only minor technical adjustment. It might be that what appears phonetically at C, in some cases at least, is the result of subsequent concord, not agreement. The principled issues arise at CI. Richards’s argument supports the conclusion about inheritance in “On Phases,” (Chomsky to appear) there relying partly on empirical observations based on sometimes subtle judgments with interaction of several factors, hence less compelling than Richards’s conceptual argument. It is sometimes felt intuitively that “inheritance” is counter-cyclic, but technically that is not the case, any more than the (somewhat similar) probe-goal relation that determines structural Case in situ, for example).

its features are inherited by v^* , the derivation will always crash because the external argument is outside the search domain of v^* .

From elementary conceptual considerations then, plausibly traceable to SMT, we conclude that v^*P and CP are the phases of the clausal skeleton, and that the uninterpretable features of C are assigned to T, which does not head a phase.

There are further reasons for expecting that TP is not a phase. T has the basic properties of uninterpretable features. It may yield a phonetic reflex, but its ϕ -features are determined by its context, so it should enter the lexicon without values for these features. T bears these features if and only if it is selected by C, hence it should inherit these from C (the precise mechanism does not matter here). The biconditional holds of embedded clauses, but it would make no sense to hold that in root clauses T has different properties. It therefore follows that root clauses must have C, even if it is unpronounced, as is also indicated by other phenomena; e.g., clausal operators in A' -positions outside TP, hence SPEC-C.

What is true of agreement features appears to hold as well for tense: in clear cases, T has this feature if and only if it is selected by C, though C never (to my knowledge) manifests Tense in the manner of ϕ -features in some languages. If that is basically accurate, then there are two possibilities. One is that Tense is a property of C, and is inherited by T. The other is that Tense is a property of T, but receives only some residual interpretation unless selected by C (or in other configurations, e.g., in English-like modal constructions).²⁷ One advantage of the latter option is that T will then have at least some feature in the lexicon, and it is not clear what would be the status of an LI with no features (one of the problems with postulating AGR or other null elements). Another advantage would be an explanation for why C never manifests Tense in the manner of ϕ -features (if that is correct). Under the former option, with Tense inherited by T, Richards's argument does not independently apply, because tense is interpretable. His argument would also apply, however, if the mechanism of inheritance is generalized (that is, simplified) to all inflectional features of C, not just ϕ -features.

For the same reasons, the inheritance mechanism is simplified if it is generalized to phase heads generally, not restricted to C but extended to v^* as well. But as Richards observes, that is necessary anyway, for the same reasons that require that C assign its features to T. Therefore V (or R) must re-

²⁷It would not suffice to have T bear tense with a condition that C must select tense, since that would leave the possibility of tensed T without C, which is impossible in embedded clauses (and root clauses too, for the reasons just given).

ceive ϕ -features from v^* . It follows that just as a nominal phrase can raise to SPEC-T within CP, so it should be able to raise to SPEC-V within v^*P . There is good evidence for that, going back to work of Paul Postal's on "raising to object" 30 years ago, reformulated and extended by Masatoshi Koizumi, Howard Lasnik, and Mamoru Saito.²⁸ I personally resisted their evidence and tried to find ways to evade it for some years, because the operation appears to make no sense. It has no visible effect, since V raises to v^* , restoring the original order; and there is no semantic motivation at all, though there are semantic consequences. But we now see that there is in fact motivation for this strange and purposeless operation, with its scopal and binding consequences; namely, it follows from SMT. These curious phenomena thus yield further support to the idea that FL may indeed be well-designed to satisfy CI interface conditions (or more radically, that these conditions in part simply reflect SMT).

Note an asymmetry, at least for the languages we are now considering: T may or may not raise to C, but V must raise to v^* , which therefore is an affix. There are other asymmetries: what we are calling V has semantic content, and may simply be a root with v^* serving as the functional element determining its category, as mentioned earlier. In contrast, C has independent content and is not categorizing a root (and whether T has semantic content remains open). In other languages v^* (and v generally) has morphological content, perhaps always affixal. A broader range of languages should, obviously, be considered seriously before conclusions can be drawn about relations among these properties.

