Derivation and Explanation in the Minimalist Program

Edited by
Samuel David Epstein and
T. Daniel Seely

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This book is dedicated, in reverse chronological order to: Charlie, Piper, Sylvie, Molly, Hannah and Elaine. Also to Josh. And to our parents Peggy and Thomas Seely; and Lucy Epstein (1920–99) and Joseph Epstein (1917–93).

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Introduction: On the Quest for Explanation

Samuel David Epstein and
T. Daniel Seely

1 Explanation through minimization

Anyone seeking to understand how humans acquire the knowledge they have, and interested in explicitly characterizing what the knowledge is, must engage in the development of a theory. Whenever asking "What exactly is X?" and "How does it develop?" and seeking an explanatory answer, the only way to proceed is to construct a theory, however preliminary or undetailed. In linguistics, once one postulates, e.g., "noun," one has engaged in theory construction. Since the goal of any theory is to explain things, the further question we all must address is "To what extent is this goal achieved?" If engaged in serious rational inquiry, the question can't be avoided. Addressing it requires that we be analytical and reflective about our proposals. In this regard, Chomsky, acknowledging the possibility that the Minimalist Program might well be "wrong," writes of its merits as follows:

> the Minimalist Program, right or wrong, has a certain therapeutic value. It is all too easy to succumb to the temptation to offer a purported explanation for some phenomenon on the basis of assumptions that are of roughly the order of complexity of what is to be explained. . . . Minimalist demands at least have the merit of . . . sharpening the question of whether we have a genuine explanation or a restatement of a problem in other terms. (Chomsky 1995: 233–4)

Expressing this same commitment to critical reflection, Whitehead wrote:

> the progress of biology and psychology has probably been checked by the uncritical assumption of half truths. If science is not to degenerate into a medley of *ad hoc* hypotheses, it must become philosophical and must enter upon a thorough criticism of its own foundations.[1]

Unfortunately, it seems to us that these so-called "philosophical" sentiments are foreign or uninteresting to some researchers. Such questions are sometimes regarded, we fear, as "not real linguistics" or "too conceptual." If this is

true, the situation is not new. C. S. Pierce, calling for reflective evaluation of the explanatory adequacy of our theories, writes:

> There remains still another kind of power of observation which *ought* to be trained; and that is the power of observing the objects of our own creative fancy. . . . The highest kind of observation is the observation of systems, forms, and ideas.[2] (editors' emphasis)

We believe that the desire to determine the properties of our theories, and the explanatory depth achieved by them, i.e. the extent to which all the relevant phenomena have been explained, should be regarded not as an issue that is "just conceptual," but instead as *the central issue confronting theory construction*.

Of course, with Newton (1690),

> We are certainly not to relinquish evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy of Nature, which is wont to be simple and always consonant itself. . . .[3]

The question that then arises is: How do we proceed? More specifically, what shall we regard as "a genuine explanation" as opposed to "a restatement of a problem in other terms"? That is not an easy question to answer; there are no hard and fast criteria; there is no explanatory gauge that can measure the "degree" to which genuine explanation has been attained. But, the question we should ask is: To what extent have we explained anything? How do we ever know if we are on the right track?

In grappling with this very question, Einstein writes:

> Can we ever hope to find the right way? Nay, more, has this right way any existence outside our illusions? Can we hope to be guided safely by experience at all when *there exist theories (such as classical mechanics) which to a large extent do justice to experience, without getting to the root of the matter*? I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it. Our experience hitherto justifies us in believing *that nature is the realization of the simplest conceivable mathematical ideas*.[4] (editors' emphasis)

Not only should each idea be "mathematical" (we assume meaning formally explicit) and the simplest conceivable, but in addition, understanding is maximized through the minimization of the number of "simplest conceivable mathematical ideas" postulated:

> Resolved to maximize our understanding, we find ourselves committed to a highly characteristic effort to minimize the *number* of theoretical premises required for explanation.[5] (editors' emphasis)

Thus, we seek to minimize each premise, and the number of them, thereby seeking to maximize explanation through deduction (not empirical "coverage" through stipulation). Importantly then, the data is not satisfactorily "covered" if it is covered by stipulation. Einstein thus speaks of:

> the grand aim of all science, which is to cover the greatest possible number of empirical facts by logical deduction from the smallest possible number of hypotheses or axioms.[6]

And Feynman writes,

> in the further development of science, we want more than just a formula. First we have an observation, then we have numbers, . . . then we have a law which summarizes all the numbers. But the real glory of science is that we can find a way of thinking such that the law is evident.[7]

We concur with these perspectives regarding the vital importance of being reflective about the theories we have; of being concerned with explanation in general and the explanatory depth of our theories in particular. Crucial is a willingness to at least inquire into ways in which deeper explanation through minimization of posited axioms and deduction from them might be attained. Again, "We are certainly not to relinquish evidence of experiments for the sake of dreams and vain fictions of our own devising," yet concern with the explanatory depth of our theories must remain a central goal in linguistics no less than it is in the other sciences.

