

On noun phrase structure in Functional Grammar: Some conceptual issues

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1. Introduction: aim and scope of this paper¹

The F(D)G account of noun phrase (NP) senses rests crucially on the concepts of *variable*, *operator*, and *scope*. Yet, the status of F(D)G variables and operators is problematic, and extant F(D)G pronouncements on their scope as well as that of other constituents of the underlying structure of NPs are contradictory. To a great extent this is because the F(D)G theory of construction is vague. Correspondingly, surface NP syntax largely rests on *domain integrity* (DI), *head proximity* (HP), and *scope*, which, in the FG version, yield only weak predictions.

The purpose of this paper is to draw attention to such shortcomings, diagnose their causes, relate them to wider issues of ontology, semantics, and syntactic theory, and extract the relevant consequences in the area of NP structure and interpretation. Although inevitably critical, the paper is meant as a sympathetic outsider's contribution to the FG programme, and its ultimate aim is therapeutic. Hence, along with careful critical discussion of term structures and their surface consequences, some suggestions will be offered that might yield a more coherent and elegant account without abandoning any of the constitutive assumptions of FG.

It must be emphasized, though, that the article remains basically critical-therapeutic: it is just a well-meaning outsider's attempt to induce FG scholars to clarify certain aspects of their account of NPs. It is not an alternative full FG theory of NPs (e.g., no attempt is made to decide which operators, core types, or types of satellites are appropriate, etc.), nor a plea for any competing version of FG. The suggestions that accompany critical discussion below are not incompatible with any deep FG principle, but they do diverge from current FG doctrine and, to the extent that they amount to a significant strengthening of its key principles, make the theory more vulnerable to empirical refutation. On methodological grounds, this is as it should be, and, with respect to Indo-European languages like English, the empirical consequences are favourable, but whether such a strengthen-

ing is desirable in view of the facts FG has collected about Language in general is a different matter well beyond the scope of this article or its author's competence. The criticisms and suggestions below, in sum, are offered in good faith and for what they may be worth. It remains for FG scholars themselves to decide whether building an alternative account of terms/NPs along such lines is wise and suits their overall research strategy or not.

2. Variables

Current FG term representations have become very heterogeneous as to the nature of the information they represent (which results from merging different analytical levels), and they are packed with variables whose import is not always clear, cf. Escribano (2004b). Of course, some are straightforward: for example, every semanticist will understand why the representation of a quantified NP contains a variable, and, to the extent that any NP can pick out different extralinguistic entities, why it carries a variable ranging over possible referents, although whether sense and reference should be represented in a single formula is, of course, a moot point (cf. Keizer this volume). Yet, what some other FG variables are doing at the representational level is more difficult to understand. For example, why should all lexical predicates carry variables? Apparently, the idea of endowing lexical predicates with variables first appeared in Dik (1989: 50) and was developed in Keizer (1991, 1992) and Hengeveld (1992a), cf. Dik (1997a: 55 footnote). However, although considering every predicate as the specific value of a predicate variable may be appropriate in a representation of the speech act as a set of action choices available to the speaker, such variables can hardly play any role in a formula that intends to capture the *sense* of an NP, where lexical items, by definition, must have replaced such variables. The same applies to the variables that lexical items receive in current FG for a different reason, i.e., *qua* categories with semantic import. As Hengeveld and Mackenzie (2006: 673) put it, "To the extent that ontological categories are reflected in the language system, they have the status of semantic categories, each of which is provided with its own variable".² Hence, every lexical item (and every NP, etc.) is entitled to carry both a Dikkian variable and one of Hengeveld and Mackenzie's, but, of course, those two among many others, since there are many other analytical perspectives (e.g., to the extent that it consists of a phonological form, it might carry a phonological variable; if it plays a discourse role, it may be en-

dowed with a discourse-functional variable; to the degree that it stands for an entity of the psychological world, it may carry a Psych-W variable, etc.). However, it would be absurd to try to integrate all those heterogeneous variables along with predicates, arguments, and *bona fide* variables³ (if any are needed) into, say, a representation of the sense of terms as reflected at the representational level so the basic point is: which variables, if any, do play a role in the computation of the NP's sense and its intrinsic referential potential?⁴

It seems that this essential question has been somewhat lost sight of in recent FG work, and the result is very heterogeneous "semantic" representations in which most variables are doing no work at all but, on the contrary, creating confusion in the formalism and its interpretation. It may be necessary, therefore,

- a. to recall the nature and role of variables in general
- b. to clarify the variables that figure in current FG representations, and
- c. to distinguish those that are appropriate to sense representations from those that belong to other analytical levels.

Since this article is meant as a small contribution towards a better FG account of NP semantics and syntax, only variables involved in the computation of the sense of terms will be discussed here in some detail, but the issues that arise apply to FG variables in general, and the Ockhamian reasoning deployed here has multiple consequences at clause and other levels which will be tacitly invoked but not discussed in depth.

2.1. Why variables?

To place the issues in perspective, it is convenient to start by asking whether, and why, term representations should contain variables at all. For natural language semanticists, the source of variables is the standard logical treatment of elementary quantification, in which first-order variables are introduced, bound by quantifiers, to replace awkward conjunctive statements with appropriate generalizations. In predicate logic formulae like $(\forall x) [P_1(x) \dots \& P_n(x)] \supset Q(x)$, for example, the (presumed) "individual" variable "x" is bound by the universal quantifier \forall and saturates the open position of the first-order predicates $P_1 \dots P_n$, and Q . That argument position is supposed to be exactly the same as is alternatively satisfied by the referent of an ordinary name like *Joe* in *Joe smokes*, represented by the

constant “j” in a formula like $S(j)$. Hence, the variable “x” and the constant “j”, of course, never co-occur.

Yet, on a little reflection, “x” is very different from “j” in its content, for whereas “j” stands for a fully specified individual whose properties are determined partly by linguistic convention (in English, *Joe* is the name of a male, etc.) and partly through use in an appropriate discourse context, “x” stands only for a strange entity whose ontological status metaphysicians have been discussing, inconclusively, for over two thousand years, and which suffices to saturate a predicate only to the extent that it is “bound” by a quantifier.

Predicate Logic quantifiers, in their turn, are analyzed, after Frege, as second-order predicates that apply to structures like $P(x)$ and express existential statements like “ $P(x)$ holds for at least one x” or “ $P(x)$ holds for all x”.⁵ Under such an analysis, whereas a sentence like *Joe passed* contains two semantically relevant constituents coextensive with the linguist’s canonical NP and VP, one like *Some students passed* has no semantic constituent to match the NP *some students*. This consequence, which has always seemed counterintuitive to grammarians, is justified to the extent that it accounts for the strange properties of *some students* under negation, i.e., the puzzling non-contradictoriness of *Some students passed and some students did not pass* or *All students did not pass, but some did*. An ordinary proper name like *Joe*, on the contrary, being well-behaved under negation, cf. **Joe passed and (Joe) did not pass*, can be directly represented as an individual constant “j” in formulae like $P(j)$ and $\neg P(j)$, and entails no variable at the representational level.

Admittedly, most NPs are not like proper names. When terms are expressed by phrases with a common noun as head and perhaps one or more modifiers, as in *the tall dark girl in jeans who received us*, if the right truth conditions and entailments are to follow, it is necessary to lay bare the intensional contribution of their various constituents. In the simplest cases, such phrases are constituted by monadic predicates (*girl, tall, dark, in jeans, who received us*, etc.)⁶ and, of course, predicates need subjects, so the question arises what the subject of such predicates is, an issue that entails determining what is a possible subject or predicate, i.e., choosing an ontology⁷ and struggling with rather complex metaphysical problems, cf. Strawson (1959).

Playing on the safe side metaphysics-wise, early philosophical semantics assumed an austere ontology containing only individuals, according to which only extensional individuals could be subjects and the only predicates were first-order predicates. Russell’s (1905) theory of definite de-

scriptions, for example, would represent the meaning of lexically complex NPs like *the tall dark girl in jeans who received us* as a conjunction of first-order predications upon a “subject” provided by the “individual” variable “x” as bound by an appropriate quantifier (“iota”). At this point, then, a variable does intervene in the logical form of definite NPs. Obviously, “x” was a rather meagre subject, especially by empiricist standards, but convenient in that it left the smallest possible unanalysed residue and allowed for a straightforward representation of the truth conditions introduced by the internal constituents of complex NPs.

Yet, the ontological status of the “individual” variable “x” was, and remains, far from clear. Under some metaphysical interpretations, e.g., Locke’s ([1690]1997: 268-276) or Wittgenstein’s (1922: 2.021, 2.024, 2.0131), if *the tall dark girl in jeans who received us* is represented by a formula including the string (...x) *Girl(x) & Dark(x) & Tall(x)*..., the variable “x” would have to stand for a kind of “substance without properties”, and is problematic by empiricist standards on precisely those grounds. On the other hand, interpreting “x” as an Aristotelian “essence” (a cluster of necessarily concurrent properties of an entity) is unwarranted, since in such a formula the “essential” property *Girl(x)* is one of the predicates of “x”, hence cannot be a constituent of “x”. The only remaining alternative, apparently (cf. Loux 2002: 96-138), is to interpret “x” according to the “bundle” theory of individuals. Yet, under a “bundle” theory, “x” must stand either for the bundle or for a selected subset of the bundle, and, in either case, insurmountable logical problems arise: If “x” stands for the bundle in ...*P(x) & Q(x)* etc., where properties like P and Q are themselves constituents of the bundle, *P(x)*, *Q(x)* etc., become tautological or downright inconsistent; if, on the contrary, “x” stands for a subset of the bundle, each of *P(x)*, *Q(x)* etc., may well be informative, but, then, by definition, the content of “x” in the various clauses *P(x)*, *Q(x)*, etc. that are supposed to characterize an individual cannot be unified, and the bundle-theoretic definition of individual identity disintegrates. To put it simply, P, Q, etc., are in that case being predicated of different particulars and the formula is no longer a representation of any consistent bundle.

The status of such “individual” variables, in sum, is problematic under any of the major metaphysical theories of individuals on the market, but one thing seems clear: if we grant, for the sake of argument, that “x” picks out “under-specified individuals” (in some sense yet to be clarified), “under-specified” (*pace* substratum theorists) cannot be synonymous with property-less, for “x” still represents only individuals within an ontology that has long been far richer. Hence, “x”, whatever it turns out to be, must

minimally consist of the ontological properties distinguishing individuals from other entities (events, properties, etc.), except existence, since that is what an existential quantifier contributes to such formulae.

Although, somewhat surprisingly, the uncertain status of “x” was not perceived as a flaw (to my knowledge), that early approach to the logical form of NPs soon proved to have other important limitations. For example, modifiers do not always receive intersective interpretations (e.g., *a good driver* is not an individual who is both good and a driver). Various attempts were made in the literature to handle such cases without abandoning an austere first-order ontology. A popular one consisted in increasing the adicity of *good* to introduce a hidden argument, i.e., saying that *good* really means “good for a driver” or “good as drivers go”, etc., but, obviously, that patch would not work for cases like *a criminal lawyer* or modal NPs like *a potential customer*, and eventually semanticists had to assume higher-typed predicates, subjects that were themselves properties, and a much richer ontology. Correspondingly, it became technically possible to quantify over properties (cf. natural language expressions like *Titanium has all the good properties that steel has*, *He has a virtue I envy*, etc.) via formulae like $\forall P [P(i) \supset P(j)]$, (where P = a property and i, j individuals), and property variables became appropriate in such cases of quantification.

Otherwise, the usual computational machinery suffices: second-order predicates combine with first-order predicate subjects in exactly the same way first-order predicates like *pass* combine with first-order arguments like *Joe* to yield *Joe passed*, i.e., both cases reduce to *functional application/composition*.⁸ Thus, *good driver*, itself a first-order predicate (type $\langle e, t \rangle$), is the result of a second-order predicate *good* (type $\langle \langle e, t \rangle, \langle e, t \rangle \rangle$) being functionally composed with a first-order predicate *driver* (type $\langle e, t \rangle$) (cf. Partee 1995, Heim and Kratzer 1998: 68-73). Informally, predicates like *good* just need “subjects” like *driver* in such cases, and the resulting predicate *good driver* will in its turn saturate an appropriate determiner of type $\langle \langle e, t \rangle e \rangle$ to yield a name of type e (= individual), but neither the individual variable “x” nor any other variables play any role in the computation of such senses. In other words, higher-order properties and their “subjects” must be represented, but nothing else need be.

A linguist like Dik (1997a: 133-136, 149-150) might surely object that such logical representations of complex NPs, even if they capture their bare truth-conditional aspects, are just too poorly structured to do justice to the way semantic composition operates to yield senses in natural language. For example, there is plenty of empirical evidence (e.g., from standard constituency tests like *One-anaphora*, *Ellipsis*, *Coordination*, etc.) that, in

cases of multiple modification, the modifiers of natural languages are not just added one after another, as suggested by the “flat” conjunctive formulae of predicate logic, but stacked, or even attached to different hierarchically organized invisible heads within the NP (cf. Cinque 1994 and work in Cinque ed. 2002). Thus, the four modifiers in *a beautiful tall dark Indian girl* modify not the head noun, but different expansions thereof, i.e., minimally (more structure may be involved), if the lowest modifier modifies the noun *girl*, the next higher one modifies *Indian girl*, etc., and, depending on which modifiers and operators (functional categories, etc.) intervene, the higher heads may, but need not, be of the same semantic type. This is absolutely correct, and further types are surely involved, but that need not entail new variables in term representations.

The semantic ontology did not long remain austere limited to individuals and properties. Another early influential enrichment was Davidson’s (1967) introduction of “events”, elaborated on in Parsons (1990) and subsequent work, to capture entailments among predicates of different adicity sharing homonymous heads. Of course, predication and quantification over events⁹ did require a new event variable “e”¹⁰ and new semantic types for events, predicates of events, and events as modified in various ways, although Davidson’s flat conjunctive statements of event structure ignore the hierarchical organization of natural language VPs, where arguments and modifiers appear at different layers of structure and affect different heads and different types of entities.¹¹ Thus, constructing Logical Forms to mirror the way expressions are compositionally built in natural languages surely entails allowing for recursion of entities within entities, properties within properties, events within events,¹² etc., and accepting a rich type system, a fact acknowledged in all major linguistic approaches, including FG,¹³ but representing stacking and recursion does not require any new variables in the internal structure of sense representations, which, as argued below, reduces to functional application/composition of *n*th-order predicates and their arguments.

In sum: as semanticists have attempted to capture more subtle aspects of the sense of NPs in natural languages, the ontology has expanded to include properties/sets, events, etc., with their own properties, recursion, etc., but variables remain necessary to compute senses only to the extent quantification is involved, if at all, and play no role otherwise. The rule of thumb is just this: symbols can only be compositionally computed if they stand for entities belonging to homogeneous ontological domains (sounds, inscriptions, actions, speech acts, senses, etc.), and, by definition, if the value of a variable symbol is computed in a formula, the variable cannot be so

computed, which makes it redundant at whatever analytical level is involved.

2.2. Variables in FG term structures

The original term formulae in Dik (1978: 16, 57) and subsequent FG work down to Dik (1997a: 160 footnote 1) are obviously inspired by the standard Fregean-Russellian treatment of quantification, i.e., an FG term is a structure $\Omega x: \Pi(x): \Psi(x): \dots \Phi(x)$, where “x” is “operated on” (cf. *infra*) by a set of grammatical operators Ω and “restricted” by a set of monadic first-order predicates $\Pi: \Psi: \dots \Phi$.¹⁴ Although FG terms do not quite have the properties of predicate logic ones (cf. Dik 1989: 117-118), term variables raise the same issues discussed above with reference to the status of “individual” (and event) variables, as well as other FG-specific problems that have remained unsolved, if not unnoticed.¹⁵ In this section, an attempt will be made to identify them and diagnose their causes.

First, the variable “x” introduced by Dik at the core of FG terms is problematic, and for the same reasons the logicians’ “individual” variables were. Dik (1989: 55, 115-118; 1997a: 136) presents it as a first-order variable picking out individuals, but such a statement immediately clashes with his other assumptions. Observe that if “x” were an individual variable, expressions denoting individuals should be freely substitutable for it, but, of course, that is incorrect (no expression denoting an individual may replace “x” in a term formula), as well as being incompatible with the FG view (e.g., Keizer 1991; Dik 1997a: 131; 1997b: 11) that only terms refer to individuals. The term-variable “x”, thus, cannot stand for an individual in the FG sense any more than it can pick out individuals in the logicians’ representations. Dik himself (1989: 126; 1997a: 148) observes that “it follows from the definition of ‘restrictor’ that individuals cannot be restricted”, so “x” cannot be restricted by the noun or its modifiers if it stands for an individual.¹⁶ Dik’s additional statement (Dik 1997a: 149) to the effect that in the case of terms structures, “entity” should be understood as “entity as so far defined by the term under construction” clearly shows that it is not “individuals” that restrictors restrict (*ergo*: “x” should not figure as saturating their open position). But if “x” does not stand for an individual, what do FG operators bind, and what saturates the open position of restrictors? Dik (1989: 122, 171 footnote 8; 1997a: 140, footnote 10) alternatively analyses individuals as singleton sets, but that does not rescue “x” as a self-standing theoretical entity: if “x” may stand for a one-

member set (intensionally: a property), then, type-wise, “x” is indistinguishable in such cases from the property variable “f”, which ranges over sets. One of them at least is otiose, or, rather, both are (cf. *infra*).

