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ON SYNTACTIC METATHEORY

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Theoretical syntax has surely made significant progress in the Chomskyan era, but the field still lacks a coherent body of doctrine and one of its essential features continues to be the competition among different "schools" whose interrelationships remain poorly understood. The hypothesis adopted here is that the current diversity of approaches in this field is best interpreted as the result of multidimensional confrontation on at least three levels —epistemological, ontological and executional— and that nothing short of a genuinely theory-neutral, "metatheoretical", analysis will reveal the "parameters" of variation and thereby yield insight into the real issues at stake. In this paper, focus is mostly on negative evidence: many well-established taxonomic categories for grammars are critically examined and found to be partisan, ill-defined, or plainly contradictory as formulated. As a result of such critical srutiny, many of them are disregarded, others are kept, but variously reinterpreted in ways that render them legitimate, and a few extra ones are added to capture relevant oppositions apparently overlooked in the literature, but no detailed justification is possible here and none is provided. Simply, a chart is offered containing tentative parameters that seem well motivated at present. Systematic application and careful evaluation, however, must await the completion of research now in progress.*

1. Introduction

Although the field of theoretical syntax is perhaps less crowded nowadays than it was in the late seventies or early eighties, there still seem to be a great many approaches "on the market" whose interrelationships remain as poorly understood as ever. In fact, it is not easy to even determine which of the thirty-odd major syntactic frameworks that have appeared over the last forty years continue "alive". Can we still consider Case Grammar, Stratificational Grammar, Tagmemics or even Generalized Phrase Structure

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* This is a thoroughly revised and slightly expanded version of a lecture with the title "Parameters for a Metatheory of Contemporary Grammars", delivered at the ESSE/2 Conference held at the Université de Bordeaux II, September 4-9, 1993.

Grammar, as productive theoretical options nowadays? In the first two cases, they seem to be institutionally dead or agonizing, since —excluding Cook, and Lamb himself, perhaps— there do not seem to be any Case or Stratificational grammarians anywhere any more. Tagmemics is institutionally alive in the sense that people who call themselves "tagmemicists" keep at work, but the theory itself has not developed in any substantial way since the early seventies at least. Lexical Functional Grammar, as largely a phenomenon of the eighties, is supposed to be institutionally alive, and has even merited special chapters in such well-known surveys of the field as Sells (1985), Horrocks (1987), McCloskey (1988), or Droste and Joseph (1991), but there do not seem to have been any major theoretical, or even implementational, changes between Kaplan and Bresnan (1982), Bresnan (1987), and "current" LFG, if such an entity exists. As to GPSG, also granted special attention in Sells (1985), Horrocks (1987), McCloskey (1988) and Borsley (1991), if not institutionally dead, at least theoretically speaking it died with Gazdar et al. (1985), or perhaps, as officially maintained, really developed into Head-Driven Phrase Structure Grammar, one of the youngest frameworks; but again, have there been any significant theoretical changes between Sag and Pollard (1987), Pollard and Sag (1988) and "current" HPSG? I do not know of any. What's more, my impression is that "frameworks" such as LFG, GPSG or HPSG are not really "theories" at all, but just "formalisms" built in such a minimalistic way from the very beginning that practically no progress was possible in principle! No wonder, then, that they have remained stable, but that arguably means that they are theoretically dead, too.

Examples of conceptual stagnation could easily be multiplied, but, of course, which theoretical frameworks are "alive" is a controversial issue and too stringent a criterion in this respect can easily be reduced *ad absurdum* by invoking an equally stringent view of identity, so let's be less radical and grant that, as generally assumed, at least such major frameworks as Principles and Parameters Theory, Head-Driven Phrase Structure Grammar, Lexical Functional Grammar, Functional Unification Grammar, Categorial Grammar, Montague Grammar, Relational Grammar, Tagmemics, Systemic Functional Grammar, Dik's Functional Grammar, several other broadly "functional" approaches like the Prague School's, Givón's, Kuno's, Haiman's, Martinet's, etc., are all alive and kicking. To those we may add Labovian grammars, Text Grammars of several kinds, and various other approaches that have appeared or re-appeared in the last few years, Sadock's Autolexical

Syntax (1991), Hudson's Word Grammar (1984; 1990), Starosta's Lexicase (1988), Langacker's Cognitive Grammar (1987; 1991), Kroch and Joshi's Tree Adjoining Grammars (Joshi 1987; Kroch and Joshi 1987), Van Valin's Role and Reference Grammar (1990; 1992), etc. The list is by no means exhaustive. Only major, internationally established, approaches have been mentioned. However, that's about two dozen to choose from, and, needless to say, P&PT, CG, FG, etc. are not monolithic theoretical corpora, but flexible frameworks within which there is a lot of elbow-room for discrepancy at all levels.

In view of such baffling diversity, Mahmoudian's recent statement to the effect that contemporary theoretical linguistics is rapidly approaching the state of Kuhnian "normal science" (1993, xv), current debate being almost exclusively centered on terminological issues (Mahmoudian 1993, 9), seems to me barely understandable, unless his optimism stems from the fact that, although his book bears the title *Modern Theories of Language*, he really meant "current disputes within our [Martinet's] school", in which case it may be appropriate. As regards work outside that narrow functionalist trend, he does not seem to be aware of any published after 1977, so the credit to be granted to his simplistic analysis of the state of the field is inversely proportional to his apparent lack of familiarity with most of it. On the contrary, as anybody knows who has taken the trouble to acquaint himself with even a tiny fraction of what is getting published in major journals at present, strictly speaking, the corpus of really established linguistic doctrine is very small, and even that is full of apparently terminological mismatches which, when carefully inspected, turn out to disguise much deeper discrepancies. Thus, Moravcsik and Wirth (1980), Droste and Joseph (1991) or Botha (1992) contain much more accurate and reliable overall perspectives than Mahmoudian in this respect.

In these circumstances, no honest scholar can avoid asking himself such obvious questions as what to believe in, what to do research on and who with, what are the real tensions in the field, in what respects are current theoretical alternatives complementary, or just compatible, to what extent are eclecticism or scepticism respectable academic strategies, and ultimately, and perhaps more peremptorily, what should one teach one's students and how can he justify his choices. The easy way out of this maze is to stick to the orthodoxy of the "school" predominant in one's academic environment —assuming there is one unquestionably predominant framework, which is seldom the case— but, of course, fashion or number of adepts, although

important sociological considerations, are not solid enough scientific grounds to make all one's work rest on. Critically-minded scholars, at least, will need better academic reasons than those. Therefore, somebody should make the effort to clarify the real options in the field.

The problem is even more complex, though. Even if one has somehow made up his mind to follow a particular approach, it is very important to distinguish between its constitutive tenets, parallel, mutually supporting beliefs in neighboring fields (e.g. psychology, sociology, computer science) and clearly peripheral aspects of ideology, design, notation, and implementation. Otherwise, he will not be in a position to contribute to the progress of the field in any substantial way. The usual "take-it-or-leave-it", "buy-the-whole-packet" attitude of sectarian school-fans is based on the assumption of absolute motivation of linguistic doctrines, but that is completely gratuitous. On careful inspection, all grammatical frameworks consist of just a few key conceptual elements concerning matters of principle and hundreds of auxiliary hypotheses and peripheral, usually hardly-motivated, assumptions on so-called "implementational" or "executional" issues. Unfortunately, though, telling the former from the latter is not always easy.

To mention only the most obvious example: Chomsky's linguistics and his psychology-biology are generally sold as if they formed a perfectly integrated doctrinal whole, and, for insiders, they do, but only because they uncritically grant Chomsky's argument from "poverty of the stimulus". On close inspection, there is no necessary connection between the linguistic and the psychological components of the standard Chomskyan packet. Indeed, very few people seem to be aware of the fact that Chomsky's argument from "poverty of the stimulus", on which the whole strategy hinges, has been losing strength with every major theoretical breakthrough since the early seventies, and that the Language Faculty has been shrinking proportionally, to the point that, nowadays, in Chomsky's "minimalist" framework (Chomsky 1991; 1992; Chomsky and Lasnik 1991) it is close to an empirically empty hypothesis. Actually, the principles constituting the key modules have now been shifted out of the Language Faculty and reinterpreted as input-output conditions imposed by neighboring (i.e., non-linguistic) cognitive systems, so practically nothing remains within UG. Thus, some Chomskyan scholars nowadays (e.g. Speas, personal communication) readily admit that the Language Faculty may after all be "a very small language faculty", but, in my view, we could, and should, go further than that. We could scrap the whole idea at no cost. This view is still very unorthodox for

a Chomskyan to adopt nowadays, but nothing would happen if we forgot about the notion that UG characterizes a separate mental system. The few general principles of any substance that still might be attributed to UG could alternatively be seen as properties of the human cognitive system in general. Indeed, as pointed out by Sterelny (1992, 5-7), Soames (1984; 1992, 191), or Katz and Postal (1991, 527-8, note 8), the psychologistic interpretation of UG has become a conceptual strait jacket that does more harm than good to the credibility of Chomsky's grammatical doctrine. In Sterelny's words:

