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Author(s): John Collins

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Syntax, More or Less

JOHN COLLINS

Much of the best contemporary work in the philosophy of language and content makes appeal to the theories developed in generative syntax. In particular, there is a presumption that—at some level and in some way—the structures provided by syntactic theory mesh with or support our conception of content/linguistic meaning as grounded in our first-person understanding of our communicative speech acts. This paper will suggest that there is no such tight fit. Its claim will be that, if recent generative theories are on the right lines, *syntactic structure provides both too much and too little to serve as the structural partner for content*, at least as that notion is generally understood in philosophy. The paper will substantiate these claims by an assessment of the recent work of King, Stanley, and others.

1. Introduction

d-structure, s-structure, and LF just don't exist.

(Chomsky 2004, p. 152)

Conceptually, we don't want to seat interpretive motivations in the driver's seat of our syntactic car.

(Lasnik and Uriagereka 2005, p. 154)

Much of the best contemporary work in the philosophy of language and content makes appeal to the principles and results of generative syntactic theory. In particular, there is a presumption that—at some level and in some way—the structures specified by syntactic theory mesh with or support our conception of content/linguistic meaning as grounded in our first-person understanding of our communicative speech acts. This paper will suggest that there is no such tight fit. Its claim will be that, if recent generative theories are on the right lines, *syntactic structure provides both too much and too little to serve as the structural partner for content*, at least as that notion is generally understood in philosophy. This is not to suggest, of course, that syntax offers no structural contribution to linguistic meaning; the suggestion, rather, is that the contribution from syntax is by no means straightforward and is certainly not clearly reflected in the kind of structure that is familiarly taken to constitute propositional content. On this view, the philoso-

pher's content, as it were, is the result of a massive cognitive interaction effect as opposed to an isomorphic map onto syntactic structure. Pietroski (2005b), after Chomsky, has advanced some intriguing ideas in this area. My purpose here is not to say much at all about the positive side of this picture, but to argue for the negative side of the thesis—that syntax fails to match up with content in a principled way. This situation is not the least problematic for syntactic theory itself, for it is under no obligation whatsoever to offer any structure that may partner our intuitive notion of a proposition. Properly to establish this claim would involve, just as a preface, the Herculean task of surveying the relevant work in syntax, much of it unfamiliar to philosophers, and the many varied philosophical appeals to syntax made over the past forty years. Space demands a different method. The paper will focus on what I take to be the most sophisticated and philosophically fecund appeals to syntax—the work of Jeff King, Jason Stanley, and Zoltan Szabó—and show thereof that the mismatch between propositional and syntactic structure is, at the very least, cause for serious concern; that is, I shall invite you to generalize from the best cases.

Before we begin in earnest, some general points of orientation will be useful. First off, my targeting of King *et al.* should not be read as a suggestion that their work is peculiarly confused; quite the contrary. It is a virtue of their work that it may be confronted with research from a related field. Such vulnerability stands in happy contrast to nigh-on all of the 'metaphysics of content' literature, which, much to its detriment, eschews the very question of how content is linguistically encoded. Secondly, I take all relevant parties to be minimally naturalistic in the sense of holding to the principle that philosophical elaborations of content impose no a priori constraint on how syntactic theory will or should develop. Indeed, as we shall see, much of the attraction of generative syntax is precisely that it is perceived to have converged on the same point as the philosophers; the syntactic results, therefore, might be properly employed as *evidence* for the philosophical claims. Thirdly and much more prosaically, I shall take the philosophers at their word in their explicit commitments to current or the 'best' syntactic theory in the generative camp. Many of the questions I shall raise, however, are independent of particular syntactic frameworks, and if the picture looks bad for the philosophers under the framework I shall employ, it is far from obvious that it would look better under the numerous other extant frameworks. Besides, it is the responsibility of philosophers who make claims about syntax to confirm that it is as they assert. Fourthly, for present purposes, the syntactic principles to which I shall appeal

need not be correct. My claim is conditional: *if* current syntactic theory is on the right lines, then it provides too much and too little to support or structure content. I am simply presuming the truth of the antecedent. If my reasoning is valid, then the philosophers have but two options: radically alter their conception of content to fit the syntax, or—being minimally naturalistic—come up with a different syntax. Syntactic structure is not a mere excrescence for King, Stanley, *et al.*, it is integral to their claims; absent the syntax and their claims become indistinguishable from much of the ‘competition’.

Finally, the issue of the relation between natural language syntax and propositional structure has dominated much of philosophy for the past century or so. It could be said that modern philosophy of language was born in the realization that the structure of the proposition is essentially logical in some sense rather than linguistic, for natural language syntax appears to be ‘systematically misleading’ as to the meanings sentences express. Cutting a long and complex story short, the leading contemporary diagnosis of this traditional thought is that it laboured under a conception of syntax that was too much in the thrall of how sentences *appear* to be structured. By positing various ‘hidden’ levels of structure, generative linguistics can be understood to have at least established the possibility that meaning is indeed linguistically structured. In other words, the traditional error—the original, albeit very fruitful, sin—was to think that there is no more to syntax than the ‘surface’ organization of words. Besides, whatever one takes propositions to be, they are things that are expressed by sentences, so we do not want the pairing of the two to be ultimately mysterious. In a sense, then, my position is conservative just to the extent that I am arguing for a mismatch between linguistic and propositional structure, much as Frege and Russell argued. This, concord, however, might be misleading. I happily accept—indeed, insist upon—the broad generativist claim that syntactic structure is not exhausted by ‘surface structure’; furthermore, syntactic structure is open to empirical discovery and should constrain our semantic inquiries. In this light, while I accept the Frege-Russell conclusion that there is a mismatch, the consequence I draw is diametric to theirs. Far from natural language being relegated in our semantic inquiries, we need to think again about the theoretical salience of notions such as proposition and truth conditions. Put in radical terms, it is the very Frege-Russell conception of meaning as propositional (as truth conditions) that is at fault. Given the empirical mismatch and the need to explain how meanings are paired with structures, we should not take our inherited ideas to be a constraint on

