1 *Orientation and goals*

1.1 Some methodological preliminaries

1.1.1 A note on empirical coverage

Before outlining the book, we want to make aspects of our overall orientation clear, and we begin with two preliminaries.

Throughout, our primary goal (whether we attain it or not!) is explanation via deduction, not empirical coverage by (re-)description or stipulation (see e.g., Epstein and Seely 2002: Introduction, for further discussion). Of course, it is undeniable that empirical coverage is vitally important, but to adapt a point we believe was made by either Dirac or Thom,[1] consider the following scenario. First suppose we have some finite set of empirical findings, such as

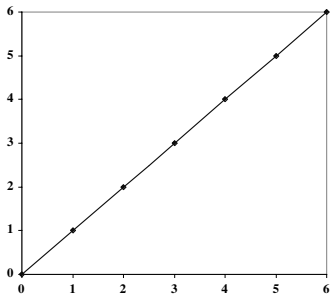

Figure 1.1 The data

From such findings we advance the non-finite hypothesis that x=y. Now consider

1. We are indebted to Josh Epstein for pointing this argument out to us. Despite his and our concerted efforts and numerous consultations, we have thus far been unable to determine the exact attribution, in particular, whether the argument is due to Dirac and/or to Thom, or someone else. We also thank Pam Beddor for very helpful discussion of and comments regarding the argument.

the following two competing theories. Theory 1 correctly predicts three of the data, namely (1,1), (3,3), and (5,5), e.g., as follows:

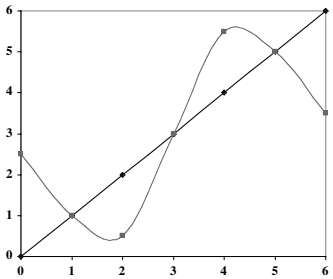

Figure 1.2 Theory 1

Now consider a different theory, Theory 2, which correctly predicts *none* of the data.

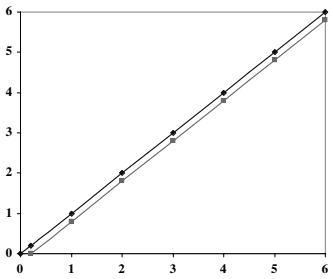

Figure 1.3 Theory 2

Clearly Theory 1 is 'empirically preferable' by a 'winning score' of 3–0. The point, as we understand it, is that Theory 2, despite getting none of the data correct, captures the data's overall (linear) pattern, and is 'closer to the truth' or 'more illuminating' than the empirically preferable Theory 1. Hence, we believe, Theory 2 is a better working or guiding hypothesis upon which to base future research. The 'balance' between coverage and insight is surely a delicate and vitally important matter, but our point here is only to re-emphasize the following: the empirical coverage of a theory (to the extent that it is ever precisely determined) is not the only issue at hand, if indeed our goal is explanation. (Needless to say, this study includes wide-

ranging and detailed empirical analyses, and we do not explicitly seek a theory with zero empirical coverage as our goal!)

There are of course other scenarios, too, in which 'better empirical coverage' doesn't necessarily weigh in favor of one theory over another. Human scientific inquiry necessarily proceeds by decomposing the world into parts; for example, human knowledge of language is hypothesized to be an investigable aspect of the world, a part which has parts within it: syntax, phonology, semantics, morphology, pragmatics, etc., each with its own subparts. Thus, we *hypothesize* that a given fact is syntactic, and we try to cover the fact empirically. But it may well be that it is a mistake for a syntactic theory to cover a particular fact, since the fact may well be non-syntactic. Thus, covering more facts doesn't necessarily make a theory preferable. It can make it a worse theory than an empirically narrower competitor, if the extra 'winning facts', covered only by the 'winning syntactic theory', turn out to be, in fact, non-syntactic phenomena.

We include this discussion simply to identify our (undoubtedly unachieved but ultimate) goal, what Einstein (1954:282) called 'the grand aim of all science':[2]

> . . . which is to cover the greatest possible number of empirical facts by logical deduction from the smallest possible number of hypotheses or axioms.

The centrality of 'minimalism' – operating in concert with the goal of empirical coverage – in all scientific explanation is evident from this perspective. (For an important, detailed discussion of the pervasive role of Minimalist method in scientific inquiry, see e.g., Freidin and Vergnaud 2001.)

The analyses that follow, we suspect, fall somewhere between Theories of Type 1 and 2. Of course, we certainly hope we got all of the data right, but needless to say, in any serious empirical inquiry, one never knows with certainty if they've truly discovered something or not. What is virtually certain is that we got at least some things wrong. Our hope is that where we are wrong, we are nonetheless 'close' like Theory 2, and thereby do not lead ourselves into a wildly wrong (Theory 1 type) hypothesis, that despite covering data is in fact 'way off track'.