> If we take transformational grammar to be a theory of linguistic symbols, it is a promising and well supported research program. If however, we take it as a theory of linguistic competence, then it is speculative cognitive psychology. (1992, 7)

Surely, this is a highly controversial issue and this is not the occasion to go into it in depth. My general point stands, though: to make responsible choices on what to teach or do research on, one has to be really well informed on all the alternatives and able to "deconstruct" them and tell issues of principle from auxiliary hypotheses and executional preferences, and that is a very demanding assignment. People like Mahmoudian (1993) just distort the picture by ignoring the conceptual complexity of the field. Much more realistically, a couple of years ago Richard Hudson wrote in his editor's preface to the series of Linguistic Theory Guides currently published by Routledge:

> There just aren't enough hours in the day to read more widely, and even if there were it wouldn't help much because so much of the literature is simply incomprehensible except to the initiated. The result is that most of us cultivate our own garden reasonably conscientiously, but have very little idea of what is happening in other people's gardens. (1990b, viii)

Of course, that attitude is deontologically unacceptable and objectively undesirable insofar as it hinders academic communication, fosters parochialism and dogmatism, makes people waste effort in reinventing wheels much too often and ultimately represents a serious obstacle to the progress of our discipline, as pointed out by Hudson, (and, ironically, by Mahmoudian, too!), but it is understandable to the extent that almost nobody can cope. The complexity of the field is such that nothing short of deep technical knowl-

edge of the alternatives and the role they play within each of the competing grammatical frameworks would do, and to that must be added considerable sophistication in disciplines like epistemology and philosophy of science if the scholar aims at a minimally enlightening perspective on what's going on. Obviously, such qualifications are beyond the powers of most of us.

The difficulty of the task, therefore, is an objective fact, but there is another factor charitatively silenced by Hudson, i.e., that the proponents of the respective linguistic approaches typically are not helping much to clarify the theoretical landscape. Stockwell put it very clear more than a decade ago:

> a failing of much contemporary syntactic work is that many theorists have insulated themselves against comparison and persist in working within a single tradition, generally comparing their work only with that of their immediate colleagues and students, to such an extent that communication between advocates of different theories has nearly ceased to take place in our profession. (1980, 353-4)

Indeed, the field has changed substantially since 1980 in other respects, but not in that one. As Hudson says, most linguists do their own thing and pay no attention whatsoever to other approaches. Some even make this a matter of principle, on the ground that, at bottom, theories are incommensurable. According to this view, there is no point in comparing them or looking for compromises or bridges across their boundaries. Others do not assume strict incommensurability, but maintain that comparisons are relevant only when theories share the same domain, scientific values and objectives. These often find an excuse to ignore other traditions by failing to see any meeting ground between their respective approaches. Last, but not least, needless to say, underlying linguistic egocentrism there often are plainly dogmatic, self-defensive attitudes.

The motives, thus, may be different, but the net outcome is much the same: many linguists still see intertheoretical discourse as necessarily involving misinterpretation, unwarranted simplification or even a menace to the integrity of their paradigms. Consequently, there is a tendency to overstress shallow differences, a perceptible lack of interest in intertheoretical discussion and clarification —when not plain distrust of metatheoretical research— and often fierce resistance to being "reduced" or even interpreted in terms of any categories but one's own.

Obviously, misinterpretations and simplifications must be avoided, but theories must remain scrutable, effable and comparable, or linguistics will never become a mature science. Pronouncing theories incommensurable amounts to abandoning rational discourse and all hope of serious progress. On the other hand, the failure to see common ground for comparison and evaluation often results from looking for it at a very shallow, impressionistic, level of analysis.

Downing's recent account of the relationship between Halliday's SFG and Chomsky's P&PT (1991) is a case in point. Downing sees "no meeting ground" between what she calls "these two main present-day linguistic schools of thought and practice" (39), with one exceptional attempt at "rapprochement", i.e., that "one strand in generativist theory has turned semantic while some systemicists are definitely becoming more generative" (39-40).

That is a beautiful example of the kind of impressionistic work of intertheoretical comparison responsible for much of the confusion in the field. Of course, no generativist will admit that he has "turned semantic". Generative work has always "been" semantic. Indeed, generativists started developing explicit semantic theories when nobody else had the slightest hint of what a semantics for natural language might look like. On the other hand, I very much doubt whether any self-respecting systemicist will ever admit to having "turned more generative". "Generative" is a purely accidental, and rather unfortunate, technical term for "explicit", and I cannot imagine the need for even the earliest "scale and category" work of Halliday's to become any more "generative" than it was from the very beginning. A (set of) paradigm(s), by definition, "generates" a precise set of options, just as a (set of) PS rule(s) does, and that's that. Far from clarifying the issues involved, such amateurish use of categories entangles them further to the point where no communication is possible. The starting assumption is the opposite from Mahmoudian's, but the effect is similar: to reassure one's followers that they are right and that nothing is to be gained from contact with one's academic competitors.

Careful comparison and evaluation of rival theories is absolutely necessary if we are to make progress, but we cannot expect real breakthroughs in our understanding of the field unless we are prepared to forget about the informal use of our own categories and look for new, genuinely theory-neutral ones. Moravcsik (1980) and Stockwell (1980) were pioneering attempts to develop such a perspective in syntax, but metatheoretical analysis on such a large scale has not been pursued, as far as I know, and any

conclusions based upon that work nowadays would need extensive revision anyway to incorporate the new developments of more than a decade. Nevertheless, the need felt for careful comparison of syntactic approaches has inspired a few textbooks (Sells 1985; Horrocks 1987; Borsley 1991), excellent review articles by McCloskey (1988) and Wasow (1985; 1989) and several important syntax conferences whose proceedings have subsequently been published (see Huck and Ojeda 1987 on discontinuous constituents; Baltin and Kroch 1989 on phrase structure; Droste and Joseph 1991 on the general form of grammars, etc.). The common defect of such works is that they juxtapose brief summaries of a few of the alternative frameworks leaving all further analysis to the reader. The current series of Linguistic Theory Guides published by Routledge under the general editorship of Richard Hudson, on the other hand, is a very welcome major effort to improve inter-school communication by making the essential concepts of alternative theories easily accessible to the non-specialist in a compact, sufficiently explicit form, but, of course, awareness is only a preliminary step towards clarification. The real job begins only when scholars know the alternatives well enough.

In my view, nothing short of a full metatheoretical study of the whole field at a fairly high level of abstraction will yield insight into the complex issues involved and make systematic comparisons and evaluations possible on independent grounds. Of course, that is an enormous task, certainly well beyond the strength of any single scholar, but I am convinced that it is nonetheless a top priority at present. For that reason, since 1989 I have been working on a research project, sponsored by the Spanish DGICYT under the title *Parámetros para una Metateoría Lingüística* (PB 089-535), some minor results of which have already appeared in print (Escribano 1990; 1992a; 1992b). In this article, which is a sort of informal report on that research, I would like to review critically some of the work of potential metatheoretical import done by other scholars in the last decade or so.

2. State of the Art

To my knowledge, very little explicitly metatheoretical work (in the sense above) has been done on a large scale after the pioneering essays by Moravcsik and Stockwell (1980), Escribano (1989) and the short introduction in Droste and Joseph (1991) being closest to them in spirit. Major

works in the philosophy of language like Katz (1981) and Chomsky (1985), of course, contain very insightful global remarks on current views of linguistic ontology and the study of language. Carr's rather weak thesis (1990) pursues the same broad topics, and so does Botha (1989) and specially in Botha (1992), an excellent, and extremely witty, account of current issues in linguistic ontology. Itkonen (1978; 1983) apparently contain very relevant metatheoretical discussion, but both are out of print and I have not been able to consult them, nor the unpublished work by Keith Allan (1988) that Botha (1992) refers to.

Apart from such monographs, general introductory surveys of contemporary syntax (in particular, Sells 1985; Shieber 1986; Horrocks 1987; Wasow 1985; 1989; McCloskey 1988; Harlow and Vincent 1988; Droste and Joseph 1991; Borsley 1991; Lightfoot 1992; Newmeyer 1992) do contain partial characterizations of various syntactic approaches from a comparative perspective. Needless to say, there are also many occasional pronouncements by linguists on what makes their approaches different from other linguists's in particular areas of grammar, but they are too scattered and too heterogeneous to be taken into consideration in a general survey like this, so, with a few exceptions, I will not discuss them here. Finally, there are a few systematic comparisons between schools (see Perlmutter 1983 on RG vs. FG; Nichols 1984 on various "functional" approaches; Dik 1978; 1989 on the "functional" vs. the "formal" paradigm; Butler 1990 on SFG vs. FG; Downing 1991 on TGG vs. SFG, etc.) some of which imply a certain amount of metatheoretical analysis and will be taken into account in what follows.