In this regard, it seems that Lappin et al. (2000: 668) misunderstand the motivation for the shift from the GB theory to the Minimalist Program. They write:

> If linguists wish to use the practices followed in the natural sciences as a guide, then it would be reasonable to expect the catalyst for the transition from GB to the MP to be a significant body of results that follow directly from Minimalist principles, but are unavailable on any plausible version of GB theory.

One pervasive catalyst, in any field, for exploring minimization is the ongoing (we would hope) unwavering quest for deeper explanation through self-criticism of articulated theories, even holding the data constant. This quest for simplification and deduction is more than just consonant with "the practices followed in the natural sciences;" it constitutes their "grand aim" (Einstein) and "real glory" (Feynman). Our hunch, following Chomsky, is that so-called GB theory (using Einstein's terms) "to a large extent do[es] justice to experience, without getting to the root of the matter . . ."

Again, empirical "coverage" (though obviously of great importance) is not the sole issue, rather how the theory "covers" the data, always a very difficult question to answer, must be posed, discussed, and addressed. Contra this perspective, Lappin et al. (2000: 667) write of the shift in interest from GB to Minimalism:

> Still more remarkable is the widespread perception that a novel approach with no more "battle-tested results" to its credit (and far narrower cross-linguistic coverage) than its predecessor represents a conceptual breakthrough for generative grammar. . . .

We believe that asking certain questions, in particular "Why?," and trying to provide an answer, can indeed represent a "conceptual" breakthrough in any field.

The Minimalist conjecture is that we can get at, or closer to, the root of the matter by first asking; "Why do these GB principles, definitions, filters, and postulates, and not others, hold – if they in fact do hold?" The derivational approach, aspects of which we outline below, seeks to answer by "finding a way of thinking" (through appeal to an independently motivated, local generative procedure; i.e. the derivation) such that the laws of GB theory (to the extent that they are empirically correct) are evident.

2 Derivation and explanation in the Minimalist Program

As concerns linguistic explanation in particular, Reuland eloquently summarizes the shift from GB to the Minimalist Program as follows:

> Contentment with higher level stipulations has been replaced by pursuing the question of why they would hold. (Reuland 2000: 848)

In our view, the question of why they would hold is tantamount to the question of whether, in Einstein's terms, they can be logically deduced from, "the smallest number of simplest conceivable mathematical ideas."

Chomsky's Minimalist Program is committed to this same "highly characteristic effort to minimize . . ." conjecturing that the language faculty

> provides no machinery beyond what is needed to satisfy minimal requirements of legibility and that it functions in as simple a way as possible. (Chomsky 2000: 112–13)

Although there is "no machinery beyond what is needed to satisfy minimal requirements of legibility . . .", it is important to note that there is machinery, i.e. mechanisms that generate objects, not just laws that the objects obey. The idea that the generative mechanisms play a crucial role in explanation is not limited to linguistics. This mode of generative explanation is pursued in, for example, J. Epstein's conception of the explanatory power of Agent-based Computational Modeling in what he calls "Generative Social Science" (J. Epstein 1999; see also J. Epstein and Axtell 1996). As Epstein notes:

> the central idea is this: To the generativist, explaining the emergence [footnote deleted] of macroscopic societal regularities, such as norms or price equilibria, requires that one answer the following question: "How could the decentralized local interactions of heterogeneous autonomous agents [i.e. individuals; SDE/TDS] generate the given regularity?" (J. Epstein 1999: 41)

J. Epstein assumes that one has explained, in at least *one* sense of the notion "explanation," the macroscopic societal regularity, to the extent that one can

> Situate an initial population of autonomous heterogeneous agents in a relevant spatial environment; allow them to *interact according to simple local rules*, and thereby generate – or "grow" – the macroscopic regularity *from the bottom up* [footnote deleted]. (J. Epstein 1999: 42; editors' emphasis)

In short, J. Epstein asserts that "If you haven't grown it, you haven't explained it."

With the above in mind, let us return to some of the specific proposals that Chomsky makes regarding the Minimalist Program. At the heart of these are, among other things, the following minimizing, simplifying, and eliminative conditions (from Chomsky 2000: 113):

(A) The only linguistically significant levels are the interface levels.

(B) The *interpretability* condition: LIs [i.e. lexical items] have no features other than those interpreted at the interface, properties of sound and meaning.