Assume the predicate-internal subject hypothesis, which is quite natural on conceptual and semantic grounds: argument structure is determined by EM, in terms of relations within the verbal (or predicate) phrase. For a subject with uninterpretable Case, the value must be assigned by some higher phase head (which will also permit it to undergo A-movement). As we have just seen, that will always cause the derivation to crash unless there is a head selected by C which can inherit its inflectional features, namely T. Again, consequences follow at both interfaces. Displacement to SPEC-T is permitted, with phonetic effects as well as familiar consequences for scope, weak crossover, anaphora, and discourse-related "edge" properties. Problems also remain. It appears that at least some element must raise from v^*P , but if so, the reasons remain controversial.

Richards's observation also provides an argument as to why T should exist

²⁸For discussion and sources, see Lasnik (2003). These approaches assume raising to AGR-O, not V, but it is not clear that the extra assumptions are necessary.

at all. Uninterpretable features of C must be inherited by an element selected by C, for his reasons, but it cannot be v^* , for the reasons mentioned. Therefore T or some counterpart must exist, selected by C and above v^* . Why then should T appear in clauses not selected by C: ECM and raising constructions? A possibility is along the lines already suggested for other cases. The UG principle that inserts T before vP is generalized, thus preventing automatic crash at a later stage if C is merged by EM.²⁹

The ϕ -features inherited by T probe for agreement and Case-assignment, but a question arises as to whether that happens before or after they are inherited from C, at the phase level. If raising is contingent on the probe-goal relation (as seems plausible from intervention effects), then the inheritance operation must precede probe by the ϕ -features (putting aside here complications about separate probing for person and number), so that T serves as the probe at the C level, not C. Otherwise, there will be no A-movement, contrary to what is empirically observed. There might be a reason for this ordering in terms of computational efficiency: the ordering inheritance-probe yields shorter search by the probe and shorter raising. If reasoning along these lines is tenable, then the A/A'-distinction would follow on computational grounds, yielding the basis for duality of semantics.

Let's look a little more closely at the general character of the mechanisms involved, which can be made precise in various ways – keeping here to NOM-ACC languages, and abstracting from interference effects, quirky Case, double objects, and other complications.

Consider a single phase of the schematic form $\{P, XP\}$, where P is the phase head, C or v^* . P assigns its inflectional features to the label L of XP, T or V. These labels then probe XP to find the closest matching goal. For $P = v^*$, it is the object of V, subject being outside the search domain even if v^* itself is the probe, not V. For $P = C$, it is either the subject of v^*P , or the object of V if v is unaccusative/passive. The uninterpretable features of L receive the values of the goal, which is assigned Case in accord with the properties of P: NOM if $P = C$, ACC if $P = v^*$. If there are several goals, then all are valued in the same way, as is the probe. Thus in a participial construction of the form $[P L (\text{participle})^n \text{object}]$, Case of the object is NOM if $P = C$ (and $L = T$), ACC if $P = v^*$ (and $L = V$; possibly an ECM construction). P and any participles have the inherent inflectional features of the object and the participles share its Case (presumably an option because of the categorial nature of participles).

²⁹Presumably control structures are CPs. The status of small clauses raises independent questions.

Probe-goal agreement may or may not be accompanied by IM. If it is not, then the goal is realized in-situ³⁰; if it is, then the goal moves step-by-step as far as it can, reaching SPEC of the probe that has inherited ϕ -features from the phase head.³¹ The intermediate copies reach the CI interface and can have semantic effects: for binding, as in “John seems to her [*John* to appear to X to have left]” (X = himself, her, *herself, *him, *Mary (by Condition (C))); for scope, “Every child doesn’t *every child* seem to his father [to be smart],” with “every child” binding “his” but with scope below negation,³² in both cases with lower copies italicized. These are basic properties of A-movement.