The problem does not seem to have been clearly perceived, but the early status of “x” must have seemed questionable to some FG scholars, for, since then, they have tended to deprive it of its original referential import, to add to the NP new discourse-functional, referential, or pragmatic variables (“big X”, cf. Rijkhoff 1992, 2002, “R” in Hengeveld’s recent work), and to keep “x”, but converting it into a “descriptive” variable, a “pragmatic” variable, or a strange hybrid of both. Rijkhoff (2002: 227-229; 2004; this volume) and other recent F(D)G writings distinguish within their underlying NP structures the “descriptive” layers (three, in Rijkhoff 1992, 2002 i.e., Quality, Quantity, and Location, four in Rijkhoff this volume, by implication more in Keizer 2004, Mackenzie 2004, and Hengeveld this volume, cf. *infra*) from a higher “discourse” layer and an “interpersonal” level in Hengeveld’s recent work (cf. also Butler this volume). Correspondingly, aside from the term variable “x”, referentially used NPs bear an additional variable, “big X” in Rijkhoff’s (2002) account of NPs in discourse, or “R” in Hengeveld’s characterization of the interpersonal level.¹⁷

Of course, something equivalent to a referential variable is still necessary if NPs are to be referentially usable, but not in the representations of the sense of NPs (cf. Escribano 2004b and Keizer this volume). Dik’s “x”, of course, figured in such representations because it was meant to serve a double (representational and referential) purpose, although for the reasons described it cannot play the former role well. If a new “X” variable accounts for the NP’s referential potential, then “x” must be independently justified, perhaps as a variable standing for linguistic entities (= senses). However, if “x” is treated as a sense variable, exactly the same issues arise as with the logicians’ and Dik’s “x”, now on the intensional level. And the same holds if “x” is used, perhaps elaborating on Dik’s idea that linguistic expressions bind variables, to capture the fact that the NP corresponds to the speaker’s utterance of a particular expression (instead of alternative ones) to refer to a certain referent (cf. Rijkhoff 2002: 229). Anyway, treating “x” as simultaneously a sense variable and an NP-choice variable is simply incoherent. In sum, Dik’s original term variable “x” was ambiguous and inconsistent, and to the extent it remains in formulae down to the latest F(D)G approaches to term structure (e.g. Hengeveld, this volume; Rijkhoff, this volume), the original inconsistencies remain unsolved.

An early proposal to sever “x” from its referential function is found in Rijkhoff’s (1992: 191) distinction between “term” and “referent” variables.

In the mature form of that proposal, cf. Rijkhoff (2002: 227-229), the term variable “x” subsists, but is no longer referential, a function now entrusted to “X”. Instead, “small x” fluctuates between two incompatible interpretations: in some passages (e.g., Rijkhoff 2002: 229) it ranges over the set of NPs available to the speaker as descriptions of the referent, whereas elsewhere (Rijkhoff 2002: 228) “x” is supposed to stand for some kind of “space” abstracted from the properties expressed within NPs via lexical or grammatical restrictors.¹⁸ However, the term variable “x” cannot be simultaneously a “sense” entity and an NP-choice variable, unless by “NP” is understood just the sense of an NP. On the contrary, what a speaker uses to refer to a referent is not the sense of an NP, but the NP *tout court*, i.e., a linguistic NP and its sense are categorially different things; if NPs *qua* NPs (i.e., linguistic expressions in their full individuality) are particulars used by speakers to refer to entities of the world of discourse (or their mental correlates), the entities in the range of “small x” cannot be senses, but entities from a different ontological domain.

However, even if “small x” is just a sense variable, we encounter exactly the same problems the logicians’ and Dik’s “x” posed: Rijkhoff’s “x” (and similarly, Hengeveld’s and Keizer’s) cannot literally stand either for the sense of an entire NP or for a zero-sense entity. It cannot pick out full NP senses, which obviously cannot replace “x” as the subject of predicates expressed by nouns and their modifiers (i.e., the sense of an NP like *the beautiful dark Indian girl* cannot become the “subject” of *girl*, *Indian*, etc.), and it cannot stand for zero sense, either, for, if it does, by definition it cannot denote anything at all, and in that case nothing is bound by the operators or saturates the open position of the restrictors, and the whole term structure collapses. Observe that, in virtue of the correlation [zero intension] \diamond {universal extension}, a strictly property-less “x” would pick out the whole Universe of Discourse (call it W for “world”), i.e., everything in W. However, in Rijkhoff (2002), as in F(D)G generally, the world contains much more than individuals, e.g., properties, events, predications, possible facts, speech acts, locations, times, manners, and what not (cf. Mackenzie 2004; Hengeveld this volume; Hengeveld and Mackenzie *fc.*), so, clearly, “x” cannot range over the set W, either. Under present F(D)G ontology, “x” (if it is indeed “x” that must stand at the core of terms) must have some content, but by no means that of a full NP: as stated above with reference to the metaphysician’s problem, minimally “x” must pick out entities with the ontological features that distinguish individuals from the rest of the ontology,¹⁹ whatever they are, and no matter what its content is, as the underlying structure of an NP is gradually built, its value must be

“updated” (cf. Dik 1997a: 150, and *infra*). The result is that, finally, the various tokens of the term variable pick out sets that cannot be unified, which makes any conjunctive representation incoherent.²⁰

Hengeveld’s recent pronouncements on the representational structure of NPs still keep Dik’s NP-internal “small x” variable (alternatively “e” for event-denoting nouns, “f” for property-denoting ones, etc., cf. Hengeveld 2004: 372; Hengeveld and Mackenzie 2006: 674, Hengeveld this volume, Hengeveld and Mackenzie, *fc.*) as the “subject” saturating a (possibly complex) nominal head plus its optional modifiers. In other words, still essentially Dik’s analysis. The structure proposed for the referential subact in Hengeveld (2004: 372), in particular, is $(\Omega R_1: [\Omega x_1 [\Omega f_1: \text{Lexeme}_N(f_1) : \mu(f_1)]] (x_1): \mu(x_1)) (R_1): \mu(R_1)$.²¹ However, on careful inspection, certain differences emerge: whereas in Dik’s successive accounts “small x” straightforwardly saturated the open predicates of the restrictor and all the satellites, and thus contributed in a transparent way to the determination of the NP’s truth conditions and referential potential (although with the caveats expressed above), Hengeveld’s “x” (also “e”, “f”, etc.) does not saturate the head nominal in that formula, it is not an argument of the Lexeme, and it is unclear how it can be an argument of the whole head structure $[\Omega f_1: \text{Lexeme}_N(f_1) : \mu(f_1)]$, which is already internally quantified and fully saturated. A parallel problem arises at the higher level, where the loosely added referential variable R_1 is bound by a referential operator ΩR_1 (irrelevant here) and restricted by a saturated NP $[\Omega x_1 [\Omega f_1: \text{Lexeme}_N(f_1) : \mu(f_1)]] (x_1): \mu(x_1)$ and a modifier $\mu(R_1)$, but, again, although R_1 is an argument of μ , it is unclear how it can fit into the argument-structure of the saturated nominal. On the other hand, it seems implausible to claim that both N and the NP each take two arguments of different ontological types or even domains, i.e., in the case of N, a property (“f”) (i.e., a sense) and an individual (the value of small “x”), and, in the case of the NP, an individual (but hardly the same individual) and a speech act R. The problem has not been solved even in Hengeveld’s last statement on the matter (Hengeveld this volume), where a head noun like *girl* or an adjective like *intelligent* in *the intelligent girl* each carry two arguments, an “ f_i ” variable bound by a low level operator and the term variable “ x_i ” bound by the higher $\Pi^x x_i$ operator.

Such difficulties are ignored in Hengeveld’s approach, and variables are treated as if they could be freely added to previously saturated structures, but the price to pay is high: “small x” (“e”, “f”, etc.) play no role in the compositional derivation of sense and reference potential, and, correspondingly, “R” can be added as a label to the NP, but nothing in the sense or

reference of the NP constrains its range. Thus, the correspondence among sense, reference, and use becomes essentially arbitrary.

Perhaps as a reaction to criticism in Escribano (2004b), Rijkhoff (this volume) has now removed some of the variables in Rijkhoff (2002), including all (tokens of) “F” variables but one, and all (tokens of) “small x” but one. If I do not misunderstand his proposal, in his new term formula “small x” represents *only* the (full) term variable, and stands for the referent of the NP (a discourse entity/its mental representation), but, contrary to traditional FG practice, unexpectedly occurs only once per term, at the very core, next to the noun. Obviously, this avoids one of my criticisms of Dik’s and Hengeveld’s accounts: if only one token of the “small x” variable is used, its value, whatever it is, will be unique, and the overall structure of the term will not become inconsistent on account of different tokens of “x” representing different sets or sense entities. However, if “small x” is the term variable and stands for the referent, its placement in the core of the term structure, as an argument of the bare noun, is inexplicable, for it is not the noun *per se* that denotes the referent. In that case, “small x” should be at the top of the representational formula, i.e., where “big X” was in Rijkhoff’s (2002) account.²²

As shown, the tension provoked by the Janus-like status of Dik’s original term variable has not yet been satisfactorily resolved within F(D)G. The fluctuation between Dik’s, Hengeveld’s, and Rijkhoff’s views of “small x” shows that F(D)G is still struggling to clarify its concept of “reference” and the relations between reference and sense, sense and NP choice, etc. That variables should proliferate in term representations, thus, is unsurprising, for in recent F(D)G work several non-isomorphic ontological spheres are invoked whose mutual relations need to be made more precise, i.e.:

1. The extra-mental world(s) endlessly shifting along the dimension of Time (Phys-W’s, hereafter), but directly assumed to be irrelevant in F(D)G (cf. Dik 1997a: 129; Mackenzie 2004; Hengeveld and Mackenzie *fc.*).
2. The different psychological world(s) of Speaker and Addressee (Psych-W’s, hereafter), traditionally considered the domain of FG reference (cf. Dik 1989: 113, 140; 1997a: 129; Keizer 1991: 1; Rijkhoff 2002: 27, 336; Mackenzie 2004: 973, etc.).
3. The transitory discourse-generated world(s) successively negotiated by S and A at particular stages of discourse and, again, different from each other, and obviously distinct from the Phys-W’s and Psych-W’s of S and A (Disc-W’s, hereafter).

4. The speech-act world (Sp-Act-W, hereafter), constituted by S, A, their referential and ascriptive acts, time, place, etc. And, finally,
5. The intralinguistic (“descriptive”) worlds of “senses” (Ling-W’s, hereafter), which also differ across speakers (and are bound to differ for each individual speaker along the time dimension, although linguists tend to dismiss changes of sense occurring within short spans of time as irrelevant).²³

Thus, clarifying the nature of F(D)G denotation/reference and the ontological domain over which each of the various F(D)G variables ranges should contribute to understanding which should be kept and which removed from the term formulae of the representational level. Let’s consider this briefly.

Starting with the variables attached at the top of NPs, Hengeveld’s R apparently ranges over events in Sp-Act-W (acts of reference). As to the classical term variable “x”, officially, FG referents are elements of Psych-W, and individuals from Psych-W (or Psych-W as linguistically construed) is what Dik’s “x” as well as Rijkhoff’s “X” are supposed to be bound by, cf. Dik (1997a: 129),²⁴ Rijkhoff (2002: 336), and Keizer (this volume, on “mental extension sets”). Rijkhoff’s (2002) “big X”, however, also picks out individuals from Disc-W,²⁵ in principle distinct from Psych-W(s), although, obviously, Disc-W and Psych-W may overlap (e.g., in internal monologue), and Disc-W(s) will be internally represented for both S and A. Hengeveld’s NP variable “x” (and perhaps intermediate “x” in Rijkhoff 2002) cannot range over “referents”, as that is what “big X” does, but there is no reason to take it to range over “senses”, either, since an NP expression does not reduce to its sense. If it is interpreted as a sense variable comparable to “f” and “small x” (except that it ranges over NP-senses), it is a Ling-W entity; otherwise it could range over events of Phys-W (physical NP occurrences, in whatever medium, i.e., sounds, inscriptions) or entities of some other “world”; the matter needs clarification, in my view. Further down, in the “descriptive” layers, F(D)G variables seem intended to range over Ling-W entities (cf. Hengeveld this volume; Keizer this volume). Rijkhoff’s (2002) “small x” does not pick out anything in Psych-W(s) or Disc-W(s), and is said to belong to the descriptive dimension of the term, which suggests that it stands for a “sense”, i.e., an entity of our Ling-W, like the *n*th-order properties over which his “f” variables range. Yet, Rijkhoff also says that “small x” picks out “space” regions with minimal properties (2002: 228, fn. 19, 229), and in that case it must be a referent in Phys-W, an ontological domain that FG traditionally considers

irrelevant, and the “space” concept and the intensional interpretation of “small x” that coexist in Rijkhoff’s (2002) account cannot be reconciled. Significantly, in Rijkhoff (this volume), which contains a revised version of term structure, “x” is said to stand for the referent of the NP, presumably a Disc-W entity as psychologically represented in virtue of the nature of linguistic construction. However, under that interpretation, why “x” is associated with the core layer, a purely “descriptive” layer, is unclear, as we said.

In sum: it is necessary to clarify the ontological domain of “x” and the other variables in F(D)G term representations, which means no less than deciding what kind of semantics F(D)G wants, perhaps a long-term enterprise, although, in my view, only immanentist theories will eventually be capable of accounting for what happens in natural languages. However, the immediate problem with all extant F(D)G proposals concerning the underlying structure of NPs is not just that the variables range over heterogeneous domains, relevant at different analytical levels, and clutter representations (cf. Escribano 2004b; Keizer this volume), but the fact that the logical architecture of the formulae makes even those variables that should play a role in the computation of senses ornamental (cf. *infra*). As a consequence, the NP sense that must help the speaker assign a value to “big X” and make the NP suitable to play the R role in a speech act does not result from the intensional value of the various tokens of “small x”, “f”, etc., via simple inheritance or any explicit compositional operation. In other words, even *prima facie* relevant sense variables play no role in the computation of the intension of the NP, nor in the determination of its referential value or range of use in speech situations, which must be stipulated by extra-compositional means.

2.3. Multiple variables/variable-tokens and the unification/update issue

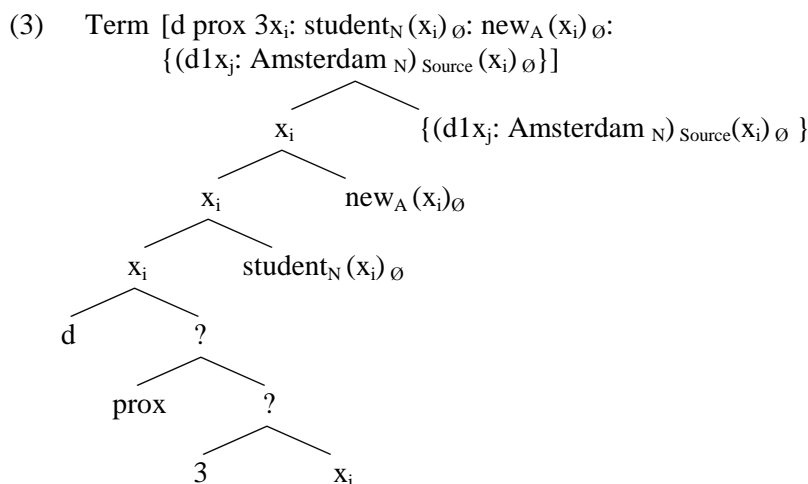
Let us now turn to a different aspect of the problem of FG variables. The structure of a term like (1), represented in (2), contains several variables or variable tokens whose mutual relations also need clarification. I illustrate the problem with Dik’s original term variable “x”, but the same difficulties arise with alternative “e” or “p” variables and with the several “f” variables at the head N, its expansions, and its satellites, that remain in Rijkhoff’s, Keizer’s and Hengeveld and Mackenzie’s recent accounts.

- (1) *These three new students from Amsterdam.*
 (2) $\text{dprox}_3x_i: \text{student}_N(x_i)_\emptyset: \text{new}_A(x_i)_\emptyset: \{(\text{d1}x_j: \text{Amsterdam}_N)_{\text{Source}}(x_i)_\emptyset\}$

According to Dik (1997b: 30) the relation between the various instances of “x” within a term formula is “anaphoric” (read: “x” is the same variable and has the same index value throughout), and he even introduces an anaphoric operator “A” to make this relation more perspicuous. That assumption faithfully echoes the logicians’ standard view of the “individual” variable “x” discussed above, but is incompatible with Dik’s theory of dynamically constructed terms (cf. Dik 1997b: 31). Indeed, Dik (1989: 116-119) had long before clearly stated that the structure of a term like (1) does not resolve into the simple conjunction of clauses that constitutes its predicate logic representation. The difference is that natural language restrictors are stacked, the construction of terms being a step-wise process (Dik speaks of “dynamic” or stepwise term construction, STC, hereafter)²⁶ whose effect is that restrictors previously attached constrain possible subjects and the predicability of subsequent restrictors, cf. Dik (1989: 116-119, 127-128; 1997a: 150; 1997b: 31-33, 37). In fact, that is what the order of the predicates separated by “:” is supposed to express in FG’s term formulae.²⁷ This, of course, has immediate consequences for the value of the possibly various occurrence(s) of the term variable “x”. Dik (1997a: 149) is very clear in this respect: “In the case of term structures, ‘entity’ should be understood as ‘entity as so far defined by the term under construction,’” and, discussing the predicability of new restrictors, Dik (1997a: 150) adds: “The issue of predicability is complicated by the fact that it is the full series of restrictors placed on a term as construed so far which determines the predicability of the next restrictor in the series”. Of course, this is entirely consistent with his idea of dynamic term construction, but it has a consequence that Dik apparently did not notice: each successive occurrence of the “x” variable stands for a different (gradually decreasing) set of entities, i.e., it is a different variable.