(C) The inclusiveness condition: No new features are introduced by Chl [Chl refers to "the computational system of human language"].

(D) Relations that enter into Chl either (i) are imposed by legibility conditions, or (ii) fall out in some natural way from the computational process [footnote deleted].

There are a number of points to make regarding (A)–(D); we start with (D)ii.

2.1 *The computational process*

First, and most important for present purposes, there is a computational *process* (see (D)ii),[8] specifically, there is a mechanism (i.e. rules) for generating syntactic objects. Like Standard Theory, the MP includes locally constrained transformational rules. These operations are, however, preferable to the Standard Theory transformations, since in the Minimalist Program, there are only two transformational rules: singulary (Move) and generalized (Merge); and neither is construction specific, nor language specific, but each is instead, by hypothesis, universal. Moreover, we assume that these rules are "minimal"[9] (as proposed in Collins (this volume); see also Seely 2000) and similar (as proposed in Kitahara 1997), each of the form: "A concatenated with B yields {A, B}." This is an attractive, arguably irreducible axiom of a generative theory of syntactic knowledge, seeking to explain the central object of inquiry – the creative aspect of language – by appeal to a finite, minimal, universal (recursive) system characterizing knowledge over an infinite domain. Notice that the "construction-specific" transformational rules of the Standard Theory might have done justice to the phenomena, but, if explanation is the concern, they indeed seemed not to get to the root of the matter. The same is true of, for example, construction-specific phrase structure rules, which to a large extent

yielded empirical "coverage." Asking (and addressing) the question of *how* the facts were covered, led to the abandonment of such construction-specific mechanisms, a highly significant shift in our understanding of the nature of the syntactic component of the human language faculty.

2.2 Was GB nonderivational?

The Minimalist Program's appeal to rules and the proposed constraints on their iterative recursive application (e.g., the cycle) purportedly constitutes a radical departure from the Government and Binding (GB) "rule-free" approach. GB, as contrasted with the Standard Theory, is traditionally assumed to be representational, characterized as a "virtually rule-free system" (Chomsky 1986: 93). But "virtually rule free" isn't "rule free." Indeed, GB theory did have rules, including, for example, Move-alpha and whatever generated structures that could comply with (or violate) the X-bar schema. In addition, there were Case assignment rules, and other feature assignment mechanisms, which had the property of applying only in government configurations. Government was an elegant intermodular unifying relation, but it was nonetheless unexplained: government was defined on already-built-up trees, with no answer to the question "Why does government, and not some other definable relation, hold?" Here we see two unattractive, related aspects of the theory. First, by appeal to D-structure (DS), a *level* of representation, complex "sentential" structures are first completely assembled, all at once. Then, and only then, are relations expressible, and since the structure has already been entirely built (by virtue of the role and function of DS) the only way to express relations is to nonexplanatorily define them on the already-built representation. In fact, all relations were defined on already-built (macroscopic) trees. There was no apparent alternative in a virtually rule-free system. This left no way of explaining relations.

In addition, representational concepts like chains, and traces, seem to us to be disguised derivational constructs, to the extent that they in fact encode into output representations the history of the derivation (for related discussion see Epstein and Seely (this volume), and Hornstein and Uriagereka (this volume)). If so, even leaving empirical matters aside,[10] our hunch is that if such constructs do justice to experience (and we don't think they do), they don't get to the root of the matter. Thus, for example, Chomsky (1981) states that a typical A-chain "in an obvious sense, represents the derivational history of this [i.e. the moved] NP by successive applications of move-alpha . . ." And similarly, Chomsky (1986: 95) regards "a chain [as] the S-structure reflection of a 'history of movement'." Within the GB framework, nothing "should", nor could, be deduced directly from (the accidental) properties of the amorphous rule "Move anything anywhere optionally" as it blindly applies in any one of an infinite number of ways that it logically could. As a result of this approach, the idiosyncratic properties of successive rule application must be *encoded* so that the output representation (to which necessarily descriptive, representational constructs apply) "in an obvious sense represents the derivational history." Herein lies

the motivation for trace theory and for representational chains. The derivation (successive rule application) is in fact recognized as vitally important, but direct appeal to the derivation is "forbidden" in a "rule-free" system so that the output representations must include constructs needed to encode derivational history – since it is clearly relevant.

At the same time, this (encoding) postulate "chain" increases the types of "syntactic objects" postulated so as to include not only syntactic categories/constituents, but also more complex objects, namely chains. (And, in fact, if we adopt a simplified, minimized, arguably ineliminable, definition of syntactic object, chains are prohibited; a chain is, unlike e.g., a DP, not a syntactic constituent.[11]) If then they do indeed encode properties of the derivation, and their definition (thus needlessly) extends the class of syntactic objects, the question of their eliminability should, at least, arise.