our linguistic theorising. In short, as revealed by generative inquiry, it looks as if language narrowly construed is just not in the business of expressing propositions.

In the first part, I shall consider the claim that syntactic structure is the actual structure of content. In a series of articles and a recent book, King has argued that it is. My riposte will be that there is just far *too much* structure in the syntax for this to be plausible. If we keep syntax constant, the only avenue open to King and others will be to reshape content to fit the syntax. It will be argued that following this course has some unwelcome consequences, at least if one still wishes to trade in an idea of propositions.

The second part will assess the claims of Stanley and others that the syntax provides *covert variables to serve as the bearers for all* constituents of linguistic content. My riposte will be that the syntax provides *too little* structure for this to be true. In simple terms, the relevant variables appear to be absent from the syntax. So, if Stanley wishes to retain his semantics and have syntactic support for it, he must hold out for a different syntactic approach than the one that currently prevails in the generative field.

2. Too much syntax: the structure of the proposition

It is commonly assumed by philosophers of language that their ultimate business is an account of propositional content: the *p* of the schema

(PC) Sentence *S* means that *p*.

This conception is inherited from Frege and Russell and continues to animate contemporary work in the philosophy of language under the guise of capturing the *explicature* (the *what is said* of an utterance).

(EPC) Sentence *S* as uttered by *U* in context *C* expresses the proposition that *p*.

The orthodox view here is that the composition of *S* (inclusive of any explicit context-sensitive items) determines explicature, with other information effects being provided by *C* (e.g. Grice 1989; Soames 2002; Cappelen and Lepore 2005). My present concern is not with the many 'contextualist' rivals to this orthodoxy, but with an internal problem for the view, namely, that it faces the question of *what is the S such that it might determine p*. Some philosophers have turned to generative linguistics in the hope that it might shed light on the nature of proposi-

tions via an independent account of the nature of sentences. The attraction of generative linguistics might be elaborated as follows:

- (i) The proposition a sentence expresses (relative to context etc.) constitutes the sentence's meaning, which in turn determines the assignment of truth conditions.¹
- (ii) Propositions, however, pose a problem. The structure a sentence possesses such that it expresses the proposition it does is not encoded on the sentence's surface.
- (iii) If, then, we take a proposition to be made up of elements such that the sentence that expresses it is true just when those elements are structured so as to express the truth conditions of the sentence, we are left in search of what structure that is, for the 'surface' sentence does not provide it.
- (iv) Generative linguistics posits a distinct level of syntactic structure that encodes the structure of a sentence such that it expresses the proposition it does.
- (v) Therefore, syntactic theory promises to provide the structure of the proposition, for that structure is none other than the underlying form of a sentence such that it means what it means, that is, such that it possess the truth conditions it possesses. Thus, a proposition is the range of values of the lexical items of a syntactic structure that are organized just as the lexical items are.²

This position is most closely associated with Jeff King; we shall also see that Stanley shares much the same conception. Many others, less directly concerned with the ontology of propositions, also make the presumption that, at some level, syntactic structure encodes the organ-

¹ The phrasing here is intended to be ecumenical between various conceptions of the relations between sentences, propositions, and truth conditions. On one view, there is a two stage process, where a proposition is determined from a sentence (relative to context) and truth conditions are then assigned to the proposition (e.g. King 1995). This dual process allows for distinct processes of composition between proposition and truth conditions. For my purposes, this complication may be sidelined (for discussion, see King and Stanley 2005). As we shall see, the problem I present is one of an overdetermination of syntactic structure vis-à-vis propositional structure, not underdetermination between a sententially determined proposition and its truth conditions.

² Many 'traditionalists' would reject this reasoning *in toto*, claiming that there is no structural map between language and thought (cf. Fodor 2001; Schiffer 1987). For the purposes of this paper, I shall ignore any such position. For the record, however, I share the view of the theorists with whom I shall be concerned that natural language semantics is a serious pursuit and is not merely an indirect way of thinking about thought, whatever that might be.

ization of sentence meaning.³ The basic thought can be crudely depicted as follows:

- (1)a. Bill sleeps
- b. <Bill, property of sleeping>
- c. [_S [_{NP} Bill] [_{VP} sleeps]]

In (1)a, we have a simple sentence of monadic predication that expresses the proposition that Bill sleeps, which, as depicted in (1)b, involves just the two constituents, Bill and the property of sleeping. The question is: how are these two constituents structured so that (1)a gets to express (1)b? How, in other words, are we to understand the angled brackets and comma? (1)c provides the answer: the structure of the proposition just is the structure of the sentence. So, the proposition expressed is that the object *Bill* refers to—Bill himself, as it were—stands in a relation of instantiation to the property *sleeps* expresses—the property of sleeping. The syntactic structure, then, simply provides the structure to determine the required relation of instantiation to save the putative proposition from merely being a list or realising a weird structure wholly divorced from how we understand the sentence in (1)a.