It must be immediately pointed out, however, that, in general, unless the perspective is explicitly metatheoretical, such attributions must be taken *cum grano salis,* for various reasons: a) they tend to be partial, typically focussing on a single property or a small cluster of related properties assumed to be generally relevant just because they happen to be helpful in distinguishing whatever subset of frameworks (e.g., generative approaches, as in Sells 1985; Horrocks 1987; Wasow 1985; 1989; McCloskey 1988; formalist and functionalist views in Dik 1978; 1989; Salverda 1985; and Halliday 1985; etc.) may be in confrontation; b) they tend to concern low-level matters of architecture and implementation instead of dealing with the underlying conceptual discrepancies; c) they usually are heavily theory-dependent, if not "cheerfully partisan", as Horrocks says (1987, 320). At least, they often beg key questions by, for example, taking for granted

notions like "competence", or "UG", as the undisputed object of linguistic inquiry, or using terms like "modularity", "naturalness", "complexity", "explanation", and the respective adjectives, in undefined, uncritical ways, etc.; and d) many are conceptually incompatible among themselves anyway, or lead to untenable conclusions in their application, or both.

In general, apart from their parochialism, a common defect is that scholars use a set of predicates which might perhaps correspond to the values of metatheoretical parameters, but usually fail to specify the parameters themselves, their domain and range of variation, and the various implicational, co-variational, and dependency relationships that exist among them. Thus, however perceptive some of the established taxonomic labels may seem (e. g., those that sanction the classification of grammars as "modular" vs. "non-modular", "monostratal" vs. "polistratal", "derivational" vs. "representational", "declarative" vs. "procedural", "transformational" vs. "non-transformational", etc.) most of them do not resist even a mildly critical scrutiny. (See Escribano 1992a for a careful analysis of the opposition modular / holistic).

In what follows, I would first like to illustrate these general criticisms with a highly selective survey of the taxonomic labels used by various linguists during the eighties. Exhaustiveness is clearly out of the question, of course, but I hope a few carefully chosen examples will suffice to show where we are at present and why a different kind of thinking is required. Then, in a more constructive vein, in the third section of this paper I will try to order and reinterpret some of those remarks in terms of a systematic set of parameters and values, suggest a few others, and show some of their most obvious interrelations.

Since in many cases the authors themselves gloss over their distinctions without proposing a conveniently concise label for the "parameter" involved, I have freely edited their names or even replaced them by more perspicuous ones without altering the substance. On the other hand, as I said above, each scholar usually mentions just three or four criteria he considers relevant, and these sometimes coincide and overlap in complex ways, sometimes do not. For expository purposes, therefore, I have decided to conflate all such proposals and present all the distinctions in chart-form mentioning some of my sources succintly within parentheses. I am aware of the fact that merging criteria from different conceptions entails a certain risk of oversimplification, but I hope to have incurred in no serious misrepresentation.

Anyway, this is just a convenient time-saving trick to impose some order onto the chaos of overlapping categories and abbreviate discussion.

An additional difficulty is that my sources generally make no attempt at establishing hierarchical, logical or even explicit ordering relations among the proposed dimensions, which are merely listed. In a general survey like this, therefore, the result of conflating such widely diverging views would have to be a long list of distinctions in random order, obviously not an ideal expository strategy. The critical work to be done, however, consists in deciding which of the dimensions are general and neutral enough to become parameters and what their values are, establishing types and a hierarchy among them, and showing their various dependencies and interrelationships, i.e., constructing a system. Properly, that is my concern in section 3 of this paper, but some kind of ordering had to be imposed even at this stage, if only to facilitate enumeration. Therefore, following Escribano (1989) and somewhat anticipating the topic of section 3, in Chart 1, I have grouped the criteria into three sets roughly corresponding to a) philosophical and epistemological issues, b) conception of the specific object of study, and c) architecture and design of grammars, respectively. The relative ordering of the distinctions within each group is not meant to be relevant and must be interpreted as merely convenient for expository purposes.

3. Discussion: Towards a Metatheoretical Perspective

As Chart 1 shows, there is no lack of suggestions concerning dimensions of contrast between syntactic approaches, particularly between generative ones, whose internal controversies have perhaps been paid more attention in recent times (Sells 1985; Horrocks 1987; Harlow and Vincent 1988; McCloskey 1988; Droste and Joseph 1991; etc.), but, in spite of my attempt at identifying the nature of the oppositions, conflating them, and presenting them in an orderly fashion, nothing like a systematic account emerges. Many of the proposed distinctions overlap, some are obviously redundant, many are clearly theory-bound and lacking in generality, etc. There is a preliminary, fundamental problem, however, i. e., that many of the predicates involved are used in an informal way that conceals their essential vagueness. When an attempt is made to use them as precise taxonomic criteria, they turn out to be inconsistent and, indeed, inapplicable unless carefully redefined.

CHART 1: PARAMETERS FOR A METATHEORY OF GRAMMARS - A FIRST PASS

A. *Philosophical and Epistemological Issues*

1. Ontological status of theories: Platonism / Conceptualism / Nominalism (Katz 1981; Carr 1990; Botha 1992).
2. Ontological restrictions on theoretical entities: Abstractness / Non-abstractness (Stockwell 1980; Newmeyer 1992).
3. Data-selection: Instrumentally determined / theory-determined / observer's judgment / native speaker's intuitions, etc. (Moravcsik 1980).
4. Scientific objectives: Description / Generation / Explanation / Simulation (Moravcsik 1980; Stockwell 1980; Newmeyer 1992).
5. Type of explanation: Functional / Formal (Dik 1978; 1989; Salverda 1985; Foley and Van Valin 1984; Halliday 1985; Thompson 1992).
6. Evaluation criteria:
   a) General: Explicitness / Consistency / Generality / Simplicity / Fertility / Veracity / Compatibility (Davis 1973; Moravcsik 1980).
   b) Specific: Psychological reality / Learnability / Parsability / Computability / Implementability (Moravcsik 1980; Stockwell 1980; Bresnan and Kaplan 1982; Droste and Joseph 1991; Berwick and Weinberg 1984; Harlow and Vincent 1988; Lightfoot 1992; etc.).

B. *Conception of the Object of Study*

1. Scope: Language / Languages / Dialects / Sociolects / Idiolects / Registers / Fragments / Styles / Corpora (Moravcsik 1980; Stockwell 1980; Halliday 1985; Droste and Joseph 1991; Newmeyer 1992).
2. Shape: Entity / Set of entities / Set of habits / Set of conventions / System / Procedure (Botha 1992).
3. Status: Autonomous / Heteronomous (Newmeyer 1983; 1986; 1992; Halliday 1985).
4. Consistency: Well-defined / Fuzzy (Matthews 1981; Halliday 1985; Droste and Joseph 1991).
5. Modus essendi: Physical / Behavioural / Biological / Psychological / Social / Cultural / Abstract (Katz 1981; Soames 1984; Gazdar et al. 1985; Carr 1990; Katz and Postal 1991; Droste and Joseph 1991; Newmeyer 1992; Botha 1992).

C. *Grammar: Architecture, Design and Implementation Issues*

1. Scope: Competence / Competence + Performance (Stockwell 1980; Harlow and Vincent 1988; Lightfoot 1992).
2. Mode: Declarative / Procedural (Stockwell 1980; Gazdar et al. 1985; Harlow and Vincent 1988).

3. Operation: Structure-producing / Structure-parsing / Structure-checking (Stockwell 1980; Moravcsik 1980; Lightfoot 1992).
4. Descriptive strategy: Monostratal / Polistratal (Perlmutter and Postal 1983; Gazdar and al. 1985; Blake 1990; McCloskey 1988; Pesetsky 1992; Borsley 1991; Lightfoot 1992).
5. Inter-level relationship: Co-representational / Derivational (Gazdar et al. 1985; Shieber 1986; Sadock 1991; Lightfoot 1992).
6. Control: Directional / Non-directional (Sadock 1991).
7. Orientation: Top-down / Bottom-up (Sadock 1991).
8. Driver: Lexicon / Syntax / Semantics / Pragmatics (Halliday 1985; Harlow and Vincent 1988; Droste and Joseph 1991; Lightfoot 1992).
9. Syntax-semantics correspondence: Isomorphic / Non-isomorphic (Droste and Joseph 1991).
10. Architecture: Modular / Integrated (Newmeyer 1983; Harlow and Vincent 1988; McCloskey 1988; Brown 1991; Sadock 1991; Pesetsky 1992; Lightfoot 1992).
11. Phase: Paradigmatic (System-centered) / Syntagmatic (Structure-centered).(Halliday 1985)
12. Combinatorics: Pattern-oriented / Word-oriented (Droste and Joseph 1991).
13. Computation-procedure: Algorithm / Repertory (Lightfoot 1992).
14. Weak generative capacity: CF / CS / Recursive (Harlow and Vincent 1988; Droste and Joseph 1991; Pullum 1992).
15. Range: Sentential / Textual (Halliday 1985).
16. Primitives: Units / Relations (Stockwell 1980; Halliday 1985).
17. Focal relation: Constituency / Dependency (Matthews 1981; Hudson 1987).
18. Status of syntactic relations: Primitive / Derived (Perlmutter and Postal 1983; Bresnan 1987; McCloskey 1988; Blake 1990).
19. Category-complexity [A, v] or [[A, v] + / , \] (Shieber 1986).
20. Type of statement: Rules / Principles (McCloskey 1988, Pesetsky 1992).
21. Plasticity: Parametric / Non-parametric (Lightfoot 1992).
22. Force: Laws / Tendencies (Droste and Joseph 1991).
23. Rule-effects on strings: Concatenative / Head-wrapping (Shieber 1986).
24. Derivational device: Transformations / Revaluations / Lexical rules (McCloskey 1988; Harlow and Vincent 1988).
25. Gap-cancellation device: Move alpha / Feature-inheritance (Pesetsky 1992).
26. Computation scope: Local / Global (Horrocks 1987; Harlow and Vincent 1988; McCloskey 1988; Borsley 1991; Lightfoot 1992).