Which sets are represented by each variable is not entirely clear, though. Interpreting Dik’s various pronouncements on the structure of terms and the relative scope of operators and restrictors (cf. Dik 1989: 127-128, 138, 308; Dik 1997a: 132ff.; Dik 1997b: 23; cf. also Keizer 1991: 6, fn. 4) is tricky, for they are contradictory and the FG theory of scope is too vague (cf. *infra*), but, if operators are themselves stacked (Dik 1989: 308)²⁸ and restrictors (including the head noun, cf. Dik 1997a: 151) are attached in the order implied by the “:” notation, what Dik’s formulae claim about the structure of a term like (2) might be more perspicuously represented as

in our preliminary tree (3), left-to-right order irrelevant, and paraphrased as in (4) (parallel to Dik’s own paraphrase of his example *the three grey elephants*, cf. Dik 1997a: 161).



(4) *The near-speaker three individuals that are students, new, and from Amsterdam.*

Observe, though, that in that case an inconsistency arises, i.e., the variable x_i can hardly remain identical to itself throughout the stepwise derivation, or the effects of stacking will be vacuous. At the bottom of tree (3), x_i would presumably stand for the universe of discourse (Psych-W, in Dik’s work), or the subset of “underspecified individuals” in it. However, after the operator *three* applies, trios of such entities (whatever they are) are denoted; after the operator *these* applies, trios of such entities near the speaker are denoted; after the definite operator applies, discourse/context-relevant proximate trios of such entities are denoted, etc. The first restrictor, the head noun *student*, will then restrict that set of contextually relevant proximate trios of individual entities in Psych-W to relevant proximate trios of students, then the second restrictor, *new*, will restrict that set to that of trios of *new students*, and *from Amsterdam*, in its turn, will restrict the resulting set, etc., until no further modifiers remain. In other words, the range of referents of the presumed term variable x_i must be “updated” as the construction of the term proceeds. If the variable at the top represents the set to which the full term is intended to refer, the lower “x”

variables will stand for different supersets thereof which cannot be unified with it or among themselves. Thus, *pace* Dik, the relation between occurrences of “x” cannot be “anaphoric” under dynamic term construction.

The same difficulty arises if “small x” is “descriptive”, as it presumably is in Rijkhoff (2002): assuming it is not a “zero sense” variable, after all, but has some content, the lowest “x” will stand for a certain subset of denotations in Ling-W, i.e., type “x” senses, “underspecified individuals” (in opposition to properties, events and other kinds of entities), but as “small x” appears, possibly more than once, as the argument of N and of Quantity and Location satellites (cf. Rijkhoff 1992: 191; 2002: 228), it, too, must be “updated”, or the effects of stacking will be vacuous.

Yet, obviously, if “small x” is updated, under any of its interpretations, the effect is devastating: the formula ceases to be a coherent description of an individual or set (for the same reason a metaphysician’s “bundle” ceases to be a bundle if “x” stands for different subsets of constituents of the bundle). Of course, what holds of small “x” holds of corresponding (small) “e” variables, and ultimately of all variables that recur in stacked modification structures within NPs, VP’s, AP’s, or sentences. This problem, present in the earliest FG theory of terms, and, at bottom, a consequence of the ontological incoherence of “x” under standard metaphysical theories of individuals, remains unsolved in all later FG analyses (e.g., Keizer 2004; Keizer this volume; Hengeveld 2004; Hengeveld this volume; Hengeveld and Mackenzie 2006: 674) except Rijkhoff (this volume), where occurrences of the term variable “x” are restricted to one, although if that single occurrence is attached to the core noun, it will not pick out the referent of the full term, but a superset thereof, i.e., all individuals of the type specified by the head noun.

2.4. The variable-binding issue

As argued above, in formulae of the representational component there is no reason to add variables unless they have to be quantified over, and they are not quantified over when the predicates that carry them are saturated by appropriate arguments, so it is unclear why variables should figure, along with items that express their values, in FG representations. However, to stick to FG’s own assumptions, if an FG variable is like one of Reichenbach’s logical variables, as Dik (1997a: 54 footnote) says, variables must be bound by operators term-internally,²⁹ and this is not always the case in

current FG term formulae. On the contrary, multiple variable-binding violations arise.

To start at the bottom, the “f” variables associated with the head and satellites (σ_{1-n}) of Rijkhoff’s (2002: 227-229) formulae raise a “binding” problem, for they are not bound by any operator (assuming that the ω_1 , ω_{2a} , and ω_{2b} operators cannot simultaneously bind “x”, and “f”, two ontologically disparate categories, a reasonable assumption). Although Rijkhoff (2002: 227-228, and fn. 18) treats Quality satellites as being of a higher type than the rest (i.e., second-order, in the basic case) and has them take the value of the “f_i” variable of N as their argument, the unbound variable “f_i” of the head N cannot just “vanish” beyond L_0 : if the nominal predicate is to project its properties in a compositional way to its higher expansions, the result at L_1 must be the fusion of the two predicates (N, Quality satellite). Even if the variable of the satellite is “discharged” rather than bound by the sense of the head N, the “f” variable of the noun remains to be bound by an operator, and there is no other operator. This part of the problem is not solved in Rijkhoff (this volume), where the “f” variables have been dispensed with in the higher layers, but the “f” variable of the core noun remains, and remains unbound, cf. Rijkhoff’s formula (45).

Further binding violations arise with Rijkhoff’s (2002) “x” and “X”: if occurrences of “x” and “X” are understood (ignoring the implications of STC, cf. *supra*) as tokens of the same variables, respectively, as the representation suggests, biuniqueness issues arise, i.e., “x” and “X” are bound by more than one operator each, an impossible configuration under standard views on operator-variable relations; if, on the contrary, “x” and “X” are cover symbols for sets of different variables with “updated” interpretations, as STC entails, then either each operator can bind several different variables, an offending configuration, or some variables remain unbound. The problem persists in recent work like Hengeveld (2004: 372), Hengeveld and Mackenzie (2006: 673-674), Hengeveld and Mackenzie (fc.). In Hengeveld (2004), for example, the Quality operator Ωf_1 must bind the “f₁” variable at the lexeme N, but, unless no updating has occurred, the “f₁” variable at the modifier μ remains unbound, and the same applies to the higher operators Ωx_1 and ΩR_1 with respect to the pairs of variables of type “x₁” and “R₁” at the heads and modifiers of the respective structural levels. This problem is not solved in Rijkhoff’s (this volume) revised five-tier model of terms, where operators no longer bind the term variable that survives at the core noun. Rijkhoff proposes, instead, to analyze operators as predicates taking the remainder of their respective layers as their argument, a solution I approve of (cf. Escribano 2004b, 2006), but still: that

leaves the surviving “x” variable of the noun as unbound as the surviving “f” variable in Rijkhoff’s formula (45).

Needless to say, the variable-binding problem becomes more acute the more articulate the internal structure of terms grows. Keizer (2004: 15-16) reasonably recalls that, since nouns may denote entities of different order (zero, 1st, 2nd, 3rd, 4th), we must expect arguments and modifiers of different “levels” (Keizer 2004: 18). A better term, I suggest, is just different “orders” or “types”, rather than “levels”, since although a term may consist of (at least) the levels Rijkhoff (2002, this volume) postulates (in fact, rather more), no term can have more than one type of variable at its core, i.e., a term may denote an individual, or an event, or a speech act, etc., but not several of such entities at various structural levels; the parallelism with clauses breaks down here. However, the point is that Keizer’s approach obviously multiplies the “types” of heads and modifiers and, under the present FG approach, the number and types of the variables awaiting suitable operators to bind them. I leave to the interested reader the task of specifying how many variables will remain unbound under a more articulated NP structure like Keizer’s.

2.5. Summary on variables

In short, leaving aside (as heterogeneous and intensionally irrelevant, in principle) big “X”, (the NP variable “intermediate x”, if appropriate) and “R” variables at the top of NPs, the variables that figure in the lower, ontologically homogeneous, layers of FG’s current representational formulae (the “descriptive” layers) still pose serious problems: “small x” is properly neither an “individual” nor a “zero sense” variable, and each complex term may well contain several “small x” variables (correspondingly, several “e”, “p” variables in the case of terms naming events, etc.) with different values, some, including Rijkhoff’s (this volume) remaining “x” variable, inevitably unbound, as are some of the “f” variables that Rijkhoff (2002, this volume) and Hengeveld (this volume) introduce and Keizer’s (2004, this volume) implied higher-order sense variables. Furthermore, in principle, the results of stacking under the stepwise construction of the term cannot be computed if variables are not “updated” and properly bound, but, on the other hand, the formulae become inconsistent if they are, as they no longer represent the intended entity. As a consequence, current FG term representations are ultimately incoherent and fail to capture the compositional relations between different elements of term-structure or their respective con-

tribution to the determination of sense, reference and pragmatic adequacy. This is a serious flaw, for once the status of variables is shown to be questionable, everything else in the FG representation of terms is, i.e., it is unclear what operators bind, what restrictors restrict, or how senses are assembled and reference compositionally determined.

3. Operators

Another major aspect of FG term representations is the set of “operators”. Broadly speaking, FG term operators are inspired by logical quantifiers, cf. Dik (1989: 137, fn. 1; 1997a: 160 fn. 1), but, under close inspection, they are rather unlike them, after all. As stated above, standard predicate logic quantifiers are formula-level constituents, explicitly take scope over restrictors containing unique unbound variables, and (vacuous quantification ignored) bind them. The syntactic and semantic function and scope of FG operators, on the contrary, is rather less clear, which casts further doubt on the soundness of the FG formalism.

The first difference between logical quantifiers and FG term operators is that, as Dik himself observes (1989: 149; 1997a: 168), term operators are not sentence-level constituents, but NP-level ones. Strictly speaking, therefore, Frege’s analysis of quantification is incompatible with FG, and the semantic quirks of *some* etc. must ultimately be captured under a modifier analysis (e.g., that of Montague 1974 or Generalized Quantifier Theory; cf. Barwise and Cooper 1981).

However, what is specially disturbing about the nature of FG operators is that, as shown directly, it is often unclear how they fit into term structures, whether they must have restrictors in their scope, whether they must bind variables (for variables are often unbound, as shown), and, if they must, whether they bind only one variable or are expected to bind more, even of different ontological types and domains, as implicit in current FG formulae (cf. *supra*). Therefore, the conceptual problems surrounding variables and operators intertwine and boost each other.

Dik presents term operators primarily as elements of abstract term structure, but does not clearly define either their semantic function or their syntactic structure with respect to their operands in the structure of terms/NPs, respectively. In general, an FG operator is a grammatical function applying to an “operand” to yield a unique “value” (cf. Dik 1989: 299-300), but it is clear that Dik did not consider operators *bona fide* predicates, since they are not assumed to take arguments and are not listed in the Fund, cf. Dik

(1997a: 160-161). Apparently, in Dik's theory, they are "features" (ultimately: predicates) applying to the term variable "x", or to "x" as previously operated on by lower operators (cf. Dik 1989: 308; Keizer 1991: 6), as suggested in our structure (3) above, or of "x" as previously "modified" by both lower operators and lower restrictors, under Dik's (1997a, b) new layered account of terms.

Of course, if operators are indeed abstract features, we need not expect them to surface as free forms with identifiable syntactic functions (i.e., they can as well be realized as morphological or lexical features on the head noun), but in English and similar languages most do have overt exponents, and archetypal ones are discrete determiners or quantifiers in surface syntax, i.e., under the FG analysis of NPs, noun dependents of some sort in the syntax. Rijkhoff (2002: 99) calls them "grammatical modifiers", in opposition to "lexical modifiers" (= satellites), and Hengeveld and Mackenzie (2006: 671) further add that what makes them different from other functional features is that they are non-relational (i.e., monadic) predicates, an analysis shared by Rijkhoff (this volume), where each operator takes the next lower layer of structure as its argument.

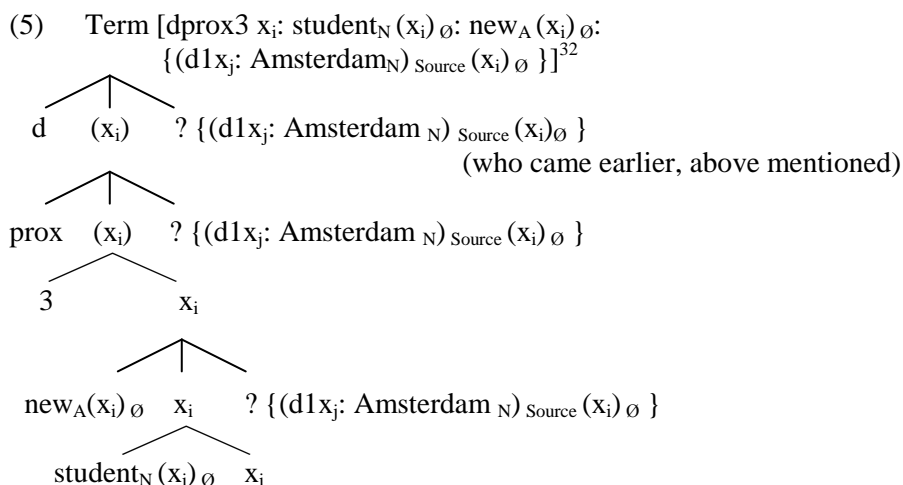
To my knowledge, Rijkhoff's is the clearest statement on the matter so far, but also a major departure from traditional FG doctrine on term operators. It definitely distinguishes them from logical ones, makes them modifiers, and of course entails that they cannot bind variables. As a consequence, either the variables are removed (as claimed in Escibano 2004b and here), or they will remain unbound, and Rijkhoff removes most of them, although not all, cf. *supra*.³⁰

However, that is not the standard FG view of term operators. If we consider what has been written on term operators since Dik (1989), their broad role as "grammatical modifiers" is clear, but what they formally "operate on" or "modify" (i.e., their syntax and scope) is not. In Dik's own writings, at least two different views co-exist:³¹

- a. In his formulae, and even more explicitly in his occasional paraphrases thereof, e.g., Dik (1989: 315-316; 1997a: 62, 161; 1997b: 23, etc.), operators operate on the term-variable before any restrictors (not even the head!) apply, which strictly corresponds to the derivation (3) above. However, Dik's official view elsewhere is b):
- b. Operators are distributed in n hierarchical layers, with those at any one layer taking scope over operators and satellites at lower layers, cf. Dik (1989: 138), Dik (1997a: 161).

The problem, of course, is that no single standard derivation, as represented by a graph structure, can satisfy both (a) and (b), so it is necessary to make a choice and adapt formulae to their paraphrases and graphic representations. Structure (3) above, in particular, satisfies clause (a) but violates (b). To satisfy clause (b), the structure of a term like (2) should be like (5).

The layered representation (5), in the spirit of Dik’s (1989: 50, 138) earlier parallel analysis of layered clause structure, corresponds to what is taken to be the intended view in Dik (1997a: 161), and is approximately the analysis of terms assumed in current FG work like Rijkhoff (2002: 218-228, 238, 314, 337, 341-343), Hengeveld (2004: 372), Hengeveld and Mackenzie (2006: 671), Keizer (2004), Hengeveld and Mackenzie (fc.), and Rijkhoff (this volume), except in what concerns the number of layers, which varies somewhat (five, in Rijkhoff’s new proposal, more in Keizer 2004).



The conflict between (a) and (b) arises because, unfortunately, Dik’s term formulae are vague as to the structure of operators and were not revised in Dik (1997a, 1997b) to match his new layered account of terms. The vagueness is due to the lack of explicit indications as to how operators fit into terms. Observe that whereas the hierarchical structure of restrictors is clearly defined by “:”, an explicit indication of the order in which restrictors contribute to the semantics of the term and their scope, cf. Dik (1997a: 62), operators are not separated by colons in term formulae, but juxtaposed. In Dik (1997a: 181), the term formula for *these three red books* is

still (6), but all (6) suggests is that all the operators apply to “x” before any of the restrictors does, as in derivation (3) above. Since Dik also says elsewhere (Dik 1989: 308) that operators are stacked and take scope over each other, in derivation (3) they have been hierarchically ordered, but as a matter of fact no hierarchical ordering follows from term formulae.³³ The same ambiguity remains in the schematic formula of underlying term structure that Dik (1997a: 163) offers, i.e., exactly (7), which, again, suggests that the higher operators (all except Quality) apply to the variable “x” (as a complex predicate?; observe that in (7) only Quant has a variable) before the restrictors do, i.e., essentially still clause (a) and derivation (3) above, rather than clause (b) and derivation (5).