1.1.2 Unclarity as a 'merely conceptual', hence non-empirical, issue

Another related issue concerning empirical inquiry is worth noting here. Certain

2. Einstein (1954:282) *Ideas and Opinions*, Bonanza Books, New York.

issues, e.g., examining the unclarity of a principle, it seems to us are sometimes viewed as a merely 'conceptual or philosophical issue, not really empirical, not real linguistics'. In many cases this seems to us an *empirically problematic* perspective to adopt. To the extent that a principle is unclear, its predictive content is unspecified, hence indeterminate. Thus unclarity is an empirical issue, and empirical issues are extremely (but not uniquely) important. The same holds true for contradictions within a theory or analysis. These are not conceptual, 'merely theoretical' non-empirical issues; rather, all the data is at risk of being unpredicted. (For an elegant unveiling of a contradiction embedded within Epstein *et al*. 1998, see Gajewski 2000.)

1.2 Outline and rationale

Our analyses are couched within and critically examine certain aspects of Chomsky's pioneering Minimalist framework (Chomsky 1991, 1995, 2000, 2001a). We develop and explore further a level-free architecture for UG, a so-called 'derivational approach' to syntactic relations, as initiated in Epstein (1994, 1999) and Epstein *et al*. (1998), and developed in Epstein and Seely (1999, 2002: Chapter 3). This eliminative derivational Minimalism seeks generative explanation (see below) through minimization, the latter, as noted, a highly characteristic, if not defining, goal of all scientific inquiry. (See Epstein and Seely 2002 for discussion, and the references cited there.)

1.2.1 Eliminating A-chains

One central 'observation' made here is that, under an independently motivated, contemporary and highly restrictive Minimalist hypothesis about what a syntactic object is (and is not), the postulate chain is in fact excluded by the theory. Thus, we seek to overcome the potential contradiction faced by adopting a chain-excluding restrictive definition of syntactic object, and concomitantly postulating chains, which fail to satisfy the restrictive conditions. We retain the restrictive definition, and seek to abandon the notion A-chain (see also Hornstein 1998). Chapter 2 explores only the elimination of A-chains, and focuses on the motivation for their postulation as a concept of UG, exploring some of the empirical motivation for A-chains in English, and the architectural aspects of UG that seem to have moti-

vated this postulate. (We leave the role of head chains and A-bar chains within a derivational framework for future research.) We argue that the empirical support for A-chains in English raising constructions is negligible, and argue in addition that postulating A-chains engenders certain thus far unnoted empirical problems as well as fundamental (predictive) unclarities which are avoided by their elimination. We propose that there is no successive cyclic A-raising in such English constructions, but rather, by hypothesis, one fell swoop movement from theta to Case position.

We do not predict that successive cyclic A-movement is universally precluded, but rather, that English raising *to* checks no features whatsoever (although it may have semantic features, perhaps those of a modal operator). Consequently, under Chomsky's Minimalist explanation-seeking theory of transformational rule application, whereby all rule application is purposeful, there is no movement to or through Spec of raising *to*.[3] In other languages or English constructions there may well be abundant evidence for purposeful successive cyclic A-movement and intermediate feature checking. We thus do not propose that 'successive cyclic A-movement is universally excluded', or even that it is invariably excluded in English. 'Successive cyclic raising' has no real theoretical status; rather only local morpholexical feature checking does, and its overall distribution can be determined only by correctly characterizing the morphological features of all relevant lexical heads and the conditions regulating individual transformational rule application and derivations.

1.2.2 Derivations

If A-chains are eliminated and if certain information that A-chains encode is indeed important, namely the set of positions occupied by a mover and the order in which they were occupied over the course of a derivation, then such information must be expressed by other means. This, we suggest, can be achieved by adopting the derivational approach to syntactic relations. Under this level-free architecture, syntactic relations are, by hypothesis, deducible from the independently motivated iterative application of the two (perhaps unifiable; see Kitahara 1997) transformational rules, Move (Attract) and Merge. The idea is to *explain* the fundamental construct 'syntactic relation', e.g., c-command, as opposed to defining relations on already built tree structures, as is necessitated by a representational, 'rule-free' approach,

3. For a critical discussion of a possible problem confronting the particular Internalist-Functionalism inherent in contemporary Minimalist syntactic theory and analyses, see Epstein (2003).