3A. *Philosophical and Epistemological Issues*

An important issue in group A (see A1) is the status granted to theoretical entities on the general ontological level, of which the abstractness controversy alluded to in A2 is but a side effect, in my view. Since mediaeval times, philosophers of science distinguish at least three possible interpretations of universals, i.e., realism, conceptualism, and nominalism. I think realism is much too general a category and should be split into various subcategories, at least a) Idealistic (= Platonic) realism (e.g., Katz), b) Aristotelian realism (= hylemorphism)(e.g., Chomsky), and c) Popperian objectivism (e.g., Carr). The conceptualist position more or less coincides with our common-sense empiricist view of abstract notions and, following Katz (1981), is that generally attributed to Chomsky, but I do not agree with that attribution (see Escribano 1989). That point of view is explicitly adopted as a matter of course by Stockwell, who criticizes the generativists for assuming their theoretical constructs to be innate mental realities:

> even though the constructs are the creations of ingenuity applied to extend metaphors from computer technology, formal logic and the mathematics of axiomatic systems to natural language, without empirical evidence that these metaphors reflect any kind of mental reality. (Stockwell 1980, 354)

As to nominalist interpretations (e.g. Quine's), nowadays they are comparatively out of fashion within Linguistics as such, but a closely related view, instrumentalism (theories are just tools involving no truth claims, etc.) is still quite popular and is to be found in the writings of linguists like Pike, Halliday, and their followers.

This issue is one of general ontology and linguists tend to avoid explicit pronouncements on "philosophical" matters, which are perceived as marginal at best (see Katz 1981, 42, note 1). Sag and Pollard's attitude is quite symptomatic of this lack of interest:

> Much as one can do quantum mechanics without ever developing commitments about (or even caring about) the ontological status of the theoretical constructs, a science of signs is imaginable in the absence of certainty about whether they are in the mind, out of the mind, or somewhere in between. (Sag and Pollard 1987, 8)

The question is far from settled at the philosophical level (see Carr 1990 and the papers in *Linguistics and Philosophy* 14, 1991), but nowadays the predominant view seems to be realism in its various forms (Platonic, as in Katz 1981, Katz and Postal 1991, Platonic or Popperian, as in Montague Grammar, GPSG, and HPSG, and Aristotelian, or materialistic, as in Bresnan's LFG and Chomsky's P&PT 1986, 23). As I said elsewhere (1989), in spite of Sag and Pollard's confident dismissal, this choice is far from irrelevant to the linguist's work. On the contrary, the realist compromise has significant implications for many other methodological, conceptual and implementational choices at all levels. A realist position, for example, leads to the search for "the" unique true theory and to adopt an evaluation procedure based on criteria like veracity, or "psychological reality", instead of simplicity for simplicity's sake, and this in turn constrains theoretical entities within the limits fixed by plausibility or compatibility with external (neurological, biological, psycholinguistic) evidence. Eventually, this choice leads to such moves as eliminating extrinsic ordering of rules, reducing transformations to Move Alpha, or dispensing with them altogether (LFG, GPSG), dropping all language-specific rules and adopting general principles instead (P&PT), etc.

On the other hand, an instrumentalist view at the ontological level may lead to theoretical work guided exclusively by aesthetics or economy-driven evaluation criteria (i.e., theories making no truth-claims, inspired by the ideals of simplicity, symmetry, immediate applicability, etc.), and also to eclectic (Pike), opportunistic (Halliday), or merely agnostic views. The philosophy inspiring much work in computational linguistics, and such notions as the idea of complementarity of perspectives, or the notion that theories are valuable or not depending on the practical goals aimed at (Tagmemics, Systemic Functional Grammar) are cases in point.

In this perspective, the question of "abstractness" alluded to in A2 appears, not as an independent parameter, but as just a consequence of the adoption of a realist ontology in A1 coupled with a particular view of the *modus essendi* of language, to which we turn in B5 below.

The relevance criterion for the selection of data mentioned in A3 is an obviously important methodological point. However, as stated in Escribano 1989, the key opposition at this level is of an epistemological character and concerns the role attributed to theories versus data, and specifically the difference between constructivist theories like P&PT, in which the selection criterion is inmanent, i.e., we let the theory itself decide whether a datum is

or is not to be accounted for (e. g., whether an expression is part of a language L), and descriptivist theories, whose role is merely to account for facts previously defined as relevant by extratheoretical criteria, be they physicalist (as in Bloomfieldian linguistics), phenomenological (= native speaker intuitions, as in classical TGG, the emic/etic distinction, as in Pike's Tagmemics) institutional, historical-geographic, social, etc. The relevant parameter, therefore, may be formulated as follows:

A2': Theory-data interaction: Descriptivist / Constructivist

From this option follows a bound methodological choice, A2.1:

A2.1: Data-selection criterion: Theory-internal / Theory-external and another subparameter, A2.1.1, classifying externally-determined data selection criteria into geographical, political, historical, social, stylistic, thematic, methodological, etc. (see Chart 2).

A4, concerning the goals of linguistic theories, is obviously relevant as well. Many discrepancies at all levels ultimately reduce to a difference in scientific objectives. The options in A4 are ordered from left to right in what I take to be a growing order of ambitiousness. One point must be emphasized, though: nowadays, grammatical theories generally no longer content themselves with description. Explanation seems to be a more or less universally accepted theoretical goal. "Explanation", however, is itself an ambiguous term, as we'll see shortly. Hence the relevance of parameter A5.

Generative (= explicit, formal) description, on the other hand, is not generally assumed, for a variety of reasons, some of principle (the fuzziness contention expressed by Matthews, Halliday, etc.) and some contingent. In fact, whether even current P&PT-inspired grammars are "explicit" is not any more clear. One of the main criticisms levelled against GB work by GPSG adherents is precisely that proposals are never worked out in sufficient detail to be testable against real data (see Gazdar et al. 1985; Pullum's critique of Chomskyan work in Pullum 1989; and Chomsky's rejoinder in Chomsky 1990) .

At the opposite extreme, certain theories aim not only at explicit description and explanation, but also at supplying a sound basis for the implementation of systems that simulate the speaker's linguistic behaviour (e. g., Stratificational Grammar, Bresnan's LFG, Dik's FG). Therefore, simulation should be added as an extra possible scientific objective in this parameter.

Let's now look at the question of explanation explicitly brought up in A5. For many functionally-biased theoreticians (see, among others, Dik 1978, 1-6; Dik 1989, 2-7; Foley and Van Valin 1984, 3-24; Salverda 1985; Halliday 1985, xxviii-xxx; Thompson 1992, 37), the key opposition in contemporary linguistics is that between functionalism and formalism or, more generally, that between the formalist and the functionalist "paradigms". In Thompson's words, "Research into grammar since about 1975 has been widely recognized as being guided by two different paradigms, which have come to be called the functionalist and the formalist approaches" (Thompson 1992, 37).

According to Dik and Thompson, those two paradigms differ in their assumptions about language and its acquisition, their methods of study, the criteria used for the selection of evidence, and the relations between different components of the grammar. The use of such general labels as "formalist" or "functionalist", however, presupposes an unambiguous definition of those terms which nobody has cared to provide and conceals the radical heterogeneity existing within such broad groups. Indeed, Thompson's "functionalist" approaches include, apart from the Prague School, the work of linguists like Bolinger, Comrie, Keenan, Dixon, Silverstein, Greenberg, Haiman, Halliday, Martinet, Kuno, Nichols, Van Valin's R&RG, Pike's Tagmemics, etc., but not highly formalized "functional" grammars like Bresnan's LFG, Kay's FUG, Montague Grammar and Categorial Grammar. Dik's FG, however, is accepted as a "functionalist" approach, in spite of its extreme emphasis on formalization. Thompson's "formalists", thus, would include Chomsky, Montague, and all their various followers (P&PT, RG, LFG, MG, CG, GPSG, HPSG, DRT, etc.).