- (6) (d prox 3 x_i: *book* [N]: *red* [A])
 (7) ω_2 -Loc ω_2 - Quant x : [ω_1 - Qual pred [N] (args)]

Thus, even if Dik’s (1997a: 161) oversight as to operators’ taking scope over the whole term is ignored as inconsistent with his pronouncements elsewhere, FG term formulae, his own paraphrases thereof, and the layered view of terms do not quite match in what concerns what operators operate on or modify, and substantial ambiguity remains.

Adopting clause (b) raises another problem: The “flat” structure of layers in all current FG term structures is inconsistent with STC and entails that operators and satellites of the same layer have each other in their scope, which, in its turn, causes an argument-binding violation.³⁴ The first point should be obvious: if STC holds, layers containing more than two constituents, say an operator, a head and a satellite, just cannot be “flat”, and nothing prevents multiple satellites (or even multiple operators) within any given layer. As to the second point, Rijkhoff (p.c.) rejects it and prefers to say that operators and satellites of the same level L_n take the same argument (Layer L_{n-1}), as he claims in Rijkhoff (this volume). Yet, it is unclear how two constituents of the same structural layer that contract predicate-argument relations with a third constituent can fail to take scope over each other. If A is an argument of P1, where P1 is, say, an operator, P1 and A must be constructed with each other, and must be in each other’s scope (i.e., sisters); if a second predicate P2 – say, a satellite – also takes A (and not [P1+A]) as its argument, then P2 must also be constructed with A, and a sister to A. Hence A is eventually constructed with both P1 and P2, and, since “sister of” is a transitive relation, P1, A, and P2 must all be sisters, and therefore in each other’s scope. Hence, either FG “scope” does not mean what “scope” means in both logic and all other kinds of linguis-

tics,³⁵ or operators and satellites of the same layer are indeed in each other's scope. Of course, FG "scope" could be a primitive relation, or just not be based on "construction with", but, say, on linear precedence,³⁶ an asymmetric relation, but in that case FG scope should be carefully stated and regulated, or constituent ordering predictions based on it will be untestable, cf. section 4 *infra*. However, nothing in the FG literature suggests that "scope" means something different in FG from what it means in all other major theoretical approaches in the field.

As stated, the problem also has an argument-binding side: A constituent cannot, in general, be simultaneously an argument of two predicates P1 and P2. This is a standard assumption in most linguistic approaches that take semantics seriously. The relevant constraint has different names in various theories (*Functional Uniqueness*, in Bresnan's Lexical Functional Grammar, the *Theta Criterion* in Chomskyan Principles and Parameters Theory/Minimalist Grammar, etc.) but the basic idea is very simple and can be stated in theory-neutral terms: argumenthood is a functional concept, and functions must yield a unique value; hence, as soon as an argument receives a context-dependent semantic role (Zero, Experiencer, etc.) from a predicate P1, it cannot receive a second semantic role from a second predicate P2. It follows that if a layer L_i is an argument of the satellite, it cannot simultaneously be an argument of the operator, and vice-versa.³⁷

The other worrying respect in which FG term operators differ from standard logical ones is that, under either Dik's (1989, 1997a) or Rijkhoff's (2002) account, whenever a term contains a different number of operators and variables, either the variables must be multiply bound, a non-standard property of variables, or operators need not bind variables, a non-standard property of operators. This is the case in both (3), and (5) above, but the technical problem is different depending on how variable occurrences are interpreted. If different occurrences of "x" ("f", etc.) are interpreted as tokens of the same variable, the variable is usually multiply bound (assuming operators are operators and must bind a variable) and the resulting structure is incoherent; on the contrary, if they are taken to stand for different variables, some of them may remain unbound, along with all variables (e.g., "f", etc.) for which current FG term formulae simply do not provide operators, cf. *supra*. Under Rijkhoff's (2002: 228) analysis, for example, the lower operators ω_1 and ω_{2a} do bind one "x_i" variable token each, but the variable, like Dik's, is assumed to be identical to itself throughout, and ends up multiply bound. On the contrary, as stated, the "f" variables of Rijkhoff's (2002) formulae are all unbound unless his operators can each bind two variables of different ontological types (i.e. "f" and "x", respec-

tively), an uncanonical assumption. Even in more recent work like Hengeveld (2004), the operators Ω_f , Ω_x , Ω_R must each bind two variables corresponding to non-identical sets (i.e., “f” at the N lexeme vs. “f” at the modifier, “x” at the head vs. “x” at the modifier, and referential “R” at the head vs. “R” at the referential modifier).³⁸ As to Rijkhoff’s revised term model (Rijkhoff this volume), only two variables remain (“x” and “f” at the core N), and they are both unbound.

Such problems result from a) the heterogeneous content of term formulae and consequent proliferation of extraneous variables in them, and b) their layered, but weakly integrated, architecture. Within current FG assumptions, the obvious way out of the vacuous quantification pitfall is to further refine the hierarchical structure of terms, distinguish more layers with variables denoting intermediate entities (sets, ensembles, restricted sets, quantified restricted sets, etc.), and allow each such layer to contain its own operator binding its own variable, i.e., to expand current analyses like Keizer (2004) or Rijkhoff (2002, this volume) by adding new binary branching layers as determined by STC. Since, as argued here, different occurrences of “x” (“f”, etc.) inside a term must stand for different sets (i.e., they are technically different variables), adjusting layers to STC will surely leave variables unbound.

To get them bound may seem trivially easy at first sight: FG might simply introduce, beside n variables, exactly n (ad hoc) operators to biuniquely bind them. However, even if the additional operators can be justified within a richer type system, enforcing biuniqueness in operator-variable relations will not of itself yield an integrated term structure that can be compositionally computed. The problem, in a nutshell, is this: to the extent it could be maintained, Dik’s term variable “x” acted as a “pivot” that held the various semantic elements of the term together, but once “x” is shown to stand for different sets, the glue that held terms together vanishes, and it is necessary to replace it with something. Piling up perfectly canonical layers of operators, correctly bound variables, etc. on top of each other does not help, for such layers contain predications with respect to different objects (sets), and no integrated structure results whose sense can be compositionally calculated. If the current strategy is pursued, FG will have to develop some other device to hold the various constituents of term structure together as parts of a unified semantic representation of some entity or set of entities.

In sum, the real issue, at bottom, is that the computation of sense (and reference potential) cannot be compositionally done as Dik assumed, so some other means is needed, but piling up parallel and carefully symmetri-

cal layers of structure on top of one another or even endowing each of them with related variables and operators, predicates and arguments, satellites and heads, etc. will not make the slightest difference. What is needed, instead, is a more closely integrated representation of terms and a compositional mechanism that monotonically calculates the intensional contribution of the various heads and modifiers involved. One way to achieve that, of course, is to treat current FG term operators as one-place functions (i.e., predicates) directly taking distinct layers of the term as their argument, with no variables involved, as proposed in Escribano (2004b) and Rijkhoff (this volume), although, obviously, such a strategy represents a significant departure from FG tradition, with wide-ranging implications at other levels (clauses, etc.).

4. NP syntax: construction, domains, layers, scope, and surface order

4.1. Current “flat” layers as violations of Dik’s Stepwise Term Construction

To the extent each layer L_n of term structure contains one (or more) operators ($\omega_{n1}\dots$), a nominal core ($= L_{n-1}$), and possibly one or more satellites ($\sigma_{n1}\dots$), an additional conceptual objection to current FG doctrine is simply that such multiple branching abstract structures cannot even be built unless Dik’s important idea of “stepwise term construction” (STC, cf. Dik 1997a: 134-136; 1997b: 31ff, 37) is abandoned. According to STC, not only operators, but also satellites, cannot just be flatly added as sisters to previous structure, but must be stacked bottom-up in the order in which each makes its semantic contribution to the term.³⁹ Dik’s STC idea never properly crystallized into an explicit FG principle, although it plays an important role in all his discussions and paraphrases of how the sense of terms is built and interpreted, but in my view he underestimated its importance, for, if STC is observed, drastic consequences follow, e.g., only binary-branching structures can be derived, functional uniqueness follows, new operators and restrictors automatically acquire asymmetric scope⁴⁰ over previously attached ones, and, by definition, the structural depth (i.e., the number of construction stages) of a term representation built out of n constituents is $= n-1$.⁴¹

In the case at hand, of course, STC leaves n -ary branching layers out of the question. Observe that, if only two term constituents intervene (i.e., operator and core,⁴² or core and satellite), no problem arises, but if the

elements to be constructed into a given layer are a core, an operator, and a satellite (the prototypical case under current FG analyses of intra-layer constituency), STC requires two of them to be attached to each other first, i.e., either [Op+Core], or [Core+Sat] (left-to-right order irrelevant) must first become a new constituent, and the third element (Sat, Op, respectively) must wait until the next constructive operation allows it to get attached to the new core, i.e., under STC *[Op+Core+Sat] is an unreachable output; possible outcomes are either [[Op+Core]+Sat] or [Op+[Core+Sat]].

To my knowledge, the STC idea has never been explicitly abandoned in FG, but, in practice, Dik himself ignored it in his analysis of clauses and NPs, and all subsequent work on term and NP structure (e.g., the current layered theory) has been flagrantly inconsistent with it. I take it, then, that STC is no longer, *de facto*, a principle of FG, but this is an unfortunate accident, for if only the implications of STC are systematically extracted, there follows a much more explicit and semantically well motivated account of construction, constituency, domains, layers, scope, and surface constituent order. Also, given STC, current FG principles like *Domain Integrity* (DI, hereafter) and *Head Proximity* (HP, hereafter) can be strengthened, particularly as to their hierarchical importance,⁴³ many apparent violations of DI and HP are avoided, their systematic exceptions explained, and better empirical predictions follow. Even more, not only DI and HP, but also other descriptively useful, but non-categorical and only approximately accurate FG “principles” like *Iconic Ordering*, *Linear Ordering*, *Centripetal Orientation*, and the core cases of *Increasing Complexity*, cf. Dik (1997a: 399–401) become redundant, since they follow as consequences of more general principles, systematic exceptions to them can be explained, and the resulting structures make the conceptual problems above easier to solve.

On the contrary, if STC is ignored, as has unfortunately occurred in FG, given n constituents, possible outcomes of construction multiply, constituents that are necessary at both the semantic and the syntactic level are not constructed, only loosely integrated domains like NP and Clause tend to be considered, constituent ordering possibilities and DI and/or HP violations proliferate, scope between constituents is symmetric where, under asymmetric scope, predictions would improve, etc. The negative consequences affect all aspects of semantics and syntax, including constituent order at surface structure, and cast unnecessary doubts on the standard (and, indeed, optimal) view that expression rules must operate subject to Iconicity on underlying semantic representations, cf. Rijkhoff (2002: 226, 253, 337–338, etc.).

4.2. Nice consequences of strengthening key FG syntactic concepts

Unfortunately, although the concepts and constructive operations *de facto* used in FG do not differ significantly from those in other major and explicitly formalized linguistic theories, what exactly happens when two elements A and B are constructed into a bigger element C has never been carefully discussed in the FG literature, to my knowledge, and the result is an informal view of *Construction* and loose concepts of *Domain*, *Head* and *Scope* which negatively affect the nature of the principles that FG can formulate (preference rules) and unnecessarily weaken their empirical predictions. Such formal details, simply, have not attracted much attention in FG, but it is necessary to work them out, or rather it is a pity not to, so I will here, in a more constructive vein than in the preceding sections, try to spell out, in terms as theory-neutral as possible, what Dik's STC idea really entails given FG's other assumptions and why it is a good idea and must be reinstated to the place of honour it deserves among FG principles.

We must start with the concept of *Construction*. Needless to say, when A and B are here said to yield [A+B] the symbol “+” is used merely as a convenient indication of the existence of a construction (dependence, etc.; a theory-neutral term intended) between A and B. As a matter of fact, the construction of A and B can hardly reduce to a set-theoretic operation like the sum (or the product) of A and B (*pace* Chomsky 2005; cf. Escribano 2005b). Rather, all linguistic theories explicitly or tacitly agree that A must be functionally integrated into the structure of B (or *vice versa*), and it is so integrated to the extent it “satisfies” B's combinatory requirements (theory-neutral terms intended; read “valency”, “argument structure”, “selection features”, etc.). Under various names, what everybody sees occurring in cases of Construction corresponds to the intuitive idea of “satisfaction” of functional properties of various kinds (adicity, semantic role, inflection, etc.). Roughly, A may be constructed with B mainly for one of three reasons: 1) to satisfy B's adicity (e.g., B's lexical entry requires an argument of a certain type and assigns to it a certain semantic role, but that argument remains unsatisfied, i.e., valueless, until A is attached to B); 2) to satisfy one of its own functional attributes, e.g., if A is a referring term, it will be interpretable only if it receives a semantic role, so a valued semantic role feature is inherently expected, but the value of that feature⁴⁴ (Agent, Beneficiary, Zero, ...) remains unspecified until A is constructed with some head B; and, depending on how morphology is handled, another reason may be 3) to license A's or B's inflectional features (e.g., a NP in the Nominative, Accusative, etc., or a verb form carrying certain inflections, fit only in con-

struction with certain other heads or dependents, etc.), cf. Escribano (2005b).

When appropriate items come to be in construction with each other, their respective requirements are satisfied, and the resulting structure “fits” together and becomes fully interpretable in exactly the sense in which an algebraic formula can be fully evaluated only when all its variable symbols have been valued. Again, terminology varies greatly, some theories will say that construction rests on “unification”, “satisfaction”, “saturation”, “agreement”, etc., or a subset of those, but the fundamental insight is shared by everybody, as far as I know. I try to use the terms that seem more theory-neutral and either in (tacit) use in FG or not likely to be resisted by FG scholars. See Escribano (2004a, 2005b) for details on what I really think happens.

It is *Satisfaction* that allows/drives the constructive process. Notice that if, say, B is fully satisfied, that is, functionally saturated for all its attributes (adicity, roles, etc.), there will be no functional slot available for (a feature of) A to fill into B, and A will simply not be constructible with B and will not be constructed with it. Observe, furthermore, that Satisfaction is a two-place relation, i.e., A satisfies B, not a 3- or *n*-place relation (cf. *A satisfies B C), which entails that Satisfaction-based construction is inherently a binary process (like Dik’s STC). Mutual Satisfaction, even multiple mutual Satisfaction, is allowed,⁴⁵ but Satisfaction must be unidirectional or bi-directional, and involve just two participant structures A and B each time it occurs, although each participant may consist of a rich set of attribute value pairs. Crucially, it is not possible for A to satisfy both B and C, or vice-versa, for B and C to both satisfy A. The former case would violate functional uniqueness (in whatever respect, semantic, inflectional, etc., is relevant); as to the latter, Satisfaction will trigger two distinct constructive operations, and one will necessarily occur first. As a consequence, two of the three participants (say, B and A) will be constructed into a new object O, and C will eventually satisfy a feature of A or B only in the sense that all features of A and B will have become features of O, but, strictly speaking, C will be satisfying a feature of O at the point it is constructed with O.

In the case of term structures, assuming participants to be Op(erator), Core, and Sat(ellite),⁴⁶ Satisfaction (> STC) yields two possible outcomes: a) Op+[Core], and b) [Core]+Sat, where [Core] may correspond to either a) or b), i.e., successive Cores (1...*n*) will result, with automatic adjustment of their senses and referent potential (in the direction of increasing intension and decreasing extension). *Op+Sat, *Op+Op, *Sat+Sat, *Core+Core

are all directly forbidden provided Ops and Sats are basically predicates (“grammatical” ones, in the first case) and core segments are nominals functioning as their arguments, as assumed in FG analyses like Rijkhoff’s (this volume). The reasoning involved is trivial: informally, a predicate needs an argument, and will not contract a Satisfaction relation with another predicate, so it will not be constructible/constructed with it;⁴⁷ correspondingly, a Core nominal inside an NP functions as an argument (of Sats or Ops, cf. Escribano 2004a, 2005a, 2005b, 2006, and Rijkhoff this volume), and will not satisfy, or be satisfied by, another nominal.⁴⁸

Furthermore, a natural, and stronger, concept of *Domain* immediately results: a “domain” is now just the set {A, B}, where A, B are in a Satisfaction relation, or, informally, a step of STC in which a successful Satisfaction relation is established, i.e., the noun with its first complement, or Core_i and its Sat (e.g., AP, PP, Cl, etc.), form a *Strong Domain*, and so do Op and its Core_j (e.g., Determiner and Core_k, Quantifier and Core_l, etc.). On the other hand, a full NP structure is not a strong domain, since many items in a complex NP may be completely unrelated from a semantic and syntactic viewpoint (i.e., not in a Satisfaction relation). The advantage of such stronger domains is their strict cohesiveness, i.e., by definition, nothing may intervene between Op and its Core, or Core and its Sat. If a constituent X occurs between Core and its Sat, X must be constructed with either Core or Sat; if X is constructed with Core, [Core+X] will become accessible to Sat, but Core itself will not, and since [Core+X] will not be of the same *Type* as [Core] (if it is a possible type at all), even though it became accessible, it would not trigger construction with Sat under Satisfaction; correspondingly, if X is constructed with Sat, [Sat+X] will be accessible to Core, but Sat itself will not, and [Sat+X] will anyway not be the right type of predicate to combine with Core. Thus, (*strong*) *Domain Integrity* follows automatically, and since “weak (= FG’s) domains” resolve into recursive construction of strong domains, DI is redundant and need not be added to the theory at all.