My substantive complaint against this proposal is that syntax provides far too much structure, much more than can be accommodated as the values that determine a sentence's truth conditions, the proposition it expresses. This, further, is not a mere quirk of some constructions: the surplus structure is generally exhibited and reflects the fact that current syntactic theory is not in the business of providing structures that answer to philosophical conceptions of propositions. That, at least, is my charge. Before substantiating it, a little excursus on syntax will be useful.

The proposal under discussion presumes that syntactic theory arrives at the same point, from a different direction, as the philosopher is aiming at. Yet the history of generative linguistics presents a somewhat different picture. It is, for example, simply false that generative linguistics has always posited a level of meaning-relevant syntactic structure—

³ See King 1994, 1995, 1996, 2002, 2007; King and Stanley 2005. This work explicitly appeals to propositions. What of those, such as Harman (1974), Higginbotham (1986), Larson and Ludlow (1993), and Larson and Segal (1995), among others, who favour (interpreted) LF structures as entering into the semantics of '... believes ...' and '... means ...'? I think the considerations below also pose complications for such accounts. Certainly such accounts, as so far presented, make no sense on the phase derivation model of syntax (see n. 4), but perhaps they can be somehow reconstructed. The issues are highly complex and deserve a separate treatment that current space precludes. (I thank Gabe Segal (p.c.) for some clarification here.)

'logical form' or 'propositional structure'. Chomsky's (1955–56/75, 1957) earliest theories had *no level of structure that uniquely encoded all that was relevant to 'meaning'*. Instead, the grammars provided a T(ransformational)-marker that detailed the derivational 'history' of a structure; the properties relevant to meaning were distributed across the marker. In the early 1970s, *two levels of structure were posited that determined meaning-relevant properties* (see Chomsky 1972 and Jackendoff 1972). In the present day, Chomsky and many other syntacticians eschew any level of logical form; instead they propose theories in which, in a manner similar to that of the earliest theories, the structure relevant to meaning is distributed across a derivational history of *phases*.⁴ My point here is not to suggest that there is anything essentially wrong with a level of syntax that encodes 'meaning', but only to point out that while some generative theories have posited a unique level of logical form, many others have not. Hence, there is nothing about the generative enterprise as such that constitutively informs the philosopher's search for propositional structure. Perhaps the 'best' syntactic theory should answer the philosopher's demands, but there is little empirical reason to think so and some empirical reasons to think not.

To continue the excursus with a technical point: philosophers often elide many aspects of syntactic structure in their presentations. For example, no one serious in the field believes that the structure of even a simple sentence like (1)a is accurately represented by (1)c; (1)c is inadequate as regards both its architecture and its labelling. Two simple points will suffice for our purposes. Firstly, the notion of a sentence ('S') has been effectively redundant since the early 1970s for good empirical and theoretical reasons. For instance, as will be noted, the structure in (1)c is exocentric, that is, it is marked as an S without its S-ness, as it were, being determined by any of its constituents. This

⁴ For the development of level-free approaches, see Chomsky 2000a, 2001, 2004, 2005a, 2005b; Epstein *et al.* 1998; Epstein 1999; Epstein and Seely 2002, 2006; Uriagereka 1999, 2002. On such approaches, the syntax inputs to 'semantics' in cascades or phases, where at phase $n+1$, the structure transferred to semantics at phase n is no longer accessible to operations or *probes* within the syntax (a *Phase Impenetrability Condition* holds). Chomsky's working assumption is that CP and vP (with full thematic assignment, thus not unaccusatives and passives) are phases, where the complement domains (i.e. minus edges: SPEC-head configurations) of each are transferred, i.e. TP and VP respectively. See the references above for disagreements over the size of phases. One may still speak of 'LF' on such approaches, although the notion simply designates an interface between syntax and semantics, not a level of representation defined by its satisfaction of unique conditions; e.g. one can no longer speak of LF as where binding theory holds or where QR applies, and so on. It bears noting that the level-free approach, and minimalism generally, is not a recent quirk. Considerations of economy have been central to syntactic theory from the beginning, even if they have not always shaped the theoretical developments. Minimalism is, in essence, simply an approach which puts such considerations to the forefront of research. See Collins 2001; Chomsky 1998; and Martin and Uriagereka 2000, for general discussions of economy.

makes a sentence categorically distinct from a verb phrase, or a noun phrase, or a prepositional phrase, and so on—these are all endocentric, that is, they are x phrases because they contain a constituent that is an x , which is said to ‘head’ the phrase. Empirically, furthermore, syntactic structure looks to be nigh-on uniformly endocentric. We should like, therefore, for the integrity a sentence has to flow from its constituents; we do not want it to be a ‘top down’ a priori stipulation that just certain structures will be sentences. Descriptively, there is no harm in the stipulation, but we fall into the explanatory red if we take the shorthand seriously. Secondly and correlatively, just as S is assumed, so tense and agreement are ignored. Again, just for shorthand, no harm is done, but when we include tense and inflectional properties we see the trouble for S double. Not only is S floating above the structure as a stipulation, but the tense/inflectional features do not project, that is, it is as if they were merely members of a list rather than an integrated part of the structure. The obvious solution, and the one adopted, is to ditch the idea of a sentence and let tense/inflection project and be the head of what we think of as a sentence, just as, for example, it is the verb *hit* that determines that *hit the ball* is a verb phrase rather than a sentence or a noun phrase. As we shall see, it is precisely this well motivated move in linguistics that causes trouble for the philosophers.