As concerns one kind of chain, in this case a type of head chain, an arguably striking illustration of Einstein's idea of being able to do justice to experience without getting to the root of the matter is reflected in the following:

> It is generally possible to formulate the desired result in terms of outputs. In the head movement case, for example [a case of raising from N-toV followed by [$_V$ N+V], raising to Infl]) one can appeal to the plausible assumption that the trace is a copy, so the intermediate V-trace includes within it a record of the local N→V raising. But surely this is the wrong move. The relevant chains at LF are (N, t_N) and (V, t_V), and in these the locality relation satisfied by successive raising has been lost. . . . These seem to be fundamental properties of language, which should be captured, not obscured by coding tricks, which are always available. A fully derivational approach both captures them straightforwardly and suggests that they should be pervasive, as seems to be the case. (Chomsky 1995: 224)

Within GB theory, it seems that rules remain ineliminable, although they can be largely, but perhaps not entirely, hidden through representational encoding devices, such as chains, which needlessly enrich the class of syntactic objects, without getting to the root of the matter.[12]

Importantly, then, GB, a so-called "representational" theory, is, in our view, a kind of derivational theory since there are transformational rules (virtually rule free is not rule free). Moreover, there are traces and chains, which are "lookback" notations whereby the empirically crucial derivation that produced the representation is encoded (see Epstein and Seely (1999), (forthcoming) for detailed discussion).

2.3 *Which type of derivational theory is preferable?*

If we are on the right track, then such so-called "representational" theories are, in fact, just one kind of derivational theory. The question then is not: "Which is preferable, derivational or representational theory?" but rather "Which type of derivational theory is preferable?" The options would seem to be these: (i) a derivational theory that seeks to empirically and explanatorily exploit the arguably ineliminable rules and derivation itself. Or, (ii) a derivational theory

that instead implicitly incorporates rules and derivations, but does not (or cannot) appeal to them and instead encodes derivational history in macroscopic, molecular, representations, by (for example) defining chains on them.

Our hunch is that the latter approach underlies the postulation of complex derivation-encoding objects such as chains, while forcing us to constrain output representations with unexplained laws (filters and principles), and definitions (e.g., Government, Minimal Domain, etc.) as defined upon macrostructure levels of representation, assembled instantaneously.

We do not think that the answer to the question of which derivational theory is preferable is obvious or clear, but we do think that the question is of fundamental importance, directly addressing the formal properties of human linguistic knowledge representation. If this is indeed a question, it concerns all analyses. Imagine being told by someone in another field, "Well, there remains, it would seem, always two different ways of implementing things." Obviously, the critical response should be, "That's really crucial, and you should seek to show either that they are equivalent, or that there is a difference, and what the difference is, empirically, and with respect to explanatory depth" (see Brody (this volume) for important discussion of this point; and for a rather different perspective than ours.) The usual assumption is that empirical coverage and explanatory depth are at odds. We disagree in that empirical "coverage" is illusory, to the extent that the data are "covered" by stipulation. All analysts should then be interested. Have I formulated my analysis in one way or another, and do I have evidence for either? The question seems to us unavoidable: What is the architecture of the system? How does it work? What is the evidence for doing it one way vs. another? One crucial "difference" seems to us to be at least partially related to the central concern of explanatory depth, even holding the data constant. The laws/higher level stipulations/macroscopic regularities might be deducible from the simplest conceivable mathematical mechanisms, such as recursive application of rules like A+B => {A, B}. It is certainly always worth exploring the idea that it is empirically and explanatorily preferable to cover the facts, or better, yet more of the facts, with fewer stipulations.

2.4 Explanation through representational minimization

Let us now continue our consideration of the minimizing conditions mentioned above, turning to (A)–(C), repeated here:

(A) The only linguistically significant levels are the interface levels.

(B) The interpretability condition: LIs [i.e. lexical items] have no features other than those interpreted at the interface, properties of sound and meaning.

(C) The inclusiveness condition: No new features are introduced by Chl.

As concerns (B), it imposes a severe limitation on the class of lexical features, which constitute the atomic elements upon which the operations of

Chl compute. Thus lexical representations are severely impoverished. Furthermore, under the Inclusiveness condition (C), nonexplanatory representational encodings like subscripts, superscripts, bar-levels, and star marking are no longer an option, but are instead explicitly prohibited. For example, Kitahara (1999) (see also Kitahara, this volume) represents perhaps the "classic" illustration of Inclusiveness in action, seeking to eliminate the "suspect" star-feature invoked in the analysis of Chomsky and Lasnik (1993). In a similar vein, Epstein and Seely (1999; forthcoming) argue, that given, in addition to (A)–(D), a restrictive definition of "syntactic object"/"syntactic category" (as provided in Chomsky 1995), chains too are prohibited by Inclusiveness (see also Hornstein 1998), an arguably welcome result if, as a matter of fact, "a chain . . . [is the] . . . S-structure reflection of a 'history of movement'" and is moreover a representational reflection which, we believe, engenders empirical difficulties in at least the case of A-chains. This brings us to (A), the issue of levels.