The question is: why such groupings? Functionalists (e. g., Dik) do not deny the decisive importance of biological and cognitive constraints, although generally share the view that language is also critically affected by contextual and communicative or usage-derived constraints. On the other hand, the most conspicuous of the "formalists", Chomsky, has always played down the role of formalism and, in fact, since about 1970 has been explicitly declaring that formalization is premature and pointless (see Chomsky 1990, 146-7 for a recent statement to this effect). As regards, functional and communicative constraints, Chomsky has never denied their importance outside of UG and core grammar. Indeed, linguistic phylogenesis must take contextual and communicative factors into account and, even ontogenetically speaking, in the current minimalist framework "functional" considerations play a

more decisive role than ever. The real difference, thus, would seem to lie in the type of explanation supposedly favoured by the respective paradigms, "functional" (see Dik 1986 for a particularly careful account of this notion) in one case, and "formal-nomothetic" in the other.

That, at least, is the theory. In practice, ignoring programmatic manifestoes, functionalist writings contain extremely few attempts at explanation of any kind, only a tiny proportion of those can be broadly interpreted as functionally inspired, and of these most are merely *ex post facto* rationalizations completely devoid of predictive power. Convincing aetiological explanations are extremely rare in the functionalist literature and tend to refer to trivial or definitional issues. Even Dik's supposedly "functional" explanation of Wackernagel's Law rests on the stipulation that "initial position is a natural position for placing communicatively prominent constituents" (Dik 1986, 4). This is correct for a very special kind of prominence, that of operators, but not for informationally prominent items in general. Indeed, the unmarked place for prominent constituents is rather final position (hence LIPOC!). The real question, then, is: Why should first position be more natural than last? And why should P1, and only P1, be insensitive to LIPOC?

The formalists' nomothetic view of explanation as grounded on the possibility of deriving facts from laws, on the other hand, is clearly insufficient, in fact, trivial, since the really explanatory issue is not whether fact F follows from principles P1...Pn, but rather, why P1...Pn take the form they do instead of other conceivable ones.

It must be pointed out, however, that explanatory strategy has changed noticeably in the so-called "formalist" side. In the last two decades, the grammar-constraining role of acquisition, performance, and biological considerations has increased substantially, as attested by the ubiquity of arguments based on such notions as "naturalness", "learnability", "parsability", "psychological reality", "minimum effort", etc. In other words, at least at the programmatic level, both groups are moving in the direction of "substantial" explanations and away from merely "formal" hypothetico-deductive ones, but in practice both are broadly "in the same boat" in this respect, i.e., still rather close to merely formal explanations.

There is a real issue here, but, in my view, it does not concern the opposition between so called "formal" and "functional" explanations in the strict sense, but, more generally, that between "formal" and "material", "substantial" or "aetiological" explanations, i.e., accounts that tell us why the laws

have the form they do instead of any other, and these may, but need not, rest on functional-communicative considerations. The causes may well be structural, anatomical, or biological, instead of "functional-communicative".

Dik's view of functional constraints is singularly comprehensive, since it encompasses the purposes, implementional means and circumstances attending language use (Dik 1986, 10), but it still begs the key question, by making functional prerequisites equivalent to "extralinguistic requirements and constraints imposed on natural languages". Why should intra-linguistic constraints be excluded? The answer can only be: because we want to believe that language is in no way autonomous. But why should human language absolutely lack inherent properties, and, anyway, how can we draw the line between arbitrary, and diachronically (but not synchronically) motivated aspects of language? This is one clear case, then, in which a widely agreed-on taxonomic distinction can be shown to rest on partisan prejudice and to be comparatively sterile or even impossible to apply in a motivated way. The relevant metatheoretical parameter is rather:

A5': Type of explanation: Formal-nomothetic / Substantial (=aetiological)

Lastly, some of the evaluation criteria mentioned in A6, such as explicitness, consistency, generality or veracity, tend to be taken for granted as definitional attributes of science, but they aren't. No doubt, issues in evaluation have mostly concentrated on the option simplicity vs. "naturalness" (or descriptive vs. explanatory adequacy), but few people realize that the evaluative role of consistency, explicitness, generality, etc., is intimately related to certain other assumptions. Consistency, for example, is out of the question if the object of study is defined with due respect to well-known sociolinguistic phenomena of variation (see B1,2). Taken as historico-geographic entities, languages are not amenable to consistent theoretical treatment. Of course, it is possible to define one's object of study in different terms, but then that is far from a trivial methodological point and calls for an independent parameter (see B5).

Explicitness, in its turn, also depends on an independent issue, i.e., one's views concerning the rigidity/fuzziness of language, hence B2. Even generality is a perfectly vacuous notion until the relationship between theory and data is made explicit (see A2' above). If one's theory merely describes a domain of facts defined by external criteria, then, obviously, the more comprehensive, the more highly-valued it is. However, if it is the theory that determines which phenomena must be accounted for, then exceptions do not

generate counterexamples (= loss of generality), but merely unrelated facts for which the theory need not (in fact, must not) take responsibility.

As to simplicity, its role depends on one's ontological position. For the instrumentalist, it is a decisive consideration. Given a realist standpoint, however, a theory is not deemed superior for being just simpler than another. Obviously, the truth may well not be simple. On the contrary, as Chomsky constantly reminds us, biological systems might turn out to be rather pedestrian, redundant and uninteresting objects from the mathematician's point of view. Correspondingly, veracity is a key consideration in a realist ontology and a completely irrelevant one in an instrumentalist scenario.

As regards compatibility (with other disciplines), it is or is not a relevant consideration depending on one's position in the autonomy / heteronomy debate, to which we'll specifically turn in B4 below.

Finally, A6 mentions a number of specific criteria which may or may not be operative depending on the compromises subscribed by the respective frameworks. These include psycholinguistic constraints like learnability or parsability, derived computation-theoretic ones like computability, complexity, etc. Their effect is to impose further restrictions on the linguist's work. I suppose their import needs no further comment. Grammars, indeed, differ considerably in their design depending on which of them, if any, are granted priority.

3B. *Conception of the Object of Study*

Many scholars have pointed out that linguistic schools differ very substantially in the way they conceive of their object of study. In fact, all the possibilities listed in Chart 1 under B1, among others, can be readily attested in the field. Detailed discussion of individual positions is out of the question here, but it must be pointed out that at the metatheoretical level of analysis that list is not a homogeneous set of values for a single parameter ("scope", according to our preliminary impressionistic characterization in Chart 1) but the result of the interaction of various "dimensions of contrast" which should be carefully distinguished.

A first major opposition turns on whether the object is or is not understood in representational terms i.e., as "language", (in various interpretations of the term), or as "knowledge of language", again, in various interpretations, as in Chomskyan linguistics. This opposition, apparently overlaps with that

concerning the *modus essendi* of language referred to in B5, since some representations are mental entities. However, at least one aspect of it can not be subsumed under B5, i.e., the epistemological opposition between first-order entities (objects) and second-order ones (representations of objects). Let's assume, therefore, that we remove B1, keep B5 pending further analysis, and add a new opposition:

Epistemic order: ± Representational

A second opposition underlying B1 turns on whether the object is conceived as a single universal entity (Human Language) or as a set of entities (particular languages). This distinction is clearly independent of the preceding one, since the four logically possible options are clearly attested in the field, i.e., there are scholars who consider their object of study to be [+ Representational, + Universal], [- Representational, + Universal], [+ Representational, - Universal], and [- Representational, - Universal]. It is obvious, therefore, that two parameters are involved, so let's add a second distinction, also partly replacing B1:

Scope: ± Universal

A third factor introducing variety in B1 is the fact that interacting with the two oppositions already discussed is an independent variable introducing domain restrictions according to geographic, political, social, stylistic, thematic, developmental or methodological criteria. For simplicity's sake, I will refer to it under the cover term "Restriction", without further analysis:

Restriction: Geographic / Political / Social / Thematic / . . .

Thus, variation concerning the way the object of study is understood involves so far not one, but at least three "parameters": two bipolar oppositions [± Representational], [± Universal], and a multivalued one [Restriction: Geographic / Social / Stylistic / Thematic / Methodological, etc.].

B2 in Chart 1 is similarly heterogeneous. In general, it refers to the ontological "shape" attributed to the object but also to its ontological sphere, i.e., as formulated, it partly overlaps with B5. The shape opposition between "individual" and "set" is relevant, though, but a practical difficulty is that certain grammatical theories, like P&PT, seem to hold several different views in this respect, and others are particularly vague about it. Thus,

Chomsky's object of study is at times "UG", Human Language, in general, or the ideal speaker's competence, all of them individuals belonging in this case to the biological or the psychological spheres of being (i.e., Chomsky is talking about an organ, a functionally isolatable mental system, or each of the possible states such entities may be in). Other times, however, the object of study is "I-language", and this, depending on the context, appears as a generative "procedure" (i.e., a set of events), or "a collection of SDs" (i.e., a set of complex entities). This indeterminacy is a serious ontological flaw in Chomsky's conception, as pointed out in Matthews (1991) and Botha (1989; 1992). Anyway, the opposition of "shape" between "individual" and "set", at least, is a relevant component of B2 and must be preserved at the metatheoretical level.