From what has just been said, we might imagine the structure of (1)a to be (2) (where ‘TP’ is a tense phrase and ‘ ϕ ’ is the tense head being ‘spelt out’ as $-s$ on the verb):

(2) [_{TP} Bill [_{T'} ϕ [_{VP} sleep]]]

(2) *appears* to give us everything we want for the proposition, including tense, yet it remains inadequate. According to theories of the past fifteen years or so, *every* full clausal structure involves some *movement* or *copying*, which produces a surplus of elements relative to property instantiation of the kind apparently required for propositional structure. Rather than merely stipulate this principle or go through the detailed syntactic argument, let us see if we can motivate the basic idea.

Consider the following three theses that are widely held in contemporary syntactic theory.⁵

⁵ See, for example, Lasnik and Uriagereka 2005 for a textbook account of these claims. The basic idea that any sentence-like structure (featuring tense) will involve movement in fact follows from the verb-internal subject hypothesis that has been discussed since the early 1980s. For the classic defence of the hypothesis, see Koopman and Sportiche 1991; for an overview of the hypothesis, see McClosky 1997. See den Dikken 2006 for the hypothesis in a minimalist setting.

- (TA) Verbal theta assignment takes place in verbal domains.⁶
- (TEN) Tense heads are merged post the formation of verbal domains and carry uninterpretable features.
- (MC) All uninterpretable features must be valued either by a matching of features or by movement/copying.

(It is important to note that my argument does not require each of these precise theses to be true; minimally, I only require syntactic ‘surplus’ from the perspective of propositions. For example, in their different ways, Kratzer (1996), Hornstein (2001), Borer (2005), and others reject (TA) in favour of at least some arguments receiving their roles outside of verbal domains, but, as we shall see, the arguments still move and so create the problematic copies.) We can briefly explain these ideas by way of our simple example.

First assume that each lexical item (roughly, word) carries inherent features and unvalued features. The former are unaffected by syntactic operations (Merge) that build structures. The latter are affected; they acquire a definite value or content only when combined with other items. It turns out that this distinction closely matches the distinction between interpretable features (semantically significant), which are inherent, and uninterpretable features (semantically empty, but potentially morphologically significant), which are unvalued. Assume this holds. In our example, then, the verb *sleep* assigns a single theta role, that is, it requires an item to be interpreted as the *agent* of the sleeping. According to (TA), this role can only be assigned to items that are dominated by the VP that is headed by *sleep*. (2) contradicts (TA), for *Bill* is not in the domain of the VP. Let the derivation of the structure of (2), then, start with *Bill* being merged with *sleep* from which it acquires its theta role. Now we need tense, T, which merges with the VP. According to (TEN), however, T carries uninterpretable features that need to be valued relative to other items (e.g. agreement features of number, person). Indeed, *Bill* carries such a feature (Case). According to (CH), such features must be valued if the structure is to be interpretable at the semantic interface and marked for morphological purposes (in other words, valuing a feature relative to other features is a way of marking the item as only interpretable (potentially) morphologically, not semantically). If we have a match between (unvalued) uninterpretable

⁶ Theta roles are features constituents of a structure have in relation to verbs and other theta-assigning items, such as prepositions. For example, in *Mary hit Bill with a bat*, *Mary* is the *agent*, *Bill* is the *patient* (the thing affected), and *the bat* is the *instrument*. For our purposes, the details of theta roles and their assignment may be happily sidelined.

features on T and interpretable features on *Bill*, we can think of the later as cancelling out the former (valuation is a way of marking an item as only of morphological significance); thus, the uninterpretable agreement features of person and number on T are cancelled by such interpretable features (3rd person, singular) that are part of *Bill* (we can assume here that Case on *Bill* is reflexively valued as well, marked as nominative for morphology, if required, as in *he* rather than *him*). But we have not finished yet, for T in English is *strong*, that is, it attracts items into its domain in order to value a remaining uninterpretable feature (so-called ‘EPP’, a mere name for we know not what⁷). To make all this somewhat clearer, we have the following derivation (where +/- mark interpretable and uninterpretable features respectively and underlining marks the valuing of uninterpretable features; the usual abbreviations mark how morphology treats the items):

(3)a. Bill sleeps

- b. [VP Bill_{+1st, +sing, -Case} sleep]
- c. [T’_{+Pres, -3rd Per, -Sing Num, -EPP} [VP Bill_{+3rd, +sing, -Nom} sleep]]
- d. [TP Bill_{+3rd, +sing, -Nom} [T’_{+Pres, -3rd Per, -Sing Num, -EPP} [VP <Bill_{+3rd, +sing, -Nom}> sleep]]]