Similarly, a new concept of *Head* relativized to different stages of construction (= Core) can be defined, with further consequences in the area of surface constituent order. In fact, STC already entails that the noun is the head of the NP only at the first stage of construction; as N is constructed with its own arguments, satellites, or operators, the Head (= Core) becomes a gradually expanding nominal phrase. It follows that at any stage S in a STC, the participants in construction are invariably a Core (acting as Head, in the revised sense) and just another element, Arg, Op or Sat. That removes the anomaly that Keizer (2004: 8) observes concerning the fact that

only terms contain multiple restrictors, a problem not solved by just re-labelling N as the only restrictor and the rest as satellites, as she proposes.

If only Core and Op are involved, assuming that Ops are monadic predicates taking the Core as their only argument, Op cannot but be adjacent to Core, since arguments must be satisfied and, by assumption, Op has only one argument, cf. Rijkhoff (this volume). Observe, furthermore, that, since both Op and its Core now are head(s), HP automatically follows from Satisfaction in this case.

If it is only Sat that is constructed with Core, as the ultimate heads of satellites may not be monadic, two cases must be distinguished. If Head(Sat)⁴⁹ is monadic (e.g., an adjective like *beautiful*), it will only require its “subject” (= Core), and Head(Sat) will be adjacent to Core, as HP requires. Of course a monadic Sat may carry its own satellite (e.g., an adverb, as in *extremely beautiful*), but in that case the internal satellite of Sat will never intervene between Core and its Sat. The reason is, again, Satisfaction as applied subject to *Priority*, another common assumption across contemporary linguistic theories, i.e., in the broadest theory-neutral version, that attributes are satisfied in a certain order as a consequence of their “rank” (as expressed by various “hierarchies”, e.g., the *Thematic Hierarchy*). Of course, *Priority* is itself a corollary of the binary character of Satisfaction: if two features are to be satisfied, one must “wait”. In the case at hand, Core is an argument of Sat, and arguments of heads must generally be satisfied before the resulting phrase, in its turn, satisfies a satellite, or it will be too late to satisfy them at all, as the types of the head as modified by the satellite and its still unsatisfied argument would no longer match.⁵⁰ Hence, Head(Sat)+Core will form a continuous constituent, and both DI and HP, again, follow directly. Now, if Head(Sat) is polyadic, in principle it would seem possible for a non-subject argument of Sat to be satisfied first and linearly intervene between Head(Sat) and Core, in violation of DI and HP, and yet, interestingly (in languages like English, Spanish, French, Italian, etc., at least) such cases never occur, cf. **a keen on jazz professor*, **a containing books box*, **an abandoned by his wife colleague* (cf. Escribano 2004a, 2005b).

The reason is that an independently necessary additional principle with an “iconicity” flavour intervenes to control the correspondence between hierarchical position and linear precedence (i.e., a possible FG “expression principle”) and just happens to block such HP violations. The best known formulation of that principle is Kayne’s (1994) *Linear Co-occurrence Axiom* (LCA, hereafter), which, in an informal, theory-neutral, version that

FG may easily adopt, says simply that if A has asymmetric scope over B, A precedes B.

In conjunction with the type of construction defended here, LCA plays a crucial role in preserving HP in this case for reasons, again, related to Satisfaction. When a phrase (e.g., a Sat) is constructed with another (e.g., a Core), their respective internal heads may both previously have been constructed with arguments (typically) or satellites. As a consequence, if both the noun and its Sat initially had arguments to satisfy, their respective arguments must be adjacent to their heads and may separate Head(Core) from Head(Sat), in violation of HP. Yet, as stated, the Satisfaction of arguments is subject to priority, cf. Escribano (2004a, 2005a), and, in particular, subjects are always satisfied last, hence the open slot of Sat will always be the last argument of Head(Sat) to be satisfied. Since “last” in a STC (“bottom up”) derivation also means “highest”, if LCA is in force, Core, being the subject of Sat, will always be hierarchically higher than other Sat arguments. It follows that Core will invariably take asymmetric scope over Head(Sat) and all Sat’s other arguments, and that Core will precede Sat and all Sat’s internal complements.

That reasoning predicts two possible orderings, i.e., Core+[Head(Sat)+Comp(Sat)], or Core+[Comp(Sat)+Head(Sat)]. In the second case, Comp(Sat) will intervene between Core and Head(Sat) and HP will still be violated. However, LCA applies systematically, to all phrases, and this has particularly nice empirical consequences in SVO languages like English:⁵¹ since Head(Sat) is itself hierarchically higher than, and takes asymmetric scope over, the constituents of Comp(Sat) (by definition, since it is a sister of Comp(Sat)), Head(Sat) itself precedes its own complement. In sum: Core precedes Head(Sat), and Head(Sat) precedes complement of Sat. The only possible configuration, then, is Core+[Head(Sat)+Comp(Sat)], i.e., Core precedes any Sat containing complements (PPs, Clauses, complex APs, etc.), and Head(Sat) is adjacent to Core, which, again, saves HP (under the definition of head = Core).

This captures the true side of HP, but HP has systematic exceptions, and it is important to understand why sometimes HP just cannot hold. With “head” interpreted as “lexical head”, as in FG, if HP is a principle it is bound to enter into conflict with the higher-ranking principle of Satisfaction. For example, the head of the Core nominal *analysis of the problem* can never be adjacent to the head of the Sat *satisfying FDG principles* in *(an) analysis of the problem satisfying FDG principles*, cf. **an analysis satisfying of the problem FDG principles*, **an of the problem analysis satisfying FDG principles*, **a satisfying analysis of the problem FDG*

principles, etc. The reason is that all such hypothetical possibilities violate Satisfaction under Priority and/or LCA once or more. For example, in the first case, the argument of *analysis* and the first argument of *satisfying* have not been constructed with their proper heads, and, of course, the attempt to satisfy HP between *analysis* and *satisfying* causes two HP violations elsewhere. In the second example, LCA is violated, since *analysis* has asymmetric scope over (*of*) *the problem* and should precede it, which will cause the systematic HP violation cited. Finally, in the third example, it is the complement of *satisfying* that has not been properly attached to its head, in violation of Priority and Satisfaction (plus HP elsewhere). Granted Satisfaction under Priority, the complement of *satisfying* should have been satisfied before its subject, not the converse, which yields the idiomatically correct solution (at the expense of a systematic violation of HP).

However, HP is just what Satisfaction predicts when the participants are lexical heads, and when they are not, HP holds or not depending on other principles. In short, HP is not a “principle”, but a generalization that sometimes holds and sometimes systematically does not, but the crucial point is that, in either case, the right facts follow from the way the “real” and more general principles of Satisfaction, Priority, and the LCA interact. Thus, it is not necessary to add to the grammar such ordering preferences. As a matter of fact, much the same can be said of nearly all the other FG general ordering principles in e.g., Dik (1997a: 399-404). Why it is so should be obvious to the reader at this point, but let us briefly recall the trivial reasoning involved in the case of SVO languages; for SOV and VSO languages the reasoning is more complex, and depends on assumptions about displacement that FG does not share, so they will be ignored here.

Iconic Ordering depends on semantic function, which depends on Construction under Satisfaction, i.e., a constituent C will have function F with respect to another constituent C' (e.g., an argument, a satellite, or an operator) to the extent it satisfies (arguments), or is satisfied (satellites), or both satisfies and is satisfied by (features of) C' (operators), and Satisfaction is subject to Priority, which fixes stage of attachment to the construct (roughly: first arguments, if any, according to their prominence, then satellites, according to the types they select, then operators; same sequence applied to all participants in construction), and stage of attachment determines Scope, which, under LCA, in turn determines surface order.

Similarly, *Centripetal Orientation* has two sides, one vertical, the other horizontal, or, informally speaking, “onion-like” stacking plus “mirror-image” sequencing. The former trivially follows from the order of Construction under Satisfaction, which fixes the relative scope of dependents;

the latter is an automatic consequence of the internal structure of the dependents themselves, which follows from the same principles and the LCA as applied inside the dependents. Briefly, if a Sat (of whatever scope) is monadic, it will precede its Core and Head(Sat) will be at the right edge of Sat and closest to its subject (a case of HP above); if Head(Sat) is not monadic, Sat will follow its Core, which is its subject, has asymmetric scope over Head(Sat) and Comp(Sat), and, granted LCA, must precede them (i.e., in the case at hand, complex APs, PPs, and Clauses will follow the Core; monadic adjectives and participles will precede the Core and be adjacent to it). Finally, on the vertical axis, Op will be where its scope dictates (i.e., where Satisfaction licenses its construction with the Core), and, under the present analysis (cf. also Rijkhoff this volume), being a monadic predicate, it will always precede and be adjacent to the Core (HP) unless it is an affix and special morphological rules apply.

As to *Increasing Complexity* (“*ne decrescat oratio*”, another preference rule in FG, cf. Dik 1997a: 404), of course, insofar as it is a consequence of right-branching, it follows neatly: non-branching operators and satellites (monadic predicates) will precede the Core and all right-branching satellites will follow it. However, increasing complexity correlates with semantic weight and the need to add information, and to that extent it does not entirely follow from sheer geometry: if two or more APs, PPs, or Clauses, etc. concur, our principles predict only that all will be postnominal; whether one or the other linearly precedes depends on its relative scope. Of course scope is predictable, but there is no obvious correlation between scope and “length” = amount of information. A monosyllabic word may have widest scope (e.g., an article), and a longish clause may belong next to the head (a complement clause like *that she could marry someone else in the idea that she could marry someone else*). Thus, it is not reasonable to expect LCA, etc. to predict Increasing Complexity in full.

Finally, the orders due to *Pragmatic Highlighting* also follow from Satisfaction and LCA if it is assumed that a constituent may have additional attributes to satisfy which may force it to participate in *Secondary Construction* elsewhere. E.g., if a constituent C (with its semantic role, inflection, etc.) is additionally specified as Focus, Topic, (Subject), Relative, etc., the Satisfaction of such additional features may lead to processes of fronting, inversion, topicalization, extraposition, etc. of C (theory-neutral terminology intended). Incidentally, that constitutes the only violation of DI the present theory tolerates, i.e., systematic violations of DI due to “displacement” (theory-neutral term intended) triggered by Satisfaction. Thus, why “displacement” occurs is entirely explicable: a constituent C marked

as [D-F: Foc/Top], [CI-F: Subj], [CI-F: Rel],⁵² etc., is a Focus, Topic, Subject or Relator “of” some other constituent C’ (a Clause, an NP). Consequently, such features just may not be satisfiable until C’ has itself been built (recall that construction works bottom-up according to STC) and becomes available to C for the purposes of Satisfaction.

Of course, the positions assigned to Focus, Subject, etc. may vary, and displacement may not be involved in all cases (or the features may be realized by morphological means subject to other rules), but in some languages topics, foci, etc. are syntactic constituents and displacement occurs. In English, for example, Topic, Subject, and certain types of Focus (WH-Interrogative, Neg-Constituent Preposing, Adverb Preposing, etc.; theory-neutral versions intended) do involve fronting, whereas unmarked Focus is XP-final and does not. Can the present theory predict the position of Topic, Focus, Subject, etc. in English-like languages? Indeed: a constituent C marked as Topic, Focus, or Subject must satisfy its own attribute of topicality, focality, or subjecthood and also supply a value to the topic, focus or subject attribute of its clause, C’, which will until then contain an unvalued [A: v] pair [Top(C’): ?], [Foc(C’): ?], etc. However, by the time C can be constructed with C’, C’ is a right-branching structure, so C will immediately acquire asymmetric scope over everything inside C’, and, granted LCA, will precede it. Thus, in English, topics, subjects, and such foci will occur at the left edge of their clauses. Since Rel is also a clause feature, but one with wider scope than Subject, Focus, and Topic, a constituent marked [CI-F: Rel] must attach to C’ higher, but with the same consequence: XP[Rel] will immediately take asymmetric scope over everything inside C’ and, granted LCA, precede C’. These predictions are entirely correct, and the reasoning can be extended to account for comparable facts in many other languages.

Other theories invoke metaphors like Copying, or Movement in such cases, and various other devices (Slash, Unification, etc.) are available in HPSG, LFG, Categorical Grammar, etc.). FG must also provide some device to account for displacement phenomena, be it at the abstract level or in the expression module. I suggest that displacement must be a) a deep phenomenon, since it has semantic consequences, but b) with surface reflexes. As to how it should be conceived of, FG will have its aesthetic preferences, but in my view the appropriate concept is just (secondary) Construction, i.e., ultimately Satisfaction. Finally, as to the technical details, my broad suggestion is that phonological material be muted *in situ* and realized in the Topic, Focus, or Subject slot, where it can satisfy the clause-level feature

that supports the relevant semantic interpretation of the “displaced” constituent.

Needless to say, this is a very quick and schematic account of how semantic and syntactic computation work under Satisfaction, and empirical discussion is necessarily scarce and limited to SVO languages, but the essential point is this: Satisfaction, as specified, entails Dik’s STC, and all theories, including FG, assume that arguments are hierarchically ordered (= Priority, basic case), so all that it has been necessary to add to what FG tacitly assumes is LCA, or an expression rule to that effect. In other words, an extremely simple and general machinery that comes for (almost) free to FG will elegantly account for binary branching structure, the integrity of domains, predicability constraints, functional uniqueness, head update, correct scope relations, and correct surface order, including FG general ordering preferences, to the extent they are correct, as well as explanations for systematic exceptions to them. As to the semantic front, appropriate semantic types must be defined for heads, arguments, successively richer cores, satellites, and operators/modifiers, but FG, like all theories, has to do that anyway. The advantages of this approach, on the other hand, are substantial: no variables are involved in sense representations, no variable-binding problems arise, no vacuous quantification results, different intensions are automatically assigned for each core nominal as well as the top-most NP, correct scope follows, and, crucially, the various levels of term construction will be functionally integrated in the strongest possible way, *via* predicate-argument relations, and the sense and reference potential of the term will be compositionally determinable.

All that follows from STC (<Satisfaction) once it is properly spelled out as to what it presupposes and entails and its formal details are worked out; what has been done here is just to fill in the gaps and systematically extract the consequences of a particular interpretation of STC that is perfectly compatible with FG doctrine in all essential respects, in my view. On the contrary, abandoning Dik’s STC idea has directly caused, or at least allowed FG doctrines that cause, severe semantic and syntactic problems. I have already extensively referred to the former (cf. discussion of the inconsistency of variables, the variable-update issue, variable-binding problems, functional uniqueness, and general semantic disintegration above).

As to the syntactic problems, of course, in languages like English, loose multiply branching structures are blatantly incompatible with the results of all standard syntactic tests (e.g., coordination, zeugma, substitution by pro-forms, anaphora, mobility, etc.), and adopting them simply amounts to giving up on a precise formulation and explanation of processes like coor-

dination, substitution, anaphora, ellipsis, processes resting on asymmetric scope (binding of reflexives, interpretation of pronouns, etc.), predicability, the fine details of constituent order and its structural motivations, and explicit accounts of syntactic and semantic composition, in general. In the case at hand, the flat layers predicted inside FG NPs do not correspond to the structure that standard tests indicate exists, and make rules like NP-internal Coordination, One-Pronominalization, or NP-internal Ellipsis (theory-neutral interpretations intended) unstatable, apart from obscuring the transparent iconic relation that exists between the adicity and selection properties of lexical predicates and their patterns of occurrence in NP structures.

Arguing and illustrating all that in detail would entail repeating well known textbook level facts and require much more space than is available here, and, on the other hand, sadly, FG is not overly concerned with surface constituency, so I will in closing refer only to an aspect of NP structure with which the fullest FG account of NPs to date, Rijkhoff (2002), is deeply concerned, i.e., the surface order inside NPs. My main claim here will be that adopting DI, HP, and other standard FG principles unnecessarily diminishes the accuracy of the predictions that accounts like Rijkhoff's (2002) can make about English NPs.⁵³

4.3. Unnecessary weakness of current FG predictions on NP-internal surface order

One of the crucial concepts in Rijkhoff's account of NP-internal order is *Domain*, which in turn supports *Domain Integrity* (DI). Rijkhoff's (2002: 248-253) domains coincide with NP and clause, but NPs are too big and loose structures. A single NP may contain multiple elements that bear no semantic or syntactic relation to each other, and as a consequence DI is violated more often than it would be if domain were equivalent to the set of constituents in a given unique relation of Satisfaction. Observe that even elements of the nucleus of an NP, like two arguments of the same N, do not contract any semantic or syntactic relations with each other. Hence even the strict "nucleus" (noun plus arguments) is much too loose a construction to constitute a useful domain for the purposes of, say, effectively constraining surface order. Of course, beyond the nucleus, the "glue" is even thinner: a determiner or a modifier and an argument of the noun are unlikely to have any connection whatsoever, and the same holds of quantifiers and relative clauses, modifiers of different levels, operators, etc. As a conse-

quence of this loose concept of Domain, Rijkhoff, like Dik (1997a: 402), must state DI in a weak form, as another tendency, cf. Rijkhoff (2002: 250) and add stipulations such as that embedded domains “tend” to be peripheral and not intervene between A and N, for example. On the contrary, if our concept of domain above (= construction, based on Satisfaction) is adopted, DI is a categorical fact, although it need not be stated as a specific principle, since it follows from Satisfaction, and, in fact, the *Peripherality* of embedded domains needs no special provision, either, as, to the extent they are descriptively correct, both follow from the way Satisfaction and LCA work, as shown above, cf. Escribano (2004a, 2005a) for details (within a different theoretical framework).