Shape: Individual / Set

As to the remaining dimensions of contrast which may be involved in our preliminary list in B2, i.e., whether language consists of a set or system of conventions, or of habits, or on the contrary is a biological, psychological or abstract algorithm or procedure, the differences introduced by the modifiers in such expressions follow from the distinctions needed in B5 anyway, so I will tentatively assume that no independent parameter need be invoked in this regard, but the ontological difference between being a "thing" (= an "ergon", in Humboldtian terms), and being an "event", or a set of events (= a process, a procedure, an "energeia") must somehow be expressed at the metatheoretical level, so we need a "type" parameter anyway:

Type: Thing / Event / Process / . . .

Thus B1 and B2 together correspond to B5 plus five new parameters in Chart 2:

Epistemic order: ± Representational
Scope: ± Universal
Shape: Individual / Set
Type: Thing./.Event / Process, . . .
Restriction: Geographic / Political / Social / Thematic / Stylistic . . .

In my view, the opposition concerning "consistency" or "degree of rigidity" of the object of study (see B4) might well be dispensed with at the metatheoretical level, since it follows from the options taken in A. Very briefly: fuzziness results only when data selection is done according to

extratheoretical (geographic, political, social, aesthetic...) criteria. Under generative assumptions concerning the constructive role of theory vs. data, and granted data selection is controlled by inmanent principles, the object of study is rigid (= mathematically well-defined) by definition.

The issue referred to in B3, although made much of by numerous scholars, seems to me merely terminological at present. It all hinges on the meaning of "autonomous". In a modular approach to the mind like Fodor's or Chomsky's, language is autonomous by definition, (just as grammar and all its modules are [Chomsky 1981, chs. 1 and 2]). However, that kind of autonomy is rather uninteresting from our point of view, since it does not imply that the linguistic module is independently capable of generating linguistic behaviour. The original controversy concerning the autonomy of syntax in the late sixties and early seventies was much more substantial, since what was at stake was the respective generative role of syntax and semantics, but in current generative work, that issue has dissolved along with such simplistic division of grammars into components.

The substance in the autonomy-heteronomy of language debate, anyway, reduces to the option taken in B5, the real moot point at this level. Obviously, if language is taken to be something biological, or socio-cultural, linguistics will be a chapter of biology, sociology or anthopology, its explanatory principles will ultimately come from those sources, and language will be heteronomous. Only if we follow Katz and Postal, Soames, Sterelny, Carr, etc. and interpret linguistic reality in abstract, ideal Platonic, or Popperian World-3 terms, will language —and, correspondingly, linguistics— be autonomous (assuming, of course, that linguistics does not become a branch of logic or mathematics, in which case we would be facing just another kind of heteronomy).

Newmeyer, however, (Newmeyer 1986, 4-6, 21, 32-33, 71-72) uses the label "autonomous" linguistics in opposition to "humanistic" (stylistics, poetics, literary criticism, etc.) and "sociological" linguistics, (pragmatics, discourse analysis, sociolinguistics, marxist speculation on language, anthropological linguistics , etc.). According to Newmeyer (41), "autonomous" linguistics corresponds to the point of view of the natural sciences (?) and is concerned with "those of its properties that exist apart from either the beliefs and values of the individual speakers of the language, or the nature of the society in which the language is spoken" (5-6). Newmeyer's "autonomous linguistics" encompasses the core of traditional linguistics, i.e., grammatical theory, phonology, morphology and syntax. On the other hand,

in historical-sociological terms, Newmeyer's category supposedly includes the European comparativists of the nineteenth century (21), most of European structural linguistics, Post-Bloomfieldian structuralism (32-33) and, of course, Chomskyan linguistics (71).

Needless to say, to exclude just beliefs, values and social aspects, and not, say, physical, biological or mathematical ones, from the purview of so-called "autonomous linguistics", and to make this coincide with the domains of phonology, syntax and grammatical theory is to beg all the key questions. Similarly, to call Chomsky's explicitly psycho-biological approach "autonomous" is quite confusing, to identify "autonomous linguistics", as above defined, with the point of view of the natural sciences is gratuitous, and to ignore the considerable differences existing between the comparativists, the structuralists and the Chomkyans in these issues is inaccurate at best. For all these reasons, I think it is preferable to ignore Newmeyer at this point and turn to the analysis of our last parameter at this level, B5, concerning the *modus essendi* of language.

The ontological issue has been extensively debated during the last decade or so and remains unsettled (Katz 1981; Langendoen and Postal 1984; Gazdar et al. 1985; Chomsky 1986; Soames 1984; 1991; Devitt and Sterelny 1987; Carr 1990; Katz and Postal 1991; etc.). By far the best and funniest recent study of ontological issues specific to linguistics is Botha (1992), who identifies no less than seven different types of ontological conceptions of language "on the market", including material, behavioural, mental, biological, abstract, social and cultural ones. Of course, detailed discussion is impossible here, so I gladly refer the interested reader to Botha's book, with which I certainly cannot compete. I'll just emphasize two points, very briefly: a) that, according to Botha, at the ontological level, only two options remain open nowadays, i.e., either language is an abstract object, or it is a psycho-biological (ultimately, "material") one, "social" and "cultural" not being independent ontological categories; and b) that whatever option is taken at this point has substantial consequences elsewhere, as, in part, we saw above, and will show in more detail in our discussion of the parameters in section C.

3C. *Grammar: Architecture, Design and Implementation Issues*

As was to be expected, the area in which diversity is greater is that of architectural, design and implementation issues. As shown in Chart 1, literally dozens of distinctions have been proposed to handle it. In fact, I have been at

great pains to find a more or less appropriate label for each of them. Needless to say, the topic is a vast and complex one, it has been only very fragmentarily investigated so far and detailed discussion and attribution of values is out of the question here, so I'll gloss over the distinctions in Chart 1 very briefly pointing out those that seem to me redundant or otherwise misconceived and I'll wind up in section 4 with a chart presentation of those that I currently consider more promising from the metatheoretical point of view.

C1-C7 overlap to some extent, although the point of view is different in each, so an effort must be made to sharpen the relevant distinctions. C1 focuses on the grammar's coverage, particularly on the "division of labor" between the grammar and other systems involved in linguistic behaviour. The two extremes are "broad", "imperialistic" grammars (Generative Semantics, Cognitive Grammar, Text Grammar, Systemic Functional Grammar, Tagmemics) vs. narrowly defined ones (P&PT, GPSG, RG, etc.). C2 refers to the type of statement used in grammar construction, i.e., "declarative" (Equational Grammar, Co-Representational Grammar, Montague Grammar, GPSG), or "instructional" (traditional TGG, at least in some interpretations). C3 focusses on the function the grammar performs generating, parsing or just "checking" or "licensing" structures. C4 on the number of representational strata postulated, several, as in traditional TGG, RG, FG and modern P&PT, or one, as in Tagmemics, GPSG, Autolexical Syntax, etc. C5 turns on whether there are feeding relations among representational strata (TGG, RG, P&PT...) or not (Autolexical Syntax), and within the former between those that imply "destructive" interactions (TGG, P&PT, RG) and those that contain only "incremental", or "monotonic" ones (Montague Grammar, LFG). C6 refers to the presence/absence of directionality in the control of the procedural component, i.e., there are speaker, or hearer-oriented grammatical descriptions, and neutral ones. Finally, C7 refers to the values of the directionality parameter, i.e., the directions themselves, basically top-down (traditional TGG, SFG, GPSG, LFG) vs. bottom-up (CG, HPSG, WG, P&PT).

Early generative grammars are most naturally interpreted as competence, procedural, structure-generating, polistratal, derivational, directional, top-down grammars, in spite of Chomsky's frequent reminders that the grammar was merely an abstract characterization of competence and was neutral between speaker and hearer. Nowadays, most varieties of generative grammar (excluding SFG and FG) claim to be competence grammars, call themselves "declarative" and mostly consist of "well-formedness conditions"

whose function it is to "license" structures produced by some (usually unspecified) extragrammatical generator. Nevertheless, to the extent that many of them (RG, P&PT, LFG, GPSG, FG, etc.) remain polistratal (C4) and require their representations to be ordered (e.g., RG, P&PT, GPSG, FG), their declarativeness is questionable and their claims to being non-derivational and non-directional must also be taken "with a pinch of salt". In fact, the most natural interpretation of contemporary P&PT, CG and HPSG, at least, is as directional bottom-up grammars.

C8, concerning which of the traditional components of the grammar generates its basic representations, is a venerable and still quite popular taxonomic criterion (Halliday 1985, Harlow and Vincent 1988, Droste and Joseph 1991, Lightfoot 1992), but in my view useless at the metatheoretical level, since most contemporary grammars are modular and no longer contain identifiable "components" bearing the names "syntax", "semantics", etc. Those labels, of course, continue to be used informally, but, what twenty years ago we would have called "syntax" or "semantics" is now distributed all over the grammar. If anything, in several modern frameworks (Montague Grammar, GPSG, HPSG, LFG, P&PT, CG) most of it is in the lexicon, and it is quite proper to say that many of them are "lexically driven".