There are two crucial things to note here. Firstly, the derivation is driven to value uninterpretable features, not to provide a propositional structure. This motive is semantic in nature, for an uninterpretable feature is precisely a feature that has no semantic significance, but the structure produced departs from what we have been imagining is required for propositional structure. This is the second point. The copying of *Bill* higher up the structure creates an item that looks to be surplus relative to property instantiation; that is, the lower copy of *Bill* exhausts what propositional structure appears to be interested in, namely, *Bill* is fixed as the agent of *sleep*, the instantiator of the property of sleeping.⁸ Further, after the introduction of tense, we have a temporal dimension. Still, *Bill* gets copied for a reason that is not recorded in the proposition,

⁷ A common idea is that EPP is in fact reducible to Case valuation. This would be a nice result, but would create complications for the simple picture I am presenting here. For our purposes, the reason for the copying is much less important than the fact that the copying appears to lack the ‘right’ semantic significance.

⁸ Propositions might feature the same object twice, if we are dealing with a reflexive relation (e.g. suicide). My present point is that the syntax appears to demand at least a pair of copies independently of the conceptual adicity or reflexivness of the verb.

as it were (perhaps Case valuation or EPP elimination). Of course, *per* the first point, this is no problem or mystery at all from the perspective of the syntax. The syntax operates to value uninterpretable features, and copying is part of that mechanism, a displacement of items to meet interface demands, not to create a propositional structure.

I have chosen this most simple of examples to show that the relevant properties hold generally. A more complex example, however, will perhaps make the philosophical points here less opaque. Consider a classic raising construction (features and other properties of the derivation are elided for expository convenience):

- (4)a. Bill seems to love Mary
- b. [_{VP} love Mary]
 - c. [_v v [_{VP} love Mary]]
 - d. [_v v+love [_{VP} <love> Mary]]
 - e. [_{VP} Bill [_v v+love [_{VP} <love> Mary]]]
 - f. [_{T'} to [_{VP} Bill [_v v+love [_{VP} <love> Mary]]]]
 - g. [_{TP} Bill [_{T'} to [_{VP} <Bill> [_v v+love [_{VP} <love> Mary]]]]]
 - h. [_{VP} seem [_{TP} Bill [_{T'} to [_{VP} <Bill> [_v v+love [_{VP} <love> Mary]]]]]]]
 - i. [_{T'} φ [_{VP} seem [_{TP} Bill [_{T'} to [_{VP} <Bill> [_v v+love [_{VP} <love> Mary]]]]]]]]]
 - j. [_{TP} Bill [_{T'} φ [_{VP} seem [_{TP} <Bill> [_{T'} to [_{VP} <Bill> [_v v+love [_{VP} <love> Mary]]]]]]]]]

Again, we see an item *Bill* copied (this time twice) so as to value uninterpretable features on functional heads—in order: infinite T *to* and finite T *-s*. Semantically, such structure appears not to be required, for the predicate *seem* appears to express the character of an experience that *Bill loves Mary* (technically, *seem* assigns just the single theta role to its TP complement; *Bill* does not *do* any seeming). The confirmation of this is given by the paraphrase *It seems that Bill loves Mary*, where the subject of *seem* is pleonastic and *Bill* is the agent of *love*.

The details of these constructions and movement/copying in general are quite complex, but a generalization can be readily extracted: copying occurs after everything relevant to a propositional (truth conditional) structure has been determined. But copying creates new structure, and so there is more structure than appears to be demanded

by the requirements of the encoding of structure relevant to propositions.⁹ Again, the crucial point is that, according to recent theory, copying is motivated by semantic requirements, that is, the valuing of uninterpretable features on functional heads, such as Tense. So, all of the structure is playing a semantic role in one sense, just not the sense apparently required for the encoding of propositions.¹⁰ It also bears emphasis that the problematic copying is not a mere notational artefact. If the reasoning I have sketched is correct, then all valuing occurs in certain 'local' environments; thus, copying is simply a mechanism to ensure that items stand in the right local relations to have their features valued and so be acceptable at the interpretive interfaces. Put crudely, an item cannot be in two localities at the same time unless it is copied.

I do not take the above reasoning to signal a refutation of any particular thesis. The apparent mismatch of structure, however, poses a serious challenge to philosophers minded to take syntax to encode propositional structure. It is far from obvious that any match can be properly elaborated. Indeed, as mentioned, Chomsky's recent work and those of many others renders the dream null and void, as the syntax proceeds in phases, with no level of structure designed just to encode meaning relevant relations, much as the earliest theories proceeded. Let us now look at some options how one might accommodate the syntactic surplus.

2.1 King on syntax

King does not explicitly address the challenge posed above, but he has made some recent remarks that might be taken to ameliorate the problem.