As a consequence of his flat view of layers, Rijkhoff’s (2002: 313, 346) *Scope* principle, another key factor in his account of NP-internal order, is also much too weak, unsurprisingly, since it is a structure-dependent relation and must rest on FG’s weak view of construction and domains. In the version that applies to satellites (a particular case, presumably), Rijkhoff states it as follows: “Modifiers tend to occur next to the part of the expression that they have in their scope”. That statement has some moderate content⁵⁴ when only one modifier is present, but the fact that modifier and modified will be adjacent follows from Satisfaction, anyway (recall that the modified is an argument of the modifier), and, if there are more modifiers, Rijkhoff’s statement becomes vacuous.

Rijkhoff (2002) does not discuss adjective order, but, just as an exercise in theory-checking, think of a “flat” Quality layer and, say, four elements like Sat1, Sat2, Sat3, and Core, e.g., *strong thick red wine*. All four are in each other’s scope (cf. discussion *supra*), so Rijkhoff’s *Scope* predicts nothing useful about their respective order. It is compatible with results like, say, **strong red thick wine*, **red thick strong wine*, **red strong thick wine*, **thick wine strong red*, **red wine strong thick*, etc., as well as the desired *strong thick red wine*.⁵⁵ On the contrary, a process of construction under Satisfaction, as above, would enforce a binary branching structure, Sat1, Sat2, Sat3 and Core would contract asymmetric scope relations, and such ordering ambiguities would disappear, i.e., if, assuming delicate typing of adjectives, a structure [*strong+[thick+[red+wine]]*] results from lexical selection specifications and Satisfaction, clearly *strong* or *thick* will never occur between e.g. *red* and *wine*, and, granted LCA, outputs like **strong red wine thick* or **red wine strong thick* will just never be generated. Observe that *thick* has asymmetric scope over [*red+wine*], and must precede them, just as *strong* has asymmetric scope over [*thick+[red+wine]*]

and must precede them. In sum, there is much to be gained from adopting Satisfaction, strong Domain, and asymmetric Scope.

The third crucial factor of Rijkhoff's account of NP-internal order is *Head Proximity*, which, again, Rijkhoff (2002: 263-264) formulates weakly, as just another tendency (cf. also Dik 1997b: 402; Rijkhoff 2002: 264-290). This may be descriptively useful, but is theoretically inadequate in two respects: First, it does not give HP its due, predicting many more HP violations than actually occur, and second, it does not explain why HP systematically does not hold in certain cases. The first point is obvious: if layers are flat, HP is bound to suffer multiple violations; for instance, if a noun is simply preceded (or followed) by two modifiers (two As, two Ops, an A and an Op, two PPs, two Rel Clauses, etc.) in the same layer, and the structure is "flat", as in Mod1+Mod2+N, N+Mod2+Mod1, etc., the head of one of the modifiers and N will always violate HP. On the contrary, if such a nominal is built under Satisfaction, the relativized concept of Head, strong Domains, asymmetric scope, etc., the structures will be binary-branching, e.g., [Mod1+[COREMod2+[COREN]]], [[COREMod2+[COREN]]+Mod1], or [[[COREN]+Mod2]+Mod1], etc., order subject to LCA, as above, the relevant heads Mod2 and Core and Mod1 and Core will always be adjacent, and no HP violation will arise.⁵⁶

Other statements by Rijkhoff in relation to HP, i.e., that the head N is as close as possible to the head V of the clause, or his first HP-derived (negative) prediction that the preferred position of any adnominal modifier⁵⁷ is not between the head noun and the head of the clause (Rijkhoff 2002: 263, 290), are also inaccurate/incomplete and inferior to what Satisfaction, Priority and LCA predict. Observe that, if correct, in SVO languages like English, NP objects carrying pre-modifiers (cf. *I tried a strong thick red wine*) and NP subjects containing post-modifiers (cf. *The wine of Bordeaux that I ordered was sour*) would both be non-preferred options, whereas, in fact, they are not only perfectly normal, but the only options available, even if the post-modifiers are long branching APs, PPs or Rel Clauses. In general, in SVO languages like English we would expect modifiers to systematically occur on opposite sides of the core depending on the NP's function and distribution with respect to the higher head, i.e., if the core noun is the head of a subject, its accompanying modifiers should all be prenominal, whereas if it is the head of an object, they should all be postnominal, etc. Obviously, that is not the case, either in English or in related SVO languages, so a stronger statement is justified: there are perfectly systematic circumstances in which an adnominal modifier of a subject (e.g., a PP or Clause) must be exactly in Rijkhoff's supposedly non-

preferred position, between the head of the subject and the verb, and there are circumstances in which a satellite cannot be there, etc.

Rijkhoff's specific HP-derived PHP1, i.e., that attributive adjectives are expected next to the noun, sounds sensible, and yet, under scrutiny, sometimes it does not hold, and when it does it follows automatically from more general principles. The latter needs no comment: if the noun is an argument of the adjective, it must be constructed with it under Satisfaction. More interesting is why PHP1 is systematically false sometimes, i.e., precisely whenever the noun's and the adjective's own satisfaction interferes. For example, granted Satisfaction and Priority one or more arguments of the noun may have to be satisfied (cf. *a friend of mine*) before any adjectival modifier of the NP can in its turn be satisfied by taking the resulting NP as its subject. If the adjective itself is monadic, it will be constructed with that NP and will not only be adjacent to it, but, in English, granted LCA, it will necessarily precede it (cf. *a good friend of mine*, vs. **a friend of mine good*);⁵⁸ however, if the adjective is not monadic (cf. *familiar with university politics*), adjacency between the two heads is impossible, in violation of HP (which does not predict why this is so), i.e., the only possible output is *a friend of mine familiar with university politics*, cf. **a familiar friend of mine with university politics*, **a familiar friend with university politics of mine*, etc. Now, if PHP1 were an option open in all cases, it might make sense to include it in the grammar, but it is not, and with the right sort of theory we know why, so mentioning incomplete/inaccurate generalizations like PHP1 (or indeed HP itself, for the same reasons) is misleading and unnecessary.

Actually, there is more evidence that PHP1 is just a false/incomplete generalization and that more abstract and subtle principles than HP are involved. For example, HP/PHP1, like Satisfaction *cum* LCA, predict **a keen on semantics student*, but the unacceptable **a keen student on semantics* should in Rijkhoff's system be just one of the "small" and acceptable violations. Observe that the two heads are adjacent, as HP demands, and that the embedded NP domain is at the periphery, as DI and Increasing Complexity/Peripherality recommend. Of course, under the FG analysis, the head of *on semantics* is not yet adjacent to *keen*, against HP, so there is no other way to satisfy both HP demands but the configuration *a student keen on semantics*, which is exactly what Satisfaction, Priority and LCA also predict will happen. However, **a keen student on semantics* is not just mildly deviant, but unacceptable, so the violation is not a tolerable one in that case. Now, since what causes intolerable unacceptability in **a keen student on semantics* is the same kind of "minor" HP violation that occurs

in *a similar car to mine*, the latter should be as marginal as the former, but as a matter of fact it is not: the “split” AP *a similar car to mine* is quite acceptable in spite of its obvious HP violation. Of course, what causes the contrast between “split” APs like those in **a keen student on semantics* and *a similar car to mine* has little to do with HP/PHP1, and much to do with the adjectives’ argument structure and the way arguments must be satisfied, cf. Escribano (2005a).

Under the layered account of NPs, in short, HP counterexamples are bound to proliferate, and Rijkhoff (2002: 290, 308) must add that “small” ones (e.g., those caused by the piling up of non-branching APs) are tolerated better than large ones (those arising when embedded domains such as NPs or clauses intervene). Well, other reasons (LCA, etc.) prevent the embedding of XPs, as we saw, but, leaving that aside, the fact is that some such “small” HP violations are not tolerated at all. Ordering restrictions among adjectives of size, colour, etc. which Rijkhoff does not discuss have already been mentioned in this respect, so I will cite just a more interesting case that suggests a necessary revision of the status of prepositions in FG. PPs like *of Bordeaux* in *strong red wine of Bordeaux*, or *from Amsterdam* in *a new professor from Amsterdam*, being structurally as simple as adjectives in underlying FG representations⁵⁹ are under Rijkhoff’s assumptions predicted to cause only the same “small” HP violations as multiple adjectives cause. However, at least in English and similar languages, this is, interestingly, wrong when the violation occurs to the left of the noun: In pre-nominal position, an intervening surface PP is always much worse than an intervening noun or adjective on the left hand side of an NP. In fact, aside from idioms like *round-the-clock* in *a round-the-clock laundry service*, PPs are completely impossible in pre-nominal position, cf. **They gave us a strong of Bordeaux wine*, **A new from Amsterdam professor has arrived*; see Escribano (2004a). Unless a bare noun with a role feature qualifies as an “embedded domain” and such examples can somehow be excluded by DI, a dubious possibility, it follows that *ad hoc* expression principles would have to be added to HP, DI, etc., to allow some “small” HP violations (those involving bare adjectives and nouns, cf. *London in efficient London bus service*) and block others.⁶⁰

Observe, though, that this problem arises only as a specific consequence of the FG view of prepositions. Under broadly similar approaches to FG’s, like Hawkins’s (1994) account of constituent order in terms of an Early Immediate Constituents principle (EIC; “Minimize Domain”, in Hawkins 2001), it does not, as prepositions are there all along and count for the evaluation of internal complexity. The same can be said of our approach

here: if *of Bordeaux*, *from Amsterdam*, etc., are the result of combining a dyadic predicate *of*, *from* with its first argument to yield a monadic restrictor, both the semantics and the syntax of such satellites follow nicely from Satisfaction and LCA, i.e., *from Amsterdam* still needs a subject (e.g., *professor*), and the subject will have asymmetric scope over *from* and its internal argument *Amsterdam*, hence, granted LCA, *professor* will also have to precede *from Amsterdam* and such deviant examples will be neatly excluded. What this solution entails is an analytical distinction between freely selectable prepositions like *from*, on the one hand, and governed ones (e.g., *with* in *comply with*, etc.) or bare functional “case” affixes like *of* in *the arrival of the king*, etc. The latter may arise from late shallow features and expression rules, but since the former clearly add semantic content and their own arguments, they must be treated as dyadic predicates to all purposes (i.e., they must be available in the Fund). This is a small and well motivated modification which immediately brings a surprising number of facts under the maximally general and elegant principles of Satisfaction and LCA, cf. Escribano (2004a).

5. Conclusion

In sum, the relatively poor performance of current FG principles at predicting and explaining the fine details of NP-internal order is but another consequence of the general looseness of FG concepts that our previous discussion of variables, operators, scope, and NP semantics had already revealed. That looseness not only endangers the status of operators, variables, and other aspects of abstract structure, as shown above, but also largely invalidates FG’s results in surface syntax, e.g., Rijkhoff’s (2002: 313-335, 346) otherwise correct claim that important surface ordering facts follow from Scope. They certainly do, as we have tried to prove, if Scope is explicitly formulated. Unfortunately, FG’s recent strategy has not been one of tightening its formalization and exploiting the interaction of semantic and syntactic principles to simplify the grammar. On the contrary, Hengeveld has now apparently abandoned the idea of deriving order from meaning through structure and Scope. In Hengeveld and Mackenzie (2006: 675) order is said to follow, not from meaning, but from “independent morpho-syntactic principles” (?), and in Hengeveld et al. (2004: 45-46) the order of head and modifier is said to be fixed either by some templatic constraint, or by special indicators of the modification relation. Such an approach to surface order amounts to saying that it is not subject to any more general

principles than the low-level idiosyncrasies of individual constructions, but, unless what has been said above about Satisfaction, Priority and LCA is completely off the mark, that is an unnecessarily weak assumption. On the contrary, a precise theory of construction, structure, and scope, coupled with the optimal assumption of iconicity between abstract and visible structures, as regulated by LCA, yields most of surface order (cf. Kayne 1994; Ernst 2002; Escribano 2004a; 2005a).

Obviously, this comparison of the Satisfaction-based account and the standard FG one in the area of NP-internal order is not conclusive evidence that the former is neatly superior *tout court*. Before accepting such a conclusion, it would be necessary to compare the performance of the two approaches with respect to the rich cross-linguistic body of order facts in e.g. Rijkhoff (2002). However, an important difference should be underlined: in the Satisfaction-based solution, all surface order predictions are directly obtained as a result of elegant interactions between principles like Satisfaction, Priority, or the iconicity constraint LCA which also control all other aspects of construction. Granted such powerful principles, phenomena like DI or HP, to the extent they hold, clearly appear as mere consequences, or incomplete generalizations, which may be useful from a descriptive point of view, but fail to identify the real forces at play and lack explanatory power. Once the stronger principles are revealed, there is no point, in my view, in proposing intuitive principles that are known to be subject to categorical and completely predictable exceptions.

Most of the problems the FG account of NPs faces ultimately result from a vague theory of Construction. If Construction is made to depend on strong (and, at bottom, cross-theoretically assumed) principles like Satisfaction subject to Priority and rigid (iconic) correspondence between structure and linear order (Kayne's LCA), compositionality and proper integration between different elements of term structure are enforced, shallow differences like that between complementation and modification dissolve, cf. Escribano (2004a), correct constituency based on unambiguous binary-branching structures and rigid scope arises directly, FG principles like Iconic Ordering, Centripetal Orientation, Domain Integrity, Head Proximity and Increasing Complexity or even Pragmatic Highlighting (cf. Dik 1997a: 399-404, and Rijkhoff 2002) all follow as consequences of interactions among such more powerful forces, and their limits and systematic exceptions are elegantly explained, at least in English-like languages.

In the case of NP structure, the functional layers assumed in current FG can only arise if Dik's STC intuition and such strong principles are ignored, and are not likely to be correct. Although they constitute a good first

approximation to the most important functions of various tracts of NP construction and allow a moderately successful account of the surface order of determiners, quantifiers, heads, complements, and modifiers, the fine details do not quite fit. The higher referential and discourse layers probably do not exist as such, cf. footnote 16, (although NPs surely have such functions), whereas, on the contrary, many other layers (= constructions = domains = cores) are very likely to exist, for rather more delicate distinctions are needed both inside the nucleus of NPs and also above it, in the broad area of modification, cf. Keizer (2004) and Rijkhoff (this volume) for recent FG work in that direction, and e.g., Crisma (1993), Cinque (1994), Bernstein (1997), Longobardi (2001), Giusti (2002), Scott (2002), for relevant fine details from several well-studied languages. In fact, aside from conceptual considerations (only binary-branching structures may result from Satisfaction), there is substantial empirical evidence, derived from constituency tests, binding facts, surface order, and semantic facts, that points towards that kind of structure in NPs as well as clauses. For reasons of space, such evidence has not been cited here against FG's NP layers, but it is ubiquitous in both syntax textbooks and theoretical discussions of constituency (cf. e.g. Pesetsky 1995).

Certain minor syntactic details may also need rethinking in FG, e.g., the status of prepositions and relative operators. In my view, very nice consequences follow from assuming that prepositions with semantic import are *bona fide* dyadic predicates, with their complement and their "subject" (here, a nominal; in the case of adverbial PPs, a verbal phrase), cf. Escribano (2004a). As to relative operators, most of their properties can be coherently derived from the assumption that Rel is also a dyadic predicate with a special capacity to attract a clause-internal constituent, and that Rel (not the verb!) is the head of the Rel Clause. For one advantage, under such an analysis, "head proximity" between the head of the Rel Clause and the core nominal follows, and, particularly, the order of relative clauses with respect to other NP constituents in English-like languages follows nicely from scope and LCA, cf. Escribano (2003; 2004a).

The major focus of attention in this article, though, has been placed on semantic issues and problems derived from the representation of NP meanings in current FG, mainly two, a) the ontological status of what is meant, including the question of what symbols belong/do not belong in underlying NP representations, and b) the overall architecture of term formulae. As to the former, FG seems to operate with a broad concept of "meaning" that covers sense, reference, discourse use, and use in speech acts, which makes FG term representations very heterogeneous. Of course, it is perfectly le-

gitimate to design models of speech situations with various slots for participants and relations in them and treat linguistic expressions as values of appropriate variables in such models, or to connect NP senses with referents in e.g., models of Psych-W or Disc-W, but the theory must first show how the senses of NPs are compositionally derived, and how they constrain referential and use potential. Thus, in my view, it is necessary to be less ambitious and start from a more constrained concept of “meaning” as “sense”.⁶¹ NPs are used by speakers to refer to extralinguistic entities, but what they can name or describe is just entities of our Ling-W, i.e., senses.⁶² Therefore, only (symbols standing for) senses should figure in the representations on which the meaning = sense of NPs must be calculated (cf. Escribano 2004b; Keizer this volume). That excludes “R”, “X”, “intermediate X”, “small x”, the predicate variable “f” and the variables that Dik (1989: 50) envisaged for lexical items. What remains is a) the senses of nouns of various types (individuals, substances, events, etc.) and adicities (properties, relations), b) the senses of their arguments, also of various types (individuals, events, propositions) and adicities, and c) the senses of their modifiers (properties, also of different adicities and *n*th-order). The senses of predicates and arguments are composed by *Functional Application*, those of modifiers and their modifieds, i.e., higher-order predicates and the predicates that saturate them as their subjects, by *Functional Composition*, both essentially the same operation based on Satisfaction of sense-related properties (Types).