Obviously, (A) represents another form of representational impoverishment. If S-structure is eliminated, chains and (what are, in a certain sense) nonexplanatory filters (e.g., the Case Filter) on representations can no longer be defined on S-structure representations. That, too, is a welcome, if not daunting, result. Moreover, as noted, with the elimination of an initiating "all-at-once" level like DS, perhaps we will not be compelled to nonexplanatorily define relations on already-built-up trees, but might instead explain syntactic relations – a central goal of an explanatory theory of syntax (see, for example, Epstein 1999 and Epstein et al. 1998).

The elimination of levels might be further advanced by seeking to eliminate not just D- and S-structure, but also PF and LF as well (as in Epstein et al. 1998; Epstein and Seely 1999, forthcoming; and Uriagereka 1999). Chomsky, moreover, characterizes his phase-based analysis as level eliminating as well:

> In this conception there is no LF: rather, the computation maps LA to <PHON, SEM> piece-by-piece cyclically. There are, therefore, no LF properties and no interpretation of LF, strictly speaking, though sigma and phi interpret units that are part of something like LF in a non-cyclic conception. (Chomsky 2001: 4)

2.5 Summary

To sum up, we believe that we all "must become philosophical and must enter upon a thorough criticism of the theory's own foundations" (Whitehead). This willingness to first carefully examine "the objects of our own creative fancy" (Pierce) and then to criticize them, and be "discontent with higher level stipulation" (Reuland), underlies the highly characteristic effort to increase explanatory depth by minimizing theoretical apparatus (Nash, as noted above, and, of course, Chomsky's Minimalist Program). Minimization-through-derivation, as discussed here, asks "Can we explain the laws by finding 'a way of thinking such that the laws are evident'?" (Feynman). Can we grow or generate the macroscopic ("sentential/syntactic") regularities from the bottom up? (J. Epstein). These are the very kinds of questions, all regarding deduction

and explanation, which we think it is never wrong to ask (and always counterproductive to denounce), that each paper in this volume seeks to explore, in one way or another, and, in some cases, with productive disagreement regarding the role of derivation in explanation.

3 The articles

This volume contains a collection of articles by leading researchers, each of which investigates aspects of the fundamental questions raised above. All papers are concerned with explanation, consonant with Reuland's summary of the shift from GB to Minimalism within which "Contentment with higher level stipulations has been replaced by pursuing the question of why they would hold;" and all assume some form of minimization. The majority develop a derivational approach, Brody's contribution being an important exception. As for the more specific proposals raised above, a few (all too brief) comments on the papers are in order.

One central research goal, as specified in section 2 above, is to deduce as much as possible from the arguably necessary (but cf. Brody) syntactic structure-building machinery; i.e. the universal and minimal rules and their mode of recursive application. What is to be avoided, among other things, on this view is defining relations on already-built-up trees, such definitions being, in our view, nonexplanatory. Kayne and Zwart, for example, don't define binding-theoretic domains on trees but rather attempt to account for constraints on the binding of Y by X, under the assumption that X and Y are first-merged as sisters (the simplest rule-created relation) and then X undergoes independently constrained movement. Thus, Kayne, "adopting the derivational perspective of Chomsky's recent work, [explores] the idea that antecedent-pronoun relations should be rethought in movement terms, including what we think of as Condition B and Condition C effects." This represents a significant shift from defining binding-theoretic domains on trees to seeking an explanation of them in terms of independently motivated, and seemingly ineliminable, apparatus (movement theory) and a highly simplified version of certain referential relations (posited to hold under sisterhood, as created by Merge when X and Y are concatenated by a single rule application). In a related paper, Zwart seeks to account for both local and nonlocal binding relations under a similarly derivational approach.

Various forms of minimization were reviewed above, including the minimization of levels (at most the interface levels are posited), minimization of lexical items (the interpretability condition: no features other than those interpreted at the interface), and the minimization of syntactic objects (the inclusiveness condition: no new features are introduced by Chl). Collins provides a classic form of representational minimization, attempting to eliminate phrasal category labels entirely, and argues that "as representations are simplified (e.g., labels are eliminated), it becomes necessary to articulate the precise nature of syntactic derivations (e.g., minimize search, the Locus Principle, etc.)."