⁹This situation also threatens Stanley and Williamson's (2001) analysis of 'know how'. They argue that 'know how' is a species of 'know that' in part on the basis that the infinite complements of complementiser *how* are propositional due to the presence of covert PRO in subject position (see below for more on PRO). As far as it goes, this thought is accurate, but infinite complements, as we are seeing, contain structure that looks redundant if the complement is simply taken to encode or reflect propositions. For example:

- (i) Bill knows [_{CP} how [_{TP} PRO to [_{VP} <PRO> swim]]]
- (ii) Bill knows [_{CP} how [_{TP} PRO to [_{vP} <PRO> v+ride [_{VP} <ride> a bike]]]]

¹⁰Chomsky's (2001, 2004, 2005a, 2005b, 2007) speculation that semantic interpretation has a dual interface would establish a 'functional' semantic role (independent of the syntax that operates blindly) for every element of a structure, most typically an operator-variable relation (especially see Chomsky 2007, p. 12). Serious complications abound. The semantic role of the later structure is speaker/discourse-information orientated; further, the proposal is made in a level-free setting, under which there would be no unitary proposition structured by the syntax, for there would be no unitary structure. This, note, makes perfect sense if the later structure serves to situate the earlier structure relative to speaker/discourse-information.

King (2007, p. 47, n. 44) writes: '[syntactic] structure is likely considerably more complex than I have represented it as being ... I don't think this affects issues that concern me.' In the absence of an account of the nature of the additional complexity, however, it is impossible to judge whether the affect will be negligible or disastrous. I am suggesting the affect is somewhere towards the latter pole, for King does present syntax as realizing an instantiation function in the simple way presented above. If the syntactic structure of even the simplest sentences involves structure via copying that goes beyond the instantiation required of a proposition, then syntactic structure is always in surplus as regards propositional structure. That is the problem.

King's sanguinity is explainable, I think, by remarks he makes on the nature of syntactic relations:

If we assume ... that any two objects that are combined by Merge stand in the same syntactic relation R, this means that in a given tree all we will ever have are pairs of expressions standing in R ... This means that to give an account of what sentential relations are, all we need to do is say what R is. (King 2007, p. 48)

King (2007, p. 48, n. 45) notes that in now standard treatments, syntactic movement/copying is construed as a species of Merge ('Internal Merge'), so, again, copying or movement in itself does not raise any additional worries for the very idea of syntactic structure doubling as propositional structure. I think King's reasoning is correct as far as it goes, and he is certainly correct to think that the ultimate question of what syntactic relations really are need not be settled before we can constructively employ the notion (King 2007, pp. 49–50). None of this, however, speaks to my concerns. King (2007, p. 47) is concerned to answer a Benacerraf-style problem of the overdetermination of propositional structure by the potential myriad of ways of understanding syntactic structure. That is not my concern. However we ultimately understand the notion of a syntactic relation, if current theory is on the right lines, then syntactic structure realizes relations between items that go beyond what propositions require. King's reasoning, as presented, can only be an answer to *this* problem if his hypothesis is merely that propositional structure realizes the same *kind* of relations as syntax realizes. Yet that is not King's claim, for he explicitly holds that LF representations map onto structured propositions (King 2007, p. 29); indeed, they are 'identical' save for a surplus on the *propositional* side to accommodate the same syntactic organization realising distinct instantiation functions (King 2007, p. 38). Thus, King's hypothesis is not merely that the same *kind* of relations are realized in language and



propositions, but that the very syntactic structure language realizes is recruited to structure propositions, that is, the proposition a sentence expresses (relative to context) is structured by the very sentence (as represented at LF). Thus, our problem of too much syntax remains, for there is a fundamental mismatch between the two structures, regardless of precisely what we take syntactic relations to be and regardless of the correct observation that movement/copying is just another species of Merge.

2.2 Biting the bullet

We have so far been assuming a fixed intuitive conception of what propositions must be like if they are to play the role philosophers envisage for them. For some, at least, this might be an unwarranted assumption. In a naturalistic spirit, the bullet might be bit; that is, granting that syntactic theory provides too much structure to fit into the common notion of a proposition, we might be enjoined to let the notion of a proposition float until anchored to our 'best' conception of the syntax. The advice here would be to elaborate propositions on the basis of our syntactic findings as opposed to seek to constrain syntax by way of our inchoate intuitions as to what propositions must be. Thus, our argument would be accepted in full, but its impact stymied, for we have assumed a fixed notion of a proposition independent of syntax (cf. King 2007, pp. 100–1). There are two problems with this gambit, one motivational, the other empirical.

The very idea of a proposition brings with it a conception of truth conditions—a proposition determines truth conditions, either directly or by way of further composition (see n. 1)—and abstractness, that is, propositions can be shared and communicated (Frege's 'common store of thoughts').¹¹ If these minimal criteria are surrendered, then it becomes opaque why one should talk about propositions. The position becomes indistinguishable from a version of sententialism or, more generally, propositional eliminativism. After all, I take it that the issue is not merely semantic, a dispute about how to use the word 'proposition'. The crucial aspect of the above considerations is precisely that the first criterion mentioned is seriously threatened: if propositions are structured by syntax, then propositions appear to contain elements that play no role in the determination of truth conditions. Any bullet can be bitten, but if this one is, I cease to understand the position.

¹¹ One might take propositions to determine less than truth conditions, but such a complication does not by itself ameliorate the present problem, where we have too much structure, not too little (cf. Soames's, 2005, change of mind from his 2002).