Given the present indeterminacy of such traditional components as syntax and semantics, the opposition between isomorphic and non-isomorphic grammars mentioned in C9 is difficult to establish, although some frameworks (e.g. Montague Grammar, CG, GPSG) still programmatically endorse the rule-to-rule correspondence hypothesis of R. Montague and E. Bach, whereas others reject it or just keep quiet about it.

C10 is absolutely everybody's favourite distinction when it comes to establishing a taxonomy of modern grammars. As McCloskey puts it, modularity "is the distinguishing mark of syntax in the eighties" (McCloskey 1988, 56). If that were the case, there would be no point in positing C10 as a metatheoretical parameter, but as I showed elsewhere (Escribano 1992a), "modularity" is itself an ambiguous notion that everybody seems to have taken for granted without considering what it takes for a grammar to be "modular". In certain cases the term is trivially employed in ways that make it completely useless as a metatheoretical criterion. E. K. Brown (1991, 490) for example, extends that qualification even to the earliest versions of TGG and, of course, does not say which grammars are not modular in that sense, whatever it is. Equally misguided, in my view, is Harlow and Vincent's characterization, according to which "module" and "component" or "analytic

level" are more or less synonyms. In the authors' own words, in modular grammars,

> the complex of linguistic behaviour that we observe is the consequence of the interaction of a number of (semi)independent systems (e.g., phonology, morphology, syntax, semantics, pragmatics), each of which is characterized by its own primitives and operations. (Harlow and Vincent 1988, 8)

Newmeyer (1983), Pesetsky (1992), McCloskey (1988), and Lightfoot (1992) all consider modularity to be the distinguishing property of generative, as against other types of grammars, but all of them take the term for granted. Sadock (1991), on the contrary, also a generative grammarian, declares that only his own brand of generative grammar, Autolexical Syntax, is genuinely modular. According to Sadock (Sadock 1991, 5-10, 12), who takes Fodor (1983) literally, modularity implies "autonomy" and "informational encapsulation", and to the extent that Chomskyan grammars, like earlier structural ones, have always implied derivations and feeding relations among representations, these can hardly be encapsulated and autonomous. Thus, according to Sadock's criterion, by definition, no directional grammar can be modular.

Surely, there is a serious misunderstanding in the field in this respect, and what surprised me most when I wrote (1992a) was that it should have remained undetected for so long. Obviously, Harlow and Vincent's view of the traditional components or levels of analysis as "modules" is completely misleading. Under that interpretation, all grammars ever have been "modular", since nobody seems to have seriously considered producing a "holistic" description (i.e., one based on just a single type of theoretical entity). Any grammar distinguishing several classes of units and the corresponding sets of rules would automatically be "modular", by Harlow and Vincent's criterion. That's clearly no good. Much the same can be said of E. K. Brown's interpretation. On the other extreme, by Sadock's criterion, grammars such as P&PT, GPSG, FG, which are generally recognized as "modular", would have to be excluded, which seems pointless. Is this, then, a purely terminological question?

I don't think so, but the notion of "modularity" requires clarification if it is to be used as a metatheoretical criterion. In Escribano (1992a), I put forward a condition which seems to me crucial to make "modular" a non-trivial predicate, i.e., a grammar is modular if and only if it features a many-

to-many relationship between its systems (principles, inventories) and its distinguished levels of representation. In other words, no system of principles or type of entities may exhaust a representation, and viceversa, no representation may be exhaustively reduced to one system or type of entities. That makes P&PT, GPSG, HPSG and Dik's FG "modular", whereas earlier versions of TGG and most structural and functional grammars, Hudson's WG, CG, etc., are not modular, but "holistic", in this technical sense (see Escribano 1992a for details).

Halliday's distinction between "syntagmatic" and "paradigmatic" grammars (C11) and Droste and Joseph's on "pattern-oriented" vs. "word-oriented" ones in C12 seem to overlap, although Halliday's is meant as a general characterization of paradigms, approximately coextensive, as he says, with the opposition between formal and functional paradigms discussed above. Taken in their literal meanings, though, both distinctions seem to me redundant and, in fact, inapplicable. On Droste and Joseph's, I have justified this view elsewhere (see Escribano 1992b) and will not repeat my criticisms here. As regards Halliday's characterization of generative grammars as formal-syntagmatic, it is completely ill-founded, since it is based on features that perhaps were relevant of the earliest versions of TGG but have long disappeared. Structures are exactly as "derived" in Chomsky's current principle-based framework as they may be in the most careful formulations of SFG, so it is plainly inaccurate to say that one is syntagmatic and the other paradigmatic. This reasoning, of course, extends to all other principle-based grammars, although some are more repertory-based than others, as we'll see immediately.

Indeed, C13 has something to it, since it is intuitively correct that certain grammars are based on the precomputation of large repertories of lexical units (Montague Grammar, GPSG, HPSG, LFG, WG), tree structures (Joshi and Kroch's TAG's), construction-types (Tagmemics), etc., whereas others tend to have much smaller inventories and assemble the rest of the configurations needed by means of algorithmic devices (interacting principles, recursive rules, etc.). However, this distinction is obviously one of degree, since no grammar is purely repertory-based nor purely algorithmic (at least insofar as all contain a lexicon). GPSG is a good test case: on the one hand, it contains a large repertory of ID rules (expressing subcategorization types) which is unnecessary in P&PT or CG granted the existence of a lexicon and something like the Projection Principle, but on the other, the meta-rules that expand the grammar, and the various feature-

passing principles invoked (HFC, FFP, CAP) are typical algorithmic devices. What seems to be to a certain extent relevant at this point, therefore, is just how much pre-computed information is assumed in each framework.

C14 is frequently invoked, but useless, in my view, since it rests on a systematic ambiguity concerning what is understood by "grammar". The categorial component of P&PT, LFG or GPSG may be CF, but, as Sadock has pointed out (Sadock 1991, 43-47), from the context-freeness of each of the modules, it doesn't follow that the resulting grammar remains CF, and nobody has cared to investigate in detail the effect of the interactions of the various devices employed within each model. According to Shieber, from the point of view of weak generative capacity, all contemporary models seem to be more or less "in the same boat", i.e., somewhere in between CS and recursive.

C15, on the scope of the grammar, is a significant distinction, again, provided we agree on what the term "grammar" implies. Under the usual restrictive interpretation, it means "sentence grammar", and the opposition between Tagmemics, SFG and R&RG, for example, on the one side, and Chomskyan grammars, RG, or LFG, on the other, is quite obvious. However, Williams, Sag, and many other Chomskyans admit the existence of a separate block of rules (covering anaphora, ellipsis phenomena, etc.) under the name "discourse grammar". Thus, in this sense, Chomskyan grammars have both a sentential component and a discourse one, and the superficial difference between those two groups of paradigms vanishes. The distinction, therefore, ceases to be operative in this case. Nevertheless, C15 should probably be maintained, but only to distinguish genuinely "holistic" grammars like Pike's Tagmemics, Halliday's SFG or Hjelmslev's Glossematics from all others. The key criterion is whether discourse level structures are handled by the same theoretical devices as sentence level ones, and this happens in Tagmemics or Glossematics, but not in Chomskyan grammars, for example.

Of course, the nature of the primitives of formal theories is an important aspect of their design, and therefore Halliday's distinction in C16 is relevant in principle. However, to my knowledge, all extant grammatical frameworks use the same kind of primitives, i.e., predicates, under several guises (distinctive features, attribute-value pairs, relational predicates). Units, of course, appear in most of them, but as derived entities ultimately analyzable as complexes of [A, v] pairs. Therefore, what is distinctive is the selection

of primitive predicates within each framework (i.e., what Shieber 1986 calls its "informational domain") but not the opposition units / relations in itself.

From this perspective, C17 is a very secondary distinction at best. In fact, although it is true that some frameworks depend more heavily on constituency (part-whole) relationships (e.g., Post-Bloomfieldian grammars, Tagmemics and traditional TGG), or conversely, on dependency (part-part) relationships (e.g., Glossematics, Stratificational Grammar, Relational Grammar, Dependency Syntax, Word Grammar) than others, no grammar depends exclusively on either. Present-day P&PT and GPSG are representative in this respect: constituency-based notions like "head", "projection" and "percolation" are surely very important in both, but so are "horizontal", dependency-based relations such as government, case-marking, theta-marking, antecedency, agreement, etc. Relational Grammar would seem to be about the best current example of a pure dependency grammar, but this is somewhat artificial, since RG has so far practically concentrated on the analysis at clause-level ignoring hierarchically lower ones. So, on the whole, Matthews's distinction is at best a matter of degree and not a very useful classificatory parameter.