Note that the notion “label” is playing only an expository role here. In constructions of the form H-XP (H a head), minimal search conditions determine that H is the designated element (label) that enters into further operations. H will be the probe, and wherever selection enters – possibly only at the CI interface – H is the only functioning element, whether selecting or selected. Questions arise about labeling only for XP-YP constructions. For IM, with XP raised from YP with head Y, Y is the probe, and the simplest assumption is that it remains the probe, so that XP need not be searched for a new probe.³³ The most problematic case is XP-YP formed by EM. The primary example is when XP is an external argument EA. But numerous questions arise about the correct analysis (e.g., why does the full v*P including EA never raise, or remain without something extracted from it? Is EA in situ a real phenomenon? Why do idioms typically exclude EA?). It may be that as understanding progresses, the notion “label” will remain only as a convenient notational device, like NP, with no theoretical status.³⁴

In any event, reference to labels (as in defining c-command beyond minimal search) is a departure from SMT, hence to be adopted only if forced by empirical evidence, enriching UG.

Consider the Reuland local anaphora cases of the form [C [T...XP...R]], where T and XP agree, XP does not c-command R, and both XP and R are in the local search domain of T. T inherits features from C, and the features

³⁰Unless raised for some other reason. See Chomsky (2001). It is not unlikely, I think, that the observations extend to English constructions involving inversion.

³¹In this case at least, no recourse to the activity condition is needed. See Nevins (2004).

³²Sauerland (2003). The consequences Sauerland draws for phase theory do not apply under the assumptions of “On Phases” (Chomsky to appear). For many problems, see Howard Lasnik, “On a Scope Reconstruction Paradox.” reprinted in Lasnik (2003).

³³Unless XP itself is a head, in which case there is an ambiguity. See “On Phases” (Chomsky to appear) and sources cited.

³⁴For a different approach to similar questions, see Collins (2002), with the notion locus replacing label. But neither may be needed.

of T, XP are valued by the probe-goal relation. TP is transferred to CI, erasing the uninterpretable features that have been valued within the CP phase. But these features cannot be erased before they reach CI, because the T-R relation establishes anaphora – that is, an interpretation by CI making use of the structure presented to it at the interface (much as articulators follow “instructions” given in the phonetic form). Thus there is a strong sense in which transfer to CI at the phase level is “instantaneous,” with the structure mapped to CI preserved for CI interpretation. Mapping to the SM interface is sharply different, as well known, another CI-SM asymmetry, consistent with earlier conclusions.

It seems that basic properties of A-movement fall out fairly naturally, though many questions remain unanswered. Among them are the perennial problems of EPP and of why language should have uninterpretable features at all, both involving serious residual problems, which are left as UG stipulation if they receive no principled answer.³⁵ One approach to the existence of uninterpretable features might be to consider more carefully their consequences. For reasons just discussed, they compel phases to be as small as possible consistent with IM and (possibly) assignment of argument structure, CP and v*P, and they impose cyclicity of Transfer (strict cyclicity, given PIC), thus reducing memory load in computation. Hence they contribute to SMT. They also signal anomaly of derivation quickly, without recourse to selectional features of lexical items that are more naturally understood as part of the interpretive processes at CI. Thus the eventual anomaly of, say, “how many trains did you say that John arrived t’” is detected by CI at the earliest possible stage, the lowest v*P. That could be a factor expediting efficient performance.

Many other questions arise when we look more closely at how the principles function; for example, what are the intermediate positions for A-movement?³⁶ The product of A-movement is an expression with one or more copies of an element that initially entered the computation by EM, receiving its role in argument structure. The collection of these copies is an A-chain (more precisely, a set of occurrences, but we can put the refinement aside). But more is needed. We have assumed that CI permits interpretation of quantification, so that language must generate expressions that yield such interpretation. As discussed earlier, one way to yield such interpretations “comes free,” without stipulation: an operator in SPEC-C taking scope

³⁵For a suggestive approach to partial reduction of EPP to general cognitive principles and “canonical surface forms,” see Bever (to appear). His proposal bears on expletive-insertion, but not on the more general question of raising to SPEC-T.