One can imagine various options to make the bullet more palatable. One thought might be that only one aspect of the syntactic structure goes to structure the proposition, the rest is somehow peripheral.¹² This is an old demarcation. Certain aspects of a structure, such as theta assignment, are determined initially, that is, no item is copied so as to get a theta role, while other aspects, more discourse related, are determined later or by copying, such as scope, topic, focus, and elements of mood (see n. 10). This is one reason for the positing of levels within syntax. But a syntactic derivation is not complete until a structure is built that does *not* correspond to a structure apt to encode a proposition. Again, from the point of view of the syntax, ~~the task is to value the features that have mere morphological significance; the task is not to deliver a structure for a proposition.~~ I do not wish to be dogmatic here. Syntax, as currently understood, does contain sub-structures that look like propositional structures. The problem is to isolate them in a principled way—that is, to define a structure that is complete relative to syntactic demands *and* the putative demands of propositional structure. Such a demarcation is what looks to be unavailable. The problem is exacerbated in the recent *phase derivation* model. Here, uninterpretable features are valued in, following Chomsky's proposal, something like a 'propositional' structure (Chomsky 2000a, 2001), but it is not the propositional structure itself that becomes interpretable, it remains uninterpretable; it is only the complement of the phase head that becomes interpretable. For example, if we look at a derivation of *Bill loves Mary* at the stage [_{VP} Bill love [_{VP} <love> Mary]], we have a 'propositional' structure (a phase) with all theta roles assigned, but it remains 'incomplete' relative to interface demands: inter alia, the uninterpretable Case of *Bill* remains unvalued. So, in effect, the only thing the derivation has finished with is [_{VP} love Mary], which, needless to say, is not the structure of a proposition. We have perhaps already ventured too far into technicalities. The basic idea that syntactic structure has an inherent duality, with one aspect dealing with propositional structure, is not a

¹² A natural way of substantiating this idea would be to take propositions to be the values of a function from S^*+X , where ' S^* ' is a partial aspect of the syntactic structure and ' X ' is the other ingredient. The problem here is to say what goes into ' S^* ' without merely imposing a top-down condition from our inchoate notion of a proposition, for such a move would apparently exclude interpretable copies that satisfy all syntactic and interface demands.

There is a principled argument for an operation of 'deletion' of all but the highest copy based on the need for linearization at PF (see Nunes 2004). Such reasoning, however, does not translate into a reason for deletion at LF, or its equivalent, at least not straightforwardly. Firstly, lower copies are interpretable at the 'semantic' interface, even if theta roles are featural rather than configurational. Secondly and correlatively, because linearization does not apply at the 'semantic' interface, there is an absence of an independent reason to view any process of deletion as applying at LF.

daft idea at all; it is probably true. The present point is simply that how this duality appears to play out precludes the recovery of a level of syntax that codes just for propositional structure, at least if that notion is intimate with that of truth conditions.

Biting the bullet also has empirical consequences. On the proposal envisaged, we should let the notion of a proposition float until anchored to some settled syntactic account. Yet, without getting into any technicalities at all, there is a problem here. The proposal cuts propositions as finely as syntax, but there are surface ‘transformations’ that preserve something we might wish to call synonymy, although discourse factors change, for example:

Passives: Bill kicked the ball – The ball was kicked by Bill

Expletives: A fly is in my soup – There is a fly in my soup

Clefts: Bill wants a car – What Bill wants is a car – It is a car that Bill wants

Raising: Bill appears to be tired – It appears that Bill is tired

Prima facie, discourse effects apart, such ‘transformations’ do not affect meaning. On reflection, it is easy to see that there are indefinite ways of ‘saying the same thing’ with distinct syntactic structures. Intuitively, an account of propositions should furnish what is shared across the structures. If, however, propositions are cut as finely as the syntax, we lose anything that could be shared. It bears emphasis that this is an empirical problem. The data are that competent English speakers understand the pairs above as ‘saying the same thing’, and it is this intuition (inter alia) that the notion of a proposition is meant to capture.

The fineness of cut problem is not new. My present worries, however, do not turn on translation between languages or propositional attitude embeddings, both of which typically animate propositional cut worries (cf. King 2007, pp. 95–101). My concern is that the data appear to tell us that syntactic constituency has parameters of freedom that are independent of the determination of propositions (the same truth conditions). Thus, tying propositions so intimately to syntax is misleading regarding both the nature of syntax and the higher level determination of truth conditions. The appropriate inquiry, it seems, is to determine the parameters of freedom within the syntax that preserve a notion of ‘same meaning’ (modulo discourse factors). For this inquiry, we precisely do not want propositions individuated by syntax; the idea of such a proposition would be a supererogatory abstraction.

2.3 Scope, variables, and reconstruction

Part of the attraction of syntax for the philosopher of language is that it promises to account for scope phenomena, which surface structure alone notoriously fails to capture, at least in the absence of much gerrymandering; indeed, the syntactic level of LF was principally posited to account for such data (Chomsky 1975, 1977; May 1977, 1985). Here at last appears to be a case where syntactic difference marches in step with propositional difference. Consider this simple example:

- (5)a. Everyone loves someone
 b. (every person x)(some person y)(x loves y)
 c. [_{TP} [_{DP} Everyone]_i [_{TP} [_{DP} someone]_j [_{TP} t_i loves t_j]]]
 d. (some person y)(every person x)(x loves y)
 e. [_{TP} [_{DP} someone]_j [_{TP} [_{DP} everyone]_i [_{TP} t_i loves t_j]]]

The syntactic structures c and e reflect the respective interpretations of ambiguous a given in b and d. As things stand here, we have movement, but not a surplus of syntax: the moved DPs are read as variable binding operators with the variables being the *traces*— t —left behind in the launch sites of the movements.