such as GB theory. (In this regard see also Uriagereka's 1999 Multiple Spell Out proposal, eliminating the disjunction in Kayne's representationally applied 1994 LCA, by appeal to cyclic (derivational) structure building, and non-surface application of the LCA. See also Lasnik 2001 for an insightful overview of derivation and representation within the Minimalist framework.) Thus, in the rule-based Minimalist approach, iterative application of well-defined transformational rules is assumed (contra 'Move-α'). Thus, it would be odd indeed to pay no attention to the form of the rules, intermediate representations, and the mode of the iterative rule application. Similarly, Chomsky's (1995) abandonment of a virtually rule-free, hence 'all-at-once', theory of D-structure generation, invites, if not requires, investigation of the empirical content of the rules and their manner of application. If, contra Move-α, there is iterative well-defined (cyclic) rule application, then within such a theory, there are by definition intermediate representations, which are generated as output of one rule application, and input to the next. If their existence is postulated, arguably the central shift from GB to Minimalism, we should maximize explanation by trying to deduce as much as possible from these independently motivated, binary-concatenation rules, and their iterative application. In the derivational model, we seek to deduce grammatical relations from the formal properties of the rules and/or their partially ordered application. In this level-free model, each transformational output is 'evaluated' by both PF and LF, as opposed to the GB Y-model. As Chomsky notes (BEA:3), his phase-based approach is also level-eliminating in the following sense:

> 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 Σ and φ interpret units that are part of something like LF in a non-cyclic conception.

In the derivational model developed here, not only do the rules themselves play a central explanatory role, but by virtue of feeding each transformational output to both PF and LF, each rule application is its own 'self-contained' Y-model (phasal) derivation. Again, the rule and the generative procedure play a central explanatory role. By contrast, in the 'rule-free' Principles and Parameters model, syntactic relations are necessarily *defined* on trees, and grammaticality is, by definition, *described* by filters that 'depict' illegitimate syntactic representations. Each mecha-

nism is, by definition, non-explanatory, since definitions-on-trees, like definitions in general, do not explain. We believe that syntactic filters, describing illegitimate configurations, might be replaced with more deeply explanatory postulates; specifically, a generative procedure from which the described filtering effects can be deduced, consistent with Einstein's grand (minimizing) aim of all science, and consistent with Whitehead's (1938:98) view that '. . . nothing is understood until its reference to process has been made evident.'

Consonant with the Minimalist Program, we assume that lexical items (consisting of certain features) play a central and ineliminable role. Perhaps, if we can discover the properties of lexical items, including their individual properties (features) of attraction and repulsion, then the way they arrange themselves in groups, as trees (or 'sentences') will fall out and thus be explained.

As Epstein and Seely (2002) discuss, this seems very similar in spirit to 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 Epstein and Axtell 1996). As J. Epstein (1999) notes:

> . . . the central idea is this: to the generativist, explaining the emergence . . . 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) generate the given regularity?'

J. Epstein (1999) assumes that one has explained 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* [Our emphasis, SDE/TDS].[4]

In J. Epstein's terms, 'if you haven't grown it, then you haven't explained it.' For us, if you define relations on (or appeal in any other way directly to the macrostructure) tree representations, you have failed to explain their properties. For example, to perform an 'end-of-the-line' bottom-up compositional semantic interpretation,

4. See J. Epstein (1999) for interesting discussion of the historical roots of such forms of explanation, and for discussion of the usual scientific situation in which more than one initial microspecification generates the macrostructure in question (thereby requiring more tests to distinguish the competitors' comparative empirical adequacy).

exactly retracing the steps of the bottom-up local pairing of two categories (the syntactic derivation), seems highly suspect. Furthermore, this postponement of interpretation, until all transformations have applied and the macrostructure is complete, in turn seems to necessitate non-minimal mechanisms such as chain-based trace theory, a look-back device whereby the admittedly important aspects of the derivation are encoded in the 'enriched', arguably Inclusiveness-violating, derived macrostructure itself.

As argued in Epstein and Seely (2002), representational theories with enriched derivation-encoding representational mechanisms, e.g., trace theory, are thus really 'just' a kind of derivational theory (cf. Brody 1995, 2001), but, we would suggest, the wrong kind. An important similar argument, the spirit of which we follow here, appears in Chomsky's (1995) discussion of another kind of successive cyclic movement, namely head-movement, in which he advocates a return to rule-based, generative theories of human knowledge of syntax. (But note this account is still *not* entirely chain-free.)

> 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-to-V followed by [$_V$ N+V], raising to Infl SDE/TDS], 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)

To summarize, we argue in Chapter 2 that, under independently motivated restrictions on the postulate 'syntactic object', there can be no 'enriched representational objects' such as chains, as one would expect if the derivational approach is on track. (Nonetheless, many Minimalist analyses continue to assume chains.)

1.2.3 The EPP

If there is no movement to or through the specifier of raising-*to*, this in turn necessitates the abandonment of standard formulations of the EPP. We explore the elimination of the EPP in Chapter 3. We suggest that it is the EPP, an unclear and (to the extent that it is clear) questionable principle, which motivates movement to Spec, *to*, and such movement is argued to be empirically problematic in Chapter 2.