With respect to Inclusiveness, Hornstein and Uriagereka argue that "[The] 'loss of information' scenario [i.e. where information is available at one point in the derivation but not at another] fits poorly into models which make use of enriched representational codings (including traces, indices, and similar elements) because whatever information may have been lost in the course of the derivation can be [arguably incorrectly] reconstructed through some abstract [and sometimes Inclusiveness-violating] coding." And Zwart, as part of his development of a derivational approach to binding phenomena, argues that "with each new operation Merge, the element merging to the structure acquires additional features which are interpreted at LF, but no trace or chain is created, and no information can be gathered from these entities."

Similarly, Kitahara, "under the framework of Epstein et al. 1998, . . . articulates how . . . mechanisms of scrambling interact with the interpretive procedures applying in the course of a derivation." And McCloskey "argues that the form of the complementizer in Irish A'-binding constructions depends not on the mechanism of Specifier Head agreement (as has been widely assumed), but rather is sensitive to the mode of introduction of the specifier material (whether it is introduced by Merge or by Move)." Thus, mere inspection of the representational output is predictively insufficient.

Richards "develop[s] an approach to the derivation that generates the material at the root of the tree first, and adds new material to the bottom of the existing structure," and explores "the surprising prediction that under certain circumstances, A' movement should share with A-movement the property of being unable to skip an intervening A-position."

Torrego seeks to account "for cross-linguistic variation involving subject-to-subject raising over an Experiencer," arguing that "the analysis must be couched in a framework of derivational economy that imposes strict derivationality. . . ." Thus, the Minimal Link Condition arguably must be a movement constraint, not an S-structure filter.

Frampton and Gutmann "argue that an optimal derivational system, at least from a computational point of view, is a system that generates only objects that are well-formed and satisfy conditions imposed by the interface systems." Further, "we demand that the computational system be crash-proof. That is, that no filters are imposed on the end products of derivations. . . ."

Epstein and Seely, also pursuing minimization, argue that Spell Out applies inside each transformational rule application, and that "this conception of Spell Out allows for the elimination of: levels, special categorial phases, look-ahead, lookback, and simultaneous rule application (as well as "principles" like the EPP)."

Pursuing an important alternative view, Brody argues that "no pure derivational theory of narrow syntax exists." And further, it is argued that "the derivational explanation of the asymmetry of the notion of c-command . . . like other explanations of c-command (and also like other derivational explanations) is not successful."

To conclude this introduction, each author's abstract is presented, in alphabetical order.

***Michael Brody**: "On the Status of Representations and Derivations" (Chapter one)*

I discuss the point here that current mixed theories of syntax that involve both derivations and representations are redundant and in principle less restrictive than their pure representational or pure derivational counterparts. Next I show that no pure derivational theory of narrow syntax exists. To be minimally adequate, derivational theories must be mixed, hence the arguments against mixed theories apply to these too. In addition to this, I argue that everything else being equal, and with no additional stipulations added, current derivational theories with the rule of move are less restrictive than representational theories with the concept of chain. In the third section of the chapter I consider the derivational explanation of the asymmetry of the notion of c-command and conclude that this explanation, like other explanations of c-command (and also like other derivational explanations) is not successful. I suggest instead that we should eliminate c-command from the grammar and replace it by simpler interacting notions.

***Chris Collins**: "Eliminating Labels" (Chapter two)*

In this chapter, I argue that it is possible to eliminate the labels of phrasal categories. I consider four areas where labels have been used in syntactic theory: X'-Theory, Selection, the Minimal Link Condition, and the PF Interface. In each case, I show that it is possible to capture the relevant generalizations without the use of phrasal category labels. This chapter illustrates the relationship between derivational and representational approaches to syntax. As representations are simplified (e.g., labels are eliminated), it becomes necessary to articulate the precise nature of syntactic derivations (e.g., minimize search, the Locus Principle, etc.).

***Samuel David Epstein and T. Daniel Seely**: "Rule Applications as Cycles in a Level-free Syntax" (Chapter three)*

In a number of important recent papers, Chomsky postulates multiple, cyclic Spell Out, and seeks to explain (not stipulate) this central, derivational departure from the long-dominant Y-model. Since features are valued iteratively, i.e. derivationally, and since once valued, Spell Out cannot properly operate, Chomsky suggests that it follows that Spell Out must itself also apply iteratively. We reveal potential problems confronting this important explanation of cyclic Spell Out. We argue that the explanation can be maintained, while overcoming the problems noted, by "driving Spell Out inside each

transformational rule application." We argue further that this conception of Spell Out allows for the elimination of: levels, special categorial phases, lookahead, lookback, and simultaneous rule application (as well as "principles" like the EPP). Although far from conclusive, this eliminative approach identifies (and pursues to some degree) what we believe to be the null hypothesis regarding the architecture of UG.