³⁶For inconclusive discussion of some of these, see “On Phases.” (Chomsky to appear)

over an A-chain regarded as a variable, with restrictions given by the content of the copies. It must be, then, that the edge feature EF of a phase head P can seek a goal in the complement of P, which it can raise to SPEC-P (perhaps covertly). A-movement is IM contingent on probe by uninterpretable inflectional features, while A'-movement is IM driven by EF of P. Like A-movement, A'-movement proceeds step by step, leaving reconstruction sites. Unlike A-movement, it proceeds successive-cyclically phase by phase, an option because SPEC-P is not transferred at the P-level. That makes broad scope interpretations possible, and opens many questions that are discussed elsewhere.³⁷

Consider the interaction of A and A'-movement with regard to improper movement:³⁸

(IM) * who [_{t₁} seems [_{t₂} C [_{t₃} T-is _{t₄} smart]]] (t a copy of *who*).

At the lowest CP, _{t₄} is Case-marked by C-T and raises by A-movement to _{t₃}. It is also the goal of EF(C), hence raises directly from _{t₄} to SPEC-C (that is, _{t₂}). There is no defined relation between _{t₂} and _{t₃}. But _{t₂} is invisible to C-T of the next higher phase, because it has been inactivated within the lower CP. Therefore A-movement to _{t₁} is impossible.

While much remains open, at least the general properties of A and A'-movement appear to be within the range of principled explanation.

One might speculate that nominal phrases have structures similar to verbal phrases, and might sometimes also constitute phases. What are intuitively nominal phrases come in two basic varieties, ±definite (maybe specific – put that aside), differentiated by presence or absence of an element with some such property as “referentiality” (meaning “used to refer,” not “referential expression,” a crucial difference). We may take this element to be D – assuming that D has some real meaning.

Consider first indefinite nominals, lacking D, like “author” or “many authors.” The label of the latter cannot be “many,” which is not an LI but an XP, so in both cases the label of the phrase must be the label of “author” (which could have a complement, as in “author of the book”; note that its structure differs from one of the options for “picture of the book,” with the counterpart “the/a picture, which is of the book”). The best theory, if feasible, would not add any additional distinguishing elements. Assuming that the basic structure corresponds to verbal phrases, the head will be n with the complement [X (YP)] (X perhaps an undifferentiated root, gaining its nominal character

³⁷“On Phases”, and sources cited.

³⁸I adopt here observations of Samuel Epstein (personal communication), adapted to the version of phase theory here.

from *n*). *X* raises to *n*, just as its counterpart raises to *v* in the verbal phrase, and the result is a nominal phrase.

Assuming the same correspondence for definite nominal phrases, the head is now *n** (analogous to *v**) with the complement [*X* (*YP*)]. In this case *X* = *D*. *D* inherits the features of *n**, so *YP* raises to its SPEC, and *D* raises to *n**, exactly parallel to *v**P. Therefore, the structure is a nominal phrase headed by *n**, not a determiner phrase headed by *D*, which is what we intuitively always wanted to say; and *D* is the “visible” head, just as *V* is the “visible” head of verbal phrases. The structure is similar to a causative verb phrase analyzed with head *v** and complement {*cause*, {*V*, complement}}. The complement of *cause* raises to specifier of the element *cause*, which inherits the features of *v** and then raises to *v**. Further outcomes depend on morphology.

That looks as though it might be roughly on the right track. Both DP and NP are nominal phrases, the natural result. It could be that only the constructions with *D* are phases, perhaps the source of extraction differences of definite vs indefinite NPs and other properties.³⁹

So far, I have kept fairly close to what seems to be a reasonable interpretation of SMT, with assumptions about third factor properties of efficient computation and CI conditions that seem plausible, and can be investigated in independent ways. Just how far this line of inquiry can reach, of course one cannot know. As it proceeds, it approaches more closely the goal of identifying the principles of UG, the residue that constitutes FL once third factor properties of growth and development are extricated, along with others not considered here. The approach proceeds in parallel with a different line of inquiry into UG, the standard one for the past half-century, based on search for explanatory adequacy in terms of conditions for language acquisition: the “bottom-up” and “top-down” approaches to UG discussed earlier. Insofar as the two lines of inquiry converge, they clarify the questions that have been at the heart of theoretical study of language since its origins: to determine the basic properties of FL – a certain biological system, adopting the biolinguistic perspective, apparently a distinctive and crucial component of human nature.

³⁹It would follow that structural Case is on *n**, not *D* or *N* (hence presumably also on *n*), or the derivation will crash at the phase level.

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