Operators are *n*th-order predicates, and no variables need be involved in the representation of senses,⁶³ since, by definition, a variable appears instead of a constant. Thus, the relation and composition of Ops and Cores is essentially the same as that of Sats and Cores, cf. Escribano (2004b, 2006), but a Sat and an Op never apply to the same core (*contra* Rijkhoff this volume and Hengeveld this volume). Under Satisfaction, the richness of the hierarchical structure inside NPs directly entails a highly differentiated structure in what concerns semantic Types, which has not yet been elaborated. As a real compositional account of NP semantics and syntax is worked out, new, more delicate semantic Types will emerge to be added to existing ones like “individuals”, “properties”, or “events” (and higher order properties of the latter two), cf. Pustejovsky (1995) for one such system, but that does not entail, and is not likely to determine, enrichments of the machinery in charge of construction (i.e., Satisfaction) and the monotonic computation of senses (Functional Application, Functional Composition), which is powerful enough, especially if semantic Co-Composition, cf. Pustejovsky (1995), is allowed along with (multiple) mutual satisfaction.

Of course, what specific types should be assigned to N and all the nominal cores above N is a matter that we cannot settle here, as it entails decisions on complex issues connected with morphological derivation and other matters, but, whatever semantic types are chosen, no variables need be involved in sense representations.

Needless to say, this quick overview just summarizes our previous diagnosis of conceptual problems in the area of term structure and adds bare guidelines, not detailed solutions to the substantive empirical issues that arise, e.g., Which Types (possibly different from metaphysical types of “real entities”) and Type-Shifting operations are relevant in natural language semantics? However, it hopefully will have shown that a clearer view of the implications of Satisfaction, rigid concepts of Construction, Domain and Scope, and a universal? expression principle like Kayne’s LCA that enforces a tight correspondence between structure and surface order are a prerequisite to any adequate account of NP form and sense. In fact, Dik’s idea of STC can be interpreted as an informal, but essentially correct, programmatic statement anticipating what is formally proposed here, and ideas like Satisfaction and Priority are already assumed *de facto* in FG. Also, since FG has also already developed a rich linguistic ontology (individuals, masses, ensembles, properties, events, etc.), further enrichment of its Type system, as proposed here, is a matter of degree, not a deep modification, and should not raise special difficulties. On the other hand, it is true that the changes defended here would make FG very different from what it is now as to the details, more rigid, and more vulnerable to empirical refutation, which is as should be from a methodological viewpoint.

As to the critical tone that has prevailed in this paper, in conclusion, although in its present form the FG treatment of underlying terms and surface NPs is seriously inconsistent in various respects, the problems are understandable, largely accidental (inherited from certain types of philosophical semantics) and by no means inherent to the theory, whose basic principles seem to me correct and fertile. FG should be further developed into a proper account of the way natural languages work, semantically and syntactically. For this reason, the ultimate purpose of this contribution has been critical, but in a well-meaning, constructive sense, i.e., to help FG scholars identify and perhaps solve some of their more pressing theoretical problems, acting from the vantage point of a sympathetic outsider with a broad view of linguistic theory, absolute academic freedom, and no axe to grind.

Notes

1. This work owes much to Daniel García Velasco, who facilitated my access to some crucial sources, read several versions of the text, and gave me wise advice on how to write a delicate piece like this, to Jan Rijkhoff, who, as an author, reacted very elegantly to my objections, and, as editor of this volume, offered very detailed criticism and suggestions, and to three other anonymous referees who discovered various obscurities and other infelicities in earlier versions that I hope have been avoided in this. I also thank Lachlan Mackenzie for quickly sending to me an offprint of an article of his that I needed. The underlying semantic research has been sponsored by the Spanish Ministry of Education under contract I+D MEC-04-HUM2004-018.
2. Interestingly, operators do not have them, which implies that they are not semantic categories under the authors' definition. This follows, presumably, from the fact that FG operators are considered grammatical modifiers. Yet, operators are "reflected in the language system" and obviously have semantic consequences. This view of Hengeveld and Mackenzie's echoes the Aristotelian concept of "categories" (= subjects and predicates), but leaves operators in a semantic limbo.
3. That is, those biuniquely bound by operators, cf. *infra*.
4. Assuming, in the spirit of Hengeveld and Mackenzie's *dictum* above (cf. semantic categories = ontological categories as reflected in the language system), that at the representational level FG semantics is, like all linguistic semantics, concerned with the sense of expressions, and that reference (to entities of psychological worlds in the case of FG, cf. Dik 1997a: 129) is, along strictly Fregean lines, a function of linguistic sense plus additional parameters.
5. Actually, a logical quantifier like $(\forall x)$ is conventionally assumed to vacuously apply also to formulae like $(\forall z) P(z)$ that contain no unbound variables, and even to formulae like $P(j)$ containing no variables at all. Thus, in Predicate Logic, $(\forall x) [(\forall z) P(z)]$ and $(\forall x) P(j)$ are not syntactically ill-formed, although the quantifier is semantically inert in them, but the motivation has to do with economy of statement of the syntax of Predicate Calculus. Natural languages, of course, radically disallow vacuous quantification (cf. **Every case, this theory does not handle such a case*).
6. Properly, predicates of higher adicity are often involved, but the various modifiers around a noun all behave as one-place predicates at the stage they are attached to their respective heads. Obviously, the head noun may itself be dyadic or even triadic (e.g., *love*, *order*, respectively) and modifiers may be headed by dyadic adjectives (e.g., *fond*, as in *a child fond of pets*), dyadic prepositions, participles of any adicity, etc.

7. The term “ontology” is polysemous. As used in this paper, it means an inventory (or closed system) of types of entities (e.g., individuals, substances, events, etc.) that can be linguistically named or described.
8. Such terminology is standard in logic textbooks and in all Fregean approaches to linguistic semantics, e.g., Montague Grammar, Categorical Grammar(s), LFG, GPSG-HPSG, and broadly “Chomskyan” works like Pustejovsky (1995) or Jackendoff (1990). McGee Wood (1993), Steedman (1993) and Heim and Kratzer (1998) contain clear and easy accounts for linguists.
9. E.g., representing *Bill met Quine at Harvard* as $(\exists e)$ [Meeting (e) & Agent (e, b) & Theme (e, q) & At (e, h) & Time (e, < now)], or *Whenever Tom sees Bill Bill is reading* as $(\forall e_1)$ [Seeing (e₁) & Experiencer (e₁, t) & Theme (e₁, b) & Time (e₁, t)] $\supset (\exists e_2)$ [Reading (e₂) & Agent (e₂, b) & Time (e₂, t)], etc.
10. Incidentally, a variable which is subject to exactly the same metaphysical difficulties posed by “x” above. The problem is a general one (of course, “f” is also affected).
11. Actually, on the negative side, Davidson’s approach also induced generative grammarians to counter-intuitively treat all constituents of the clause (i.e., the head predicate, the subject, objects, adjuncts, tense affixes, etc.) as predicates of events, suppressing the important differences between heads, arguments, and modifiers. Although any clause constituent adds its own truth conditions and to that extent can be thought of as a “restrictor”, the different status of heads, complements and adjuncts has wide-ranging syntactic and semantic consequences in natural languages and cannot be ignored in any structure-dependent account of senses. The problem is particularly acute in current Chomskyan linguistics as understood by Kayne and Cinque, cf. Escribano (2006).
12. In fact, apart from stacking, recursion is also involved, for events recur embedded inside more encompassing events. Davidson’s (1967) initial treatment did not recognize the fact that coexisting verb modifiers often do not even make sense if predicated of a unique event. For example, it has been well known since at least Dowty (1979) that whereas “in X time” adverbials modify telic VP’s (e.g., *composed this cantata in a week*), “for X time” adverbials modify atelic ones (e.g., *worked at Leipzig for years*), and, insofar as no verb sense can be both telic and atelic, we do not expect the two types of adverbials to co-occur. Yet, they do, and in either order (cf. *Bach composed cantatas in a week for several years*, *They jailed him for ten years in ten minutes*), which proves that even seemingly simple VP’s are endowed with elaborate internal event structure, and therefore we need not only stacked modifiers, but modifiers applying to different event structures embedded within a single VP, i.e., complex event structures and a hierarchy of types of events and properties of events.
13. Particularly since Dik (1989) and Hengeveld (1989) (cf., e.g., Keizer 1992, 2004; Rijkhoff 1992; Mackenzie 2004), Yet, the structure attributed to events

in current F(D)G is still pretty much Davidsonian. The formulae offered in current work as representations of expressions like *Sheila put the book on a shelf* are absolutely flat. There is no attempt to capture the fact that accomplishment verbs like *put* involve three different sub-events (state, change of state, causation thereof, plus an “action” tier), or that Location, Theme and Agent are constituents of different sub-events.

14. The ontology assumed in FG has, of course, expanded considerably since Dik (1978), and therefore neither predicates nor their arguments need any longer be first-order, as explicitly acknowledged in Dik (1989) and later work. Keizer (2004), Hengeveld and Mackenzie (fc.), and Mackenzie (2004) contain recent discussions of the various types of entities (properties, individuals, places, times, manners, states of affairs, propositions, speech acts), and their modifiers. In fact, the test for entity-hood is: whenever it is possible to refer anaphorically to or focus a WH-word on a constituent, its denotatum is an entity (cf. Hengeveld and Mackenzie fc.). Whether the ontological enrichment affects only the intralinguistic world of senses or also the “real world” is less obvious. Current pronouncements (e.g., in Hengeveld and Mackenzie 2006: 673) to the effect that properties have no independent existence and perhaps Dik’s (1997a: 131) distinction between “refer” and “designate” suggest that the extralinguistic ontology assumed in FG is still austere, but, be that as it may, its intralinguistic ontology is certainly rather rich (cf. *infra*). As the ontology that terms must name has expanded, parallel variables standing for properties (“f”), events (“e”), propositional contents (“p”), speech acts (E), etc., occur alternating with “x” in the structure of FG terms (cf. Dik 1989: 180-182; Keizer 1992 on properties; Hengeveld 1992a: 7, on speech acts; Dik 1997a: 131-132, 136-138; Dik 1997b: 93; Hengeveld 2004; Keizer 2004: 15-16; Hengeveld 2005, and Mackenzie 2004 for a good short overview). Although I discuss matters here mostly with reference to the “x” variable, parallel reasoning applies to “f”, “e”, etc.
15. To be fair, at least early FG seems to have simply inherited the problem from the logicians of the time. Russellian-Davidsonian accounts were just about all the semantic doctrine available on NPs in the early seventies, and it was natural for Dik to adopt their views. Although FG did not share the realist theory of meaning, he probably saw the mental world that FG terms refer to as sufficiently isomorphic to the “real” one to allow for a very similar semantic treatment.
16. Hengeveld (this volume) shares the view that proper nouns are not modifiable in the representational level, although they can be modified by predicates of the interpersonal level (cf. *Poor John!* Cf. also Butler this volume).
17. Whether the higher variable and its function can indeed be associated with a specific level of structure, rather than with a function of the full NP, is dubious, though. Rijkhoff’s (2002: 229-231) discourse “level” clearly does not correspond to any well-defined continuous stretch of structure at the top of

NPs, since discourse-oriented modifiers like *aforementioned*, *former*, *latter*, *same*, etc. must also often be structurally attached under the scope of Location operators and modifiers. And, as a matter of fact, the same applies to Hengeveld's interpersonal "level", which is supposed to contain the grammatical features that account for the referential vs. ascriptive use of NPs, but also other elements (e.g., Hengeveld this volume). Since articles crucially help the addressee construct or identify the referent, it seems *prima facie* reasonable to associate them with that level, but articles cannot be purely "non-representational" (i.e., pure referential operators) since they do also have predicative uses, and, to that extent, their association with the "interpersonal" level is questionable (cf. Keizer this volume). On the other hand, other interpersonally relevant items may occur fairly deeply embedded within NP structure (e.g. in *the poor fellow*, *poor* is visible at the interpersonal level but not at the representational level (cf. Hengeveld this volume). Thus, whether the grammar is top-down, as in current F(D)G, or bottom-up, as in classical FG, the interpersonal and discourse "levels" cannot be built either before or after the representationally relevant ones. Hence, calling reference/discourse-oriented properties of NPs a further "level" of term structure is somewhat misleading: such a structural level does not seem to exist (cf. Butler this volume). A hint in this respect is that, according to Hengeveld and Mackenzie (fc.), Hengeveld (this volume) the difference between referential and ascriptive NPs is just functional, not structural. If so, labelling referential tokens of NPs "RefP's" (cf. e.g. Hengeveld 2005; Hengeveld et al. 2004) is a category mistake. I suspect, though, that structural differences between referential and predicative NPs may well be indirectly detectable, as they are in languages like Spanish, and, anyway, Hengeveld and Mackenzie's (2006: 674) stronger view that nothing at the representational level distinguishes ascriptive from referential uses of NPs is implausible: after all, ascriptive NPs denote properties, whereas referential ones must denote individuals. Such a radical difference in sense is unlikely to be irrelevant precisely at the representational level (cf. also Keizer this volume).

18. Properly, in that passage, Rijkhoff's move, if taken literally, fits well with the metaphysical analysis of individuals in "substratum" theories, since it is an attempt to separate a substratum from the remaining intensional properties that may constitute an NP's sense.
19. Correspondingly, "small e", "small f", "small p", etc., as appropriate in NPs describing events, properties, propositional attitudes, etc. that Rijkhoff (2002) does not discuss.
20. Apart from "small x", Rijkhoff's account mentions predicate variables ($f_i \dots f_m$) ranging over senses, at N and the heads of all satellites. For reasons already stated, they play no role in the computation of senses or referents and will be ignored here, although their status will re-emerge briefly below in our discussion of operators and binding issues surrounding them.

21. Terms denoting other types of entities follow the same pattern, with different variables “e”, “p”, etc.
22. A further issue is what binds “small x”, cf. discussion of variable-binding problems *infra*.
23. Admittedly, this multiplicity holds only if individual competence matters. What view current FG adopts with respect to the internalism/externalism issue and the private language debates is unclear to me. If individual competence does not matter in F(D)G, there is a single, idealized “linguistic world”, the world of senses generated by each “langue” in the Saussurean sense. Be that as it may, Ling-W has a place of honour in linguistic semantics. For structural/internalist semanticists like Coseriu (1992) and Chomsky (2000), what we here call “Ling-W” is the only world that matters semantics-wise. As to F(D)G, to the extent the equation [semantically relevant entity] = [entity as construed by the language system] is assumed (cf. Dik 1997a: 129; Hengeveld and Mackenzie 2006), it is Ling-W, not really Psych-W, that is the core domain for semantics in FG too.
24. Dik (1997a: 131) restricts the concept of “reference” to the entities named by terms functioning as arguments or satellites in clause structure, but that must be an oversight, or an NP like *my office* inside another NP like *the key to my office* will not be a referential expression. Why predicates, predications, propositions and clauses do not “refer” to entities of type f, e, X and E, respectively, but merely “designate” them is unclear. It suggests that, as a metaphysician, Dik was a realist with an austere Phys-W ontology (just extensional individuals) and considered predicates, predications, etc. as names of Ling-W entities (mere senses), which would explain why he adds that designation is similar to constructive reference. However, the rest of Dik’s account does not make room for such an asymmetry between terms naming first-order entities and those naming higher order ones.
25. Correspondingly, “big E”, “big P” etc. would be nth-order variables bound by discourse-constructed events, facts, speech acts, etc.
26. Although Dik uses both terms, “stepwise” is a more accurate specification for what Dik has in mind and will be preferred here.
27. Of course, the truth conditions (but just the truth conditions) of a term like *the beautiful tall dark Indian girl* can, in principle, be adequately represented by a flat unordered conjunction of clauses like ... [girl(x) & Indian(x) & beautiful (x) & dark (x) & tall (x)], etc., but such a formula does not capture the fact that in natural languages head and modifiers are attached in a certain order that is subject to strict, near-categorical, constraints, cf. **the Indian tall beautiful dark girl*, **the dark Indian beautiful tall girl*, etc. Obviously, the grammarian can simply stipulate such constraints, but that is hardly an elegant strategy if it is possible to derive the order of modifiers from deeper principles, and Dik was clearly taking the latter course when he talked about “dynamic construction” and based his analysis of the contrast *Japanese buddhist / buddhist*

Japanese on predicability (hence, on the extension of the respective sets involved). Spelling out that idea a bit, the result is that predicates like *girl*, *beautiful*, *tall*, *dark*, *Indian*, etc., are predicable of different sets, but in that case the sets cannot be represented by the same variable “x”: simply, each of the tokens of “x” in the flat conjunctive formula above will have a different range; *ergo*, the various instances of “x” cannot be tokens of the same variable.