John Frampton and Sam Gutmann: *"Crash-proof Syntax" (Chapter four)*

The Minimalist Program is guided by the idea that the syntactic system of the language faculty is designed to optimally meet design specifications imposed by the interface systems. In this chapter we argue that an optimal derivational system, at least from a computational point of view, is a system that generates only objects that are well formed and satisfy conditions imposed by the interface systems. We explore the feasibility of constructing such a system, one in which there is no notion of "crashing derivations." We also argue that both the interface conditions and the derivational system which is designed to satisfy them are real, and in this sense we expect to find a pervasive redundancy in linguistic explanation. We demand that the computational system be crash-proof, that is, that no filters are imposed on the end products of derivations, and that no global filters (e.g. "comparison of derivations") assign status to derivations as a whole. By exploring the properties of a crash-proof system, we hope to clarify the boundary between the derivational system and the interface conditions, and to make progress in understanding both.

Norbert Hornstein and Juan Uriagereka: *"Reprojections" (Chapter five)*

This chapter explores the possibility that projected labels may change in the course of the derivation (hence the name *reprojection*). Label changes do not alter phrasal geometry, and in that sense are not transformational processes; however, representational dependencies are altered via reprojection in such a way that, for instance, head–specifier relations are redefined. The main consequence of this is that it provides a natural syntax for binary quantifiers, in much the same way that transitive verbs take their arguments within their (extended) projection. Interestingly, it is these quantifiers which induce islands for various LF dependencies across them. In our terms this follows from the fact that, after reprojection, a putative LF dependency would have to be across a non-complement branch. Unary quantifiers need not reproject in order to meet their syntactic/semantic demands, and hence do not induce LF islands of this sort. The chapter extends these results to other, less obvious domains.

Richard S. Kayne: *"Pronouns and their Antecedents" (Chapter six)*

Adopting the derivational perspective of Chomsky's recent work, I explore the idea that antecedent–pronoun relations should be rethought in movement terms, including what we think of as Condition B and Condition C effects. This allows me to capture the compelling similarity between Condition C and the prohibition against downward movement, and to dispense with the notion of "accidental coreference" in Lasnik's sense. All instances of "intended coreference/binding" involve a movement relation. In the spirit of the Lebeaux/Chomsky proposals concerning Condition A, I argue that neither Condition B nor Condition C is a primitive of UG. The effects of both follow, in a derivational perspective, from basic properties of pronouns and basic properties of movement. What underlies Condition B effects underlies the existence of reflexives. The movement approach explored, which leads to some use of sideward movement, has consequences for circularity effects, for reconstruction effects, and for backwards pronominalization.

Hisatsugu Kitahara: *"Scrambling, Case, and Interpretability" (Chapter seven)*

Saito (1992) examines a number of binding phenomena involving scrambling and proposes that the binding theory applies not solely at LF, but at some pre-LF-level as well. In this paper, I elaborate Saito's proposal under the framework of Epstein et al. (1998). I first specify the mechanisms of scrambling in Japanese, adopting certain aspects of the probe-goal system (Chomsky 2000, 2001). I then articulate how these mechanisms of scrambling interact with the interpretive procedures applying in the course of a derivation.

James McCloskey: *"Resumption, Successive Cyclicity, and the Locality of Operations" (Chapter eight)*

This chapter is concerned with the mechanisms by which "long" A'-binding relations are constructed, and in particular with the role of intermediate positions in constructing those relations. The main empirical issue it grapples with is the way in which in Irish the form of C varies according to:

(i) whether or not a clause hosts an instance of A'-binding;
(ii) what kind of A'-binding (binding of a trace or binding of a resumptive pronoun) it hosts.

The paper shows that long A'-binding can involve "mixed chains," some of whose links are created by movement, and some of which are created by merge of a binding operator in the specifier of CP.

It also argues that the form of the complementizer in Irish A′-binding constructions depends not on the mechanism of specifier–head agreement (as has been widely assumed), but rather is sensitive to the mode of introduction of the specifier material (whether it is introduced by Merge or by Move). The core characteristic of the system that emerges is its locally blind character. Choices are made at intermediate positions with no "foresight" about what the consequences for semantic interpretation will be. Locality is more highly valued than ease of interpretation.

Norvin Richards: *"Very Local A′ Movement in a Root-first Derivation (Chapter nine)*

Richards (1999), following Phillips (1996, to appear), developed an approach to the derivation that generates the material at the root of the tree first, and adds new material to the bottom of the existing structure. The approach made the surprising prediction that under certain circumstances, A′ movement should share with A-movement the property of being unable to skip an intervening A-position. In this paper I try to show that the prediction is true in a range of cases, under circumstances largely predicted by the theory.