C18, on the primitive/derived status of syntactic relations, has been widely invoked as a key difference between RG, LFG, Tagmemics and FG, on the one hand, and configurational grammars (e.g., GPSG, CG, P&PT), on the other, so it must be granted metatheoretical status, but, again, only as a comparatively low-level parameter.

Similarly, C19 is a very secondary "local" parameter, useful only in distinguishing certain subclasses of categorial grammars (i.e., those lacking/ containing categories augmented with the directional combinators / and \, respectively).

The remaining distinctions, C20-26, refer to the types of rules or statements used in different grammars. C20 is meant to distinguish principle-based grammars from rule-based ones, e.g., modern versions of Chomskyan P&PT from earlier ones, and, within the former group, C21 captures the opposition between grammars based on parameters and those based on fixed universal principles. However, the difference between "rules" and "principles" is increasingly difficult to tell. In many cases the two terms seem to be used interchangeably. For example, X-bar and Move Alpha are indistinctly referred to sometimes as "rules" and sometimes as "principles". On the other hand, a parameterized principle is nothing but a special type of context-sensitive rule, so, in my view, the distinctions introduced by C20-

21 may well be dropped from our metatheory. C22, in turn, distinguishes "rigid" grammars from "fuzzy" ones (i.e., Labovian, or Hallidayan ones, for example), but this is already implied in each linguist's conception of his object of study (see B4 above). The grammar cannot but be rigid/fuzzy, as the case may be, if the object it characterizes is. Therefore, C22 should be dropped, as well.

The other distinctions in Chart 1 refer to the specific properties of the rules used in grammars. Thus, C23 is formulated with Categorial Grammar in mind, and cannot easily be generalized. Traditional rewrite rules and modern transformations (excluding head-to-head movement) preserve the integrity of phrasal categories. The issue is whether the grammar should also contain rules allowed to search down for the heads of constituents. That's what head-wrapping operations do, and it represents a considerable increase in power. However, the distinction invoked here may not be metatheoretically fruitful. The reason is that although head-wrapping rules as such exist only in CG, as far as I know, all modern grammars contain alternative devices whose contribution amounts to much the same thing. "Head to head movement" and "percolation" are cases in point. Thus, C23 probably should disappear, too.

C24 is supposed to distinguish P&PT from RG, and these two from LFG, and C25 in its turn would distinguish P&PT from all non-transformational grammars. However, it is not clear to me whether the real import of the distinction between Chomskyan transformations, Perlmutter's revaluations and Bresnan's lexical rules is other than terminological. At least, the objective differences that seem to exist, as far as I know, all follow from other parameters (e.g., mono/polistratality), so C24 may not be independently relevant. On the other hand, the idea of "movement" is just a convenient metaphor. There are notational variants of Move Alpha which do not imply "movement" and which seem to be just free implementational alternatives to such feature-passing devices as "slash", or "unification". Therefore, I am inclined to believe that nothing very substantial hinges on features like those invoked in C24-25 and would accordingly dispense with such parameters, too.

Finally, C26 is supposed to refer to a crucial difference between GPSG, LFG, HPSG and CG, on the one hand, and Chomskyan P&PT on the other. In the first case, all rules would have "local" scope, whereas in Chomsky's grammar some rules or principles would apply "globally". That distinction was motivated in earlier stages of generative grammar (e.g., during the

linguistic wars between "semanticists" and "interpretivists" in the early 1970's), but, again, nowadays it has lost most of its point. Even if GPSG rules are strictly "local", the overall effect of feature transmission devices like the Head Feature Convention or the Foot Feature Principle is "global". On the other hand, in modern versions of Chomskyan grammar based on cyclicity, subjacency or minimality, it is no longer possible to pin down an obviously "global" rule. Therefore, this too may well be a spurious parameter.

That concludes my necessarily schematic survey and discussion of the most important metatheoretical distinctions that have been proposed, as far as I know, in the literature during the last decade or so.

4. Conclusions

Needless to say, conclusions at this stage are still very modest. Much more research is needed before we can say we understand all the ins and outs of the various theoretical alternatives, and the extra knowledge thereby obtained might well suggest a different, perhaps higher, level of abstraction as the proper environment to carry out systematic comparisons. However, the metatheoretical parameters that seem to me more fruitful at present are those that appear in Chart 2.

Chart 2 leaves an important unanalyzed residue, i.e., no attempt has been made to capture possible parametric dimensions within the primitive core of what Shieber (1986) calls the "informational domain" of grammatical theories. Our analysis in this respect (see section 3) has yielded rather negative results, i.e., it has shown the inadequacy of well-known broad distinctions such as unit- vs. relation-based, constituency- vs. dependency-based, transformational vs. non-transformational, global vs. local, etc., but I have deliberately refrained from establishing parameters to classify the inventories of primitive terms, relations and operations the different grammars depend on. Broadly speaking, the crucial informational distinctions (person, number, gender, tense, case, etc.) tend to be indispensable (short of merely notational variation), and the same applies to really primitive relations like "is a", "co-occurs with", "precedes", "dominates", "governs", and a few others. However, further research in the ontology of theoretical entities may uncover significant dimensions of contrast even in this domain. With that caveat, the three levels of analysis and the general approach taken in Escribano (1989; 1992a; 1992b) and in this paper seem to me fertile and

CHART 2: PARAMETERS FOR A METATHEORY OF GRAMMARS
SECOND THOUGHTS

A. *Philosophical and Epistemological Issues*

1. Ontological status of theoretical entities: Realism / Instrumentalism
   1.1. Realism: Platonic / Aristotelian / Popperian
2. Theory-data interaction: Descriptivist / Constructivist + (bound option)
   Data-selection: Theory-determined / Externally determined
   2.1. Externally determined: Physicalist / Phenomenologist
3. Scientific objectives: Description / Explanation / Simulation
   3.1. Description: Formal / Informal
   3.2. Explanation: Formal-Nomothetic / Material-aetiological
4. Evaluation criteria: General / General + Specific
   4.1. General: (Explicitness) (Consistency) (Generality)
   (Simplicity) (Compatibility) (Veracity)
   4.2. Specific: (Learnability) (Parsability) (Implementability) . . .

B. *Conception of the Object of Study*

1. Epistemic order: ± Representational
2. Scope: ± Universal
3. Shape: Individual / Set
4. Type: Thing / Event / Process,...
5. Restriction: Geographic / Political / Social / Thematic / Stylistic / Methodological, ...
6. *Modus essendi* : Psychobiological / Abstract

C. *Architecture, Design and Implementation Issues*

1. Scope: Competence / ± Performance
2. Mode: Declarative / Procedural
3. Operation: Structure-producing / Structure-checking
4. Descriptive strategy: Monostratal / Polistratal
5. Inter-level relationship: Co-representational / Derivational
   5.1. Derivational relationship: Incremental / Replacive
6. Control: Directional / Non-directional
7. Orientation: Top-down / Bottom-up
8. Architecture: Modular (many-to-many) / Non-modular
9. Range: Sentential / Textual (Holistic)
10. Pre-computed information: More>Less
11. Status of syntactic relations: Primitive / Derived
12. Category-complexity: [A, V] ± [/ , \]

delicate enough to expose the real causes of tension and discrepancy between current theories of grammar in an objective and realistic way.

The attentive reader will surely have noticed that the analytic method employed here is essentially the same used in classical phonology for the analysis of phonological systems: First, we analyze all the observed oppositions at those three levels. Then, we try to motivate just the minimum number of "distinctive features" that would suffice to capture them, and formulate them as parameters, each of them with two (+/-) or, occasionally, more values, supplying them with appropriately perspicuous labels. Finally, we attempt to characterize the grammatical approaches under consideration in metatheoretical terms as clusters of values for subsets of those parameters so that the essentially multidimensional oppositions separating grammatical theories can be elegantly expressed. Indeed, the parallelism with the method of classical phonology may be pursued even further: thus, there are approaches left unspecified for particular parameters corresponding to aspects left vague by their proponents, or to regions of the field only partly researched so far, and there are cases of "neutralization" of certain oppositions in particular contexts.

I am aware of the fact that, ideally, the analysis presented here should have been supported by an exhaustive reduction of the field in terms of the proposed parameters, but that would have entailed massive quotation and very fine-tuned justification which, obviously, required well beyond the space available here. Therefore, I had to content myself with partial, suggestive (and mostly unsupported) attributions of values to approaches. Hopefully, many of them will be uncontroversial to those familiar with the field, but, nevertheless, a book-length study showing how these categories apply to the various theoretical approaches competing in the field (Escribano forthcoming) is currently in preparation and will, I hope, be published soon. Pending the exhaustive re-checking still in progress, my conclusions must be considered tentative. However, for those parts of the field which have been fully checked, the analytical residues are fairly small and the categories chosen above seem to express the crucial tensions in a perspicuous way. To that extent, I hope that even the cursory inspection here undertaken will yield some insight into the structure of the otherwise rather bafflingly diverse field of contemporary theoretical linguistics.

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