28. Observe that when several operators occur, they are not separated by “:”, i.e., they are not attached in the order of their left-to-right position in the formula. If they are stacked, as Dik (1989) claims, the determiner is predicted to have scope over the numerator, and the same result is obtained when, following Rijkhoff’s work, Dik (1997a: 163) associates each operator with a different layer of term structure. Of course, that would not yield a tree like (3), cf. *infra*.
29. See Reichenbach (1947: 87, footnote and ff.). Term-internal binding is the only formal difference that Dik (1997a: 168) acknowledges between FG quantifying operators and logical ones. Recall that Frege’s quantifiers are functions applying to propositional functions.
30. It also entails revising the status of all other “operators”, primarily clausal ones: a very significant rethinking of FG, all in all.
31. Arguably, there is a third. Number, definiteness, and case, in particular, are also said to extend their influence over the whole term on which they operate, cf. Dik (1989: 138-139, 317-319). A similar statement, generalized to all term operators, remains in Dik (1997a: 161), but must be a mistake there, for a few lines down on the same page Dik approvingly introduces Rijkhoff’s own account of term operators as associated with successive onion-like layers of term structure. That organization, of course, is strictly parallel to Dik’s own earlier treatment of clause-level operators as stacked around their operand, with inner operators falling within the scope of outer ones, cf. Dik (1989: 308), which is kept in the revised version of Dik (1997a). However, Dik’s early view of number, etc. is far from dead in FG. Bakker and Pfau (this volume) still insist on the idea that the number, definiteness, and case operators are “global” operators with scope over the whole term, although this may be due to a misreading of Dik’s text. Anyway, there are very good reasons why e.g. case must be a very “external” operator, cf. Escribano (2005c) and section 4 *infra*.
32. In (5), *from Amsterdam* will be higher or lower (hence “?”) depending on whether it is taken to be an identifying satellite (cf. Keizer 2004) or a qualifying one, but the point does not affect the argumentation here. I stick to the same example just for the purposes of comparing alternative structures. If, instead of *from Amsterdam*, the last restrictor were a relative clause like *who came earlier* or a discourse-level identifier like *already mentioned* they would definitely be attached as sisters to the operator *d*, as suggested.
33. Observe that although the left-to-right sequence of restrictors is meant to express increasing scope, if the same criterion is applied to the sequence *d* prox 3 we obtain exactly the converse of what Dik elsewhere claims to be the rela-

tive scope of d, prox, and 3: the numeral will have scope over prox, and prox over d. Assuming the contrary, the operators are themselves stacked onion-like with respect to the variable “x”, but that yields structures like (3), not (5). From Dik’s formulae nothing follows as to how operators are attached with respect to restrictors.

34. Just as variables must be uniquely bound by appropriate operators, arguments must be uniquely “bound” by, or “linked” to (terminology varies, but the differences are immaterial here) appropriate predicates, or they will not receive a semantic function and will be uninterpretable (just like uninterpreted variables in a calculus).
35. The standard view of scope, used in mathematics, formal logic, and formal linguistics, entails that if A and B are constructed into a new entity [A*B] (order irrelevant; * = whatever combinatory operation applies), then A and B have scope over each other, and that if [A*B], in its turn, is constructed with a new object C into a structure C*[A*B] or [A*B]*C, then C and [A*B] are in each other’s scope, C has asymmetric scope over A and B, and neither A nor B has scope over C.
36. Linear precedence is a non-starter, though, if the structure of terms is as claimed in current layered models and the aim is to say that e.g., satellites at L_n have scope over constituents of layer L_{n-1} , since when the onion-like term structure is projected onto a linear sequence, the head noun precedes (and therefore would have scope over) all post-nominal satellites.
37. Functional uniqueness entails that full NPs will not be further restricted: since the full NP must be able to receive a semantic function from the head it is constructed with (e.g., a verb), it cannot receive a second semantic function as subject of a restrictor. Hence, restrictors must operate at intermediate levels of term structure, and on categories different from the NP (i.e., not on the term variable “x”). It also follows from functional uniqueness that the same head nominal cannot be restricted by more than one restrictor, or by one restrictor and one operator, i.e., the structure cannot be “flat”; it must be strictly binary branching, as STC predicts. The head nominal is constantly “updated” as the construction of the term proceeds (cf. our discussion of the variable update issue above).
38. In what concerns the “f” variable, the problem has now been eliminated in Hengeveld (this volume), where each of the restrictors (e.g., *girl*, *intelligent*, in *the intelligent girl*) carries its own operator and its own variable. However, the higher Πx operator must still bind two instances of “x” which cannot stand for the same set (i.e., two different variables).
39. Note that, stepwise construction, according to Dik, is a deep property that affects the sense of terms, not just a surface realization detail. Actually, Dik even attributes psycholinguistic significance to it, for he claims that it reflects “die Art des Gegebenseins” (Frege’s phraseology), cf. Dik (1997a: 62, 135).

40. A has asymmetric scope over B if A has scope over B but B does not have scope over A, as in the structure A[B, C]. Sisters, on the contrary, have symmetric or mutual scope, as in structures like [A, B, C].
41. Obviously, this is consistent with grouping subsets of such “steps” of construction into as many functional “tracts” or “segments” as are appropriate to the nature and semantic contribution of operators and satellites, e.g., a term may have a discourse status segment (for (in)definiteness), a spatial location segment (for deictics), an ordinator segment (for *first*, *last*, etc.), a quantification segment, a modal segment (for modifiers like *possible* or *would-be*), and as many aspectual, qualification, classification, and complementation segments as need to be established. Current FG’s four or five layers have that functional character, but the functional distinctions so far established, especially in what concerns the intermediate and lower levels, are still too broad. Work like Scott (2002) reported in Cinque ed. (2002) suggests that a much more delicate layering of modifiers is justified, and recent proposals by FG scholars like Keizer (2004) and Rijkhoff (this volume) point in the same direction.
42. I will use “core”, Rijkhoff’s (this volume) term, to mean the head noun and all its successive expansions as a consequence of stepwise construction with satellites and operators, i.e., in the sense of lexical or phrasal head of the NP. If necessary, sub-indices can be used to unambiguously identify successively bigger nominal cores, as X-bar theories do with bar levels, following an idea due to Zellig S. Harris.
43. This is an important respect in which FG differs from other approaches that also explain facts, particularly constituent order facts, in terms of a compromise among competing “forces”. One such approach is Optimality Theory, cf. papers in Barbosa et al. eds. (1998). The difference is that in OT, the respective “strength” of each of the competing principles is explicitly defined in a hierarchy, the import of each type of violation is strictly quantified, and to that extent it is possible to predict which of the competing forces will prevail in cases of conflict. On the contrary, FG does not say anything on the relative strength of its ordering preferences, and it is impossible to predict what will happen, except ex post facto.
44. It is assumed here that theoretical linguistic objects are sets or matrices of [attribute: value] pairs. An [A: v] pair is a function that applies to its carrier and yields a unique value over a restricted domain. Informally, an [A: v] pair simply expresses a proposition about its carrier. All sorts of properties can be formalized with precision in that format, which is extensively used in grammars like LFG, GPSG, HPSG, Construction Grammar, and all related computational approaches. Traditional features like +F, used in FG and many other theories, correspond to a particular subset of [A: v] pairs whose value range is Boolean (+,-). Since many attributes necessary to characterize linguistic items are not Boolean, the [A: v] format is preferred, though. An attribute is “valued” when it has one of the legitimate values in its range. Unvalued attributes

leave their carriers as incalculable factors in computations, causing incompleteness or anomaly.

45. E.g., the construction of a verb, adjective or noun with its (PP) complement, or an adjective with its N(P), satisfies both an adicity requirement on the verb, noun or adjective, and a role value, and often a case value requirement, on the complement, and semanticists like Pustejovsky (1995) claim that co-composition (= bi-directional functional application) must be allowed.
46. Also Core+Argument, obviously; I ignore that case here, since the discussion is here focused on the structure of the upper FG layers at which Sats and Ops intervene. Arguments are also attached in a stepwise fashion, but I will ignore the details here, since I suspect in matters like event structure inside nominals there is little common ground across different linguistic schools and I want to keep this discussion as close to theory-neutral as possible.
47. In cases of predicate composition, one of the predicates functions as an argument of the other, i.e., one is type $\langle\langle e,t\rangle,\langle e,t\rangle\rangle$, a property of a property, (an n th order property), whereas the other is just $\langle e,t\rangle$ a property of order $n-1$.
48. This does not entail that a noun cannot in the earlier stages of term construction function as a predicate and take its own arguments, cf. Keizer (2004) for an FG view of this. What is claimed here is that even relational nouns, once saturated, form cores that function as arguments of satellites at higher levels, but cannot satisfy or be satisfied by another core nominal.
49. “Head of” is also a function that applies to its argument, in this case Sat. In the present context, head refers to the “ultimate” head of the satellite, i.e., a lexical predicate, usually an adjective, a semantically non-trivial preposition (cf. *infra*), a participle, etc.
50. There is a systematic but innocuous apparent exception: Since the subject is the last (sometimes called “external”) argument of its lexical head (but note that it is also the “subject of” a full predicate), the head of that predicate may have to satisfy a satellite of its own before satisfying its subject, since the semantic type of the object resulting after the head is completely satisfied would no longer be constructible with the satellite. This may happen only when the satellite does not alter the type of the head in a significant way, e.g., in cases like *a teacher extremely fond of children*, vs. **an extremely teacher fond of children*. In this example, *extremely* is a second-order degree property. It will therefore satisfy its argument if it combines with *fond of children*, a gradable monadic predicate, but will not match the type of the combination resulting when *fond* satisfies its subject, i.e., *teacher fond of children*, a non-gradable property. Hence, either the adverb is satisfied first or it remains unsatisfied, dangling, and the whole structure fails. Thus, Satisfaction forces a violation of Priority in this case, unsurprisingly, as Satisfaction is the real principle; Priority just defines how it is to be applied.
51. The facts of SOV languages are different, and must be accounted for by additional principles, either Kayne’s (i.e., Movement), or others, so HP may not be

so easily deduced and saved from counterexamples in all cases. In this programmatic conceptual discussion, I will limit attention to how much follows from Satisfaction in English-like languages.

52. It is assumed here that Focus and Topic are “discourse functions” and that the constituents that discharge them carry attributes like [D-F(unction): Top], etc., and that the Subject is a “clause function” (i.e., not just a VP-internal argument), hence, a subject will be marked as e.g. [Cl-F(unction): Subj]. In other theories the subject is treated as a special or “external” argument. Terminology is immaterial. What matters is that the subject is not just an argument of the verb, although it is also an argument of the verb.
53. Only a few suggestive examples can be discussed here, though. Further coverage of data is clearly impossible in the space available in this already long article, and well beyond my competence in what concerns non-IE languages. It follows that current FG concepts might still prove descriptively preferable to the present version in view of the facts of a large sample of languages like that used by Rijkhoff (2002), so I am well aware that the adequacy of either the proposal sketched here or FG as currently developed cannot be judged with reference to just English and similar languages. However, those few languages, especially English, happen to be those about which we know incomparably more, and to that extent it is understandable that the adequacy of theories be first of all judged with respect to how well they handle really well investigated problems known to a great majority of linguists. Also, under the assumption that Human Language is based on a unique universal computational mechanism (N.B.: such a mechanism need not be a “syntax”, and certainly is not an “autonomous” syntax) it is a reasonable strategy to adopt as a zero hypothesis that if an elegant set of principles makes really strong and delicate predictions in one or a few well investigated languages, such principles may well be appropriate to a wider domain. In this respect, the versions of Satisfaction, Priority, etc., presented above have a simplicity, coherence, and explanatory power (if only, in English and similar languages) that make them strong candidates worth comparing with alternative concepts. Obviously, it will be much easier to produce *prima facie* counterexamples to them than to HP, DI, etc. as currently understood in FG, since the theory argued for here is more rigid, but the real issue is to determine whether the apparent counterexamples are genuine and really invalidate the principles, or just follow from low level facts, or further principles of morphology, etc., without need to abandon the principles. Of course, that cannot be done here, so all I will do is compare the predictions of the theory above with those that follow from the standard FG concepts as developed mainly in Rijkhoff (2002) and Keizer (2004).
54. Even ignoring the hedge “tend”, Scope predicts that modifier A and modified B will usually be adjacent, but does not say in which order (A+B, B+A?) they will occur. As shown above, depending on the internal structure of A and B, it is possible to do rather better, cf. Escribano (2004a) for details.

55. Refining the layer structure, as in Keizer (2004) and Rijkhoff (this volume), may avoid piling up modifiers at a single layer and exclude some of the bad predictions, e.g., if one of the modifiers of *wine* is *Bordeaux* (*Burgundy*, *Rioja*, etc.), in Rijkhoff's five-layer model it might belong to the classifier layer, which would force it to at least be adjacent to the head, i.e., only orderings containing the sub-string *Bordeaux+wine* or the deviant **wine+Bordeaux* will be predicted.
56. Of course, the FG analysis of PPs and Rel Clauses automatically induces a violation of HP as soon as a PP or Rel Clause is attached to a noun, even if no other dependent intervenes, whereas under analyses that assume that P and Rel are heads no HP violations arise in such cases. There is strong evidence in support of the view that both unselected P and Rel are heads with their own arguments, cf. Escribano (2004a) on Ps and Escribano (2003) on Rel Clauses.
57. Rijkhoff (2002: 290, 308) directly excludes demonstratives, numerals, and adjectives from the scope of his HP constraint. The reason adduced is that such elements cause only minor violations that speakers can tolerate, whereas phrases cause intolerable processing difficulties. However, in the case of articles, demonstratives and numerals, there is no need to weaken HP: since they never take any other complements but the core nominal, appropriate layering and the relativized concept of Head above will immediately eliminate the false minor HP violation anyway. On the contrary, in the case of adjectives, which can take complements, it is a mistake to exempt them from HP, and the immediate consequence is that constructions like **a clever at chess boy* will be licensed (cf. *infra*). Of course, Rijkhoff (2002) does not really discuss complex phrases, which disguises this type of problem, but it is a problem.
58. There are a few exceptions, of course, cf. *the people present*, *the resources available*, etc., but they are not significant at this level of abstraction. See Escribano (2004a) and (2005a) for a possible explanation in terms of Focus.
59. Underlyingly, *of Bordeaux* is just a noun with a role feature; recall that prepositions are not predicates in FG and do not exist until the expression rules introduce them later.
60. One of the editors questions this analysis of *from Amsterdam* as "strange" within FG. Yet, as far as I know, there is no other FG analysis of such modifiers: if prepositions are inserted "late" as realizations of role features, in underlying structure that PP is just a noun with an Origin/Source? role feature. If the PP contained a full NP (e.g., *from the University of Amsterdam*), it could arguably be treated as an embedded domain and excluded as a "major" HP violation, but, as it stands, it is no different, in my view, from having adjectives or quantifiers intervening between determiners and nouns (the "minor" violations that Rijkhoff's account tolerates by excluding them from the scope of HP).
61. It need not be an impoverished concept of sense, though. Thus, the affective-interactive aspects of meaning that Butler (this volume) discusses need not be extraneous to Ling-W as here understood. This is obvious: to the extent

such features are expressible through linguistic means (i.e., not by means of gestures, musical resources, etc.), they belong to the language, are subject to its conventions, etc.

62. Keizer (this volume) defends the same view. Rijkhoff (this volume), and Hengeveld (this volume) emphasize the “descriptive” layers, but it remains unclear to me whether their NPs ultimately denote Ling-W entities, or rather extralinguistic ones. As to Butler’s “conceptual ontology”, his “content”, it rather seems to correspond to what we here call C-World, i.e., his meanings seem to be conceptual in a broad sense compatible with the existence of different modules to handle affective-interactional concepts and narrowly conceptual-representational structures. The approach defended here, on the contrary, is not conceptualist, but internalist, or language-immanentist, i.e., our semantics is the kind of semantics that Saussure, Hjelmslev, Coseriu, (and, when he is consistently internalist, Chomsky) had in mind. The mind with all its content is surely relevant to any explanation of performance, and to the integrated account of linguistic behaviour that FG aims at (cf. Dik 1989, Butler this volume), but linguistic expressions name what they can name, i.e., entities from Ling-W. The relation between Ling-W entities and C-World (and Phys-W) ones is altogether a different story, and a fascinating one.
63. In Escribano (2004b), I proposed eliminating variable-binding issues and the variable-update problem by limiting term operators and variables to a higher-order one at the top of the term structure. Of course, that was my compromise with the traditional FG assumption that all terms and clauses have an (Ωx) $[\Phi(x)]$ form. However, since Rijkhoff (this volume) now proposes to treat operators as modifiers and nearly dispenses with variables altogether, I feel free to propose here in an FG framework what I proposed in Escribano (this volume [written 2004]), in a broadly Chomskyan one. This proposal, therefore, differs from, and is incompatible with all those that maintain sense/term variables in the formulae of the representational component, e.g., Rijkhoff (this volume), Hengeveld (this volume) and Keizer (this volume).

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