Esther Torrego: *"Arguments for a Derivational Approach to Syntactic Relations Based on Clitics" (Chapter ten)*

A well-known complication concerning the data studied for subject-to-subject raising over an Experiencer is the grammatical source of cross-linguistic variation. Concentrating on Romance in the area of clitics, I show how certain aspects of the particulars of dative Case influence this variation for the languages considered. The analysis must be couched in a framework of derivational economy that imposes strict derivationality of the sort argued by Epstein and Seely (1999). Relevant comparison between languages of the French type and of the Spanish type attributes a range of grammatical differences between the two groups to a general property of "doubling" clitics, and the range of clitics that license *pro*.

Jan-Wouter Zwart: *"Issues Relating to a Derivational Theory of Binding" (Chapter eleven)*

The derivational approach to syntactic relations (DASR, Epstein et al. 1998), adopted here, implies (in its strongest form) that syntactic relations between α and β exist iff α and β are merged as sisters at some point in the derivation. In the spirit of DASR, Kayne (this volume) proposes that coreference results from the circumstance that the antecedent and the pronoun are merged as sisters, after which the antecedent moves up to the appropriate Case and

theta-positions. This paper adopts the general outlook, but argues that Kayne's proposal applies to local anaphor binding only, not to nonlocal coreference/bound variable anaphora. The approach assumes that a reflexive pronoun is the marked Spell Out of an abstract category (bundle of features) PRONOUN, which acquires the feature [+coreferential (with α)] iff it is merged with α in a sisterhood configuration. Since the antecedent moves away from the PRONOUN via A-movement, the fact that the locality conditions on binding and A-movement coincide is accounted for.

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Notes

1 Whitehead (1925) as cited in Carl Caither and A. Gavazos-Gaither, eds., (2000) *Scientifically Speaking*, Bristol: Institute of Physics, p. 305.
2 From C. S. Pierce, Chapter 5, "Training in Reasoning," in *Reasoning and the Logic of Things*, ed. K. L. Ketner, Cambridge, MA: Harvard University Press 1992, pp. 186–7.
3 As cited by E. Sober, *Core Questions in Philosophy*, New York: Macmillan 1991, p. 279.
4 Albert Einstein (1934) "On the method of theoretical physics." Delivered at Oxford, June 10, 1933. In *Ideas and Opinions*, New York: Bonanza Books 1954, p. 274.
5 From L. K. Nash (1963) *The Nature of the Natural Sciences*, Boston, MA: Little, Brown, p. 173.
6 Albert Einstein, *Ideas and Opinions*, New York: Bonanza Books 1954, p. 282.
7 See R. P. Feynman, R. B. Leighton, and M. Sands, *The Feynman Lectures on Physics*, vol. 1, chapter 26, p. 3, Reading, MA: Addison-Wesley 1963 (as cited in Freidin and Vergnaud's very insightful "Exquisite connections: some remarks on the evolution of linguistic theory." MS, Princeton, NJ, 2001, p. 22).
8 Notice that (D) constitutes a disjunctive, hence suspect, definition of the absolutely central notion "syntactic relation." See Epstein (2000) for discussion. Importantly, we believe that contra D(i) the bare Output conditions cannot "impose" relations, and (thus) the relations are, by hypothesis, only those created by the generative procedure (Epstein et al. (1998), S. D. Epstein (1999), Epstein and Seely (1999, forthcoming), a procedure which precisely relates categories by combining them (recursively). Chomsky's "optimal solution" question, we believe, is then whether the relations created by the generative procedure, are "used for" interface interpretation, as appears to be true of, e.g., c-command, hypothesized to be used both for the "purpose" of phonetic linearization, under Kayne's (1994) LCA, and for semantic/"LF" interpretation as well. See John Frampton and Sam Gutmann (this volume) who "argue that both the interface conditions and the derivational system which is designed to satisfy them are real, and in this sense we expect to find a pervasive redundancy in linguistic explanation."
9 See Chomsky (1994). As for labels, in the sense of Chomsky (1994), see the label-free program of Collins (this volume); and see also Seely (2000).

10 For an interesting recent debate see Hornstein (1998), Brody (1999), Hornstein (2000), Brody (2001).
11 See Hornstein (1998) and Epstein and Seely (1999; forthcoming).
12 See Hornstein (1998), Epstein and Seely (1999; forthcoming), and Lasnik (1999) for arguments that justice to experience is in fact not achieved through certain chain analyses, which these authors argue are empirically inadequate.

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