Thus, in Chapter 3, we seek to eliminate the EPP as a universal principle, following those that have already challenged its efficacy in other domains, including McCloskey (1986, 1996, 1997) and within recent Minimalist assumptions, Martin (1999), and Bošković (2002).

One of the leading ideas of our exploration is that the EPP is redundant with numerous other independently motivated mechanisms of the grammar. While Epstein (1990) has argued that *certain* redundancies are empirically supported by evidence concerning varying types and degrees of grammaticality (see also Chomsky 1965), we argue here that the EPP is not independently motivated. Given its widespread redundancy with other principles, we argue for its elimination as a universal principle of grammar. Thus, in this respect we follow Chomsky's methodological lead regarding (empirically unsubstantiated) redundancy, as reflected in e.g., the following:

> Repeatedly, it has been found that these [redundant principles with overlapping empirical coverage SDE/TDS] are wrongly formulated and must be replaced by non-redundant ones. The discovery has been so regular that the need to eliminate redundancy has become a working principle in inquiry. (Chomsky 1995:5)

But suppose one adopts this strategy, seeking to eliminate such redundancy.[5] How do we determine which of the overlapping principles should be targeted for modification or elimination so as to remove the redundancy? It takes at least two to be redundant. One issue is of course the nature of the empirical overlap. Here the idea we follow is that there are multiple independently motivated principles, each overlapping with, i.e. intersecting, the empirical domain of the EPP; hence, it's the EPP that should be targeted for modification or elimination. In addition to the redundancy, the EPP remains unclear – 'a pervasive mystery' according to Lasnik (2002). Furthermore, on some formulations, the EPP (as a 'macro phrase-structural' principle) seems to exhibit precisely the formal properties prohibited by the very

5. For extremely illuminating discussion of Minimalist method, including the elimination of redundancy, see Kitahara (2003). For other important discussion of Minimalist method, see e.g., McGilvray (1999) and Smith (1999). For a highly critical perspective, see Lappin, Levine and Johnson (2000a). For important discussion of Lappin, Levine and Johnson's perspective, see Holmberg (2000), Reuland (2000), Roberts (2000), Piattelli-Palmarini (2000), Uriagereka (2000), and Epstein and Seely (2002: introduction). See also Lappin, Levine and Johnson (2000b) for a response to some of these responses. See also Edelman and Christianson (2003) for criticism of Minimalist method, along with Phillips and Lasnik's illuminating (2003) response.

heart of the derivational Minimalist attempt to *explain* macrostructure properties by deducing them from lexical properties, a minimal theory of transformations, and by hypothesis, language-independent principles of efficient computation.

The redundancy concerning the EPP (much of which has already been noted by previous researchers, as we'll discuss in some detail) can be informally illustrated as shown in Figure 1.4.

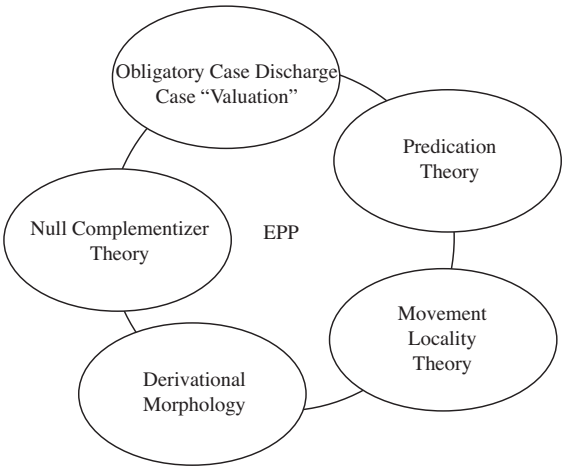

Figure 1.4 The EPP

Of course, we do not claim to have 'demonstrably eliminated the EPP'.[6] What we hope to have done instead is to suggest that analytical reliance on the EPP is reliance on something quite unclear, hence empirically problematic in its unclarity. Moreover, where it is clear, the EPP: (i) is highly redundant in its welcome empirical effects with numerous other independently motivated mechanisms; (ii) is empirically problematic to the extent that the predictions are clear (see Chapter 2); and (iii) (as noted above, on at least some formulations) violates both the spirit and letter of 'Minimalist law', threatening to mislead us into believing we have a genuine explanation of human knowledge of (un)grammaticality when we say, 'This data can be readily accounted for by the EPP.'

6. The five postulates intersecting the EPP might (contra this diagram) also display certain intersections with each other. This issue is not investigated here, nor do we *demonstrate* that the combined intersections with the EPP entirely subsumes this principle.