The operation (Quantifier Raising—QR) that moves the DPs in this case was precisely designed to capture the semantic operator-variable readings given in b. and d. It is for this reason that QR is very suspicious from a purely syntactic point of view¹³ (a more faithful rendering, on the lines witnessed earlier, would see the subject DP copied from an internal SPEC- ν P position to a SPEC-TP position, which here is occupied by t_i). Let us put such qualms aside. For present purposes, I am happy to let the structures structure the corresponding propositions (see King 2007 for a detailed account of how this might go). After all, QR was devised to capture scope construal rather than narrow syntactic demands of feature valuation. The question is: Is it plausible to construe all cases of copying as creating structure relevant to the encoding of propositions? The idea here might be to take a proposition to be determined from what we might call a ‘full-reconstruction’, where every copy plays a propositional role. There are many technical issues here,

¹³ For suspicions over QR, see Hornstein 1995; Kitahara 1996; Uriagereka 1998; but see also Fox 2000, 2003.

which I shall sideline.¹⁴ Let us, though, consider the proposal in rough outline to see how far it might take us.

It is certainly true that intermediate copies have scopal and binding effects. Consider, for example, the following case:

- (6) Bill seems to her [*<Bill> to appear to X [*<Bill> to <Bill> like Mary*]] (X = himself, her, *herself, *him)*

Here we see that, where X = himself, the intermediate copy in the subject position of *to appear* acts as a clause-mate antecedent, thus ruling out X = herself. Thus, we may construe the highest copy as an operator and all lower copies as bound items (cf. Chomsky 2007). This produces a propositional form along the following lines:

- (7) Bill is such that it seems to her that Bill appears to himself to be such a Bill that is a Bill that likes Mary

Here we have four instances of *Bill* to match the four copies of *Bill* in (6). Similarly, our simple example of *Bill sleeps* should be rendered as, say, *Bill is a Bill that sleeps*. Questions might be raised about the fidelity of such forms to the original ‘meanings’, but, in such cases at least, let us accept them as accurate full-reconstructions. Two problems face this general strategy, one conceptual, one empirical.

The conceptual problem is one suggested above. Were we to follow this line, we would radically depart from any intuitive idea of a proposition that encodes truth conditions, or something as simple as, say, Bill instantiating the property of sleeping. Now this might be a price many would happily pay, but then they land in the problems above that face the ‘empirical’ idea of a proposition. Perhaps all the reconstructions show us is that all copies *in this case* must be interpretable at the semantic interface; why should we conclude from that that a proposition is thus structured? Indeed, if the phase derivation model is accepted, then the syntax will not deliver the structure of a proposition even in the case of (6), for the VP *like Mary*, will be spelt out once the phase [_{VP} Bill like Mary] is constructed. This problem will hold for all structures that feature copying through phases. At best, then, the proposal will give us the structure of a proposition for non-phase including TP complements of phase CP. It bears emphasis here that this does not affect the

¹⁴The status of reconstruction is very much a live one. Suffice it to say that issues turn on which copies should be interpretable and which should be, in some sense, deleted; thus, there is broad consensus that low A-bar copies are uninterpretable. The status of A-copies is much less certain (e.g. Chomsky 1995b; Boeckx 2001). In general, therefore, the idea that a proposition is going to be reconstructed from the full set of copies looks to be a non-starter.

general point intimated just above that the reconstruction of copying simply shows the interpretability of copies.¹⁵

The empirical problem is that it is difficult to see how the proposal would work for all cases of copying. The idea, in effect, is to construe the proposition expressed by a given sentence as the proposition expressed by the syntactic structure of the reconstruction of the given sentence. Yet not all instances of copying have reconstructions.

Consider verb raising, which is widely believed to hold for all transitive verbs.

(8)a. Bill loves Mary

b. [_{TP} Bill [_{T'} ϕ [_{VP} <Bill> [_v v+<love> [_{VP} love Mary]]]]]

The problem here is to say what the semantic relation is between the copies of *love*. It is not difficult, of course, to find paraphrases of (8)a that feature two instances of *Bill* and *love*. The problem is to relate the copies of *love* so that they might realize a semantic relation analogous to an operator-variable relation that might be underwritten by the syntax. For example, *Bill is a Bill that loves the loving of Mary* gets the meaning wrong. Worse, *Bill is a Bill that loves and the loved is Mary* (or, say, ... *the loving is of Mary*) just signals a surrender of the idea that syntax is to be a guide to the proposition—that is, while the meaning is in some sense captured, the reconstruction has, in effect, the verb *love* agreeing with the DPs, as if there were two distinct relations in the syntax, one for *Bill* and one for *Mary*. It might be that the syntax works in this manner, distinguishing internal and external arguments as monadic predicates of the same event, but the proposition appears to involve just the one dyadic relation. The problem becomes more vivid if we look at ergative/causative verbs, such as *break*, *sink*, *boil*, etc. With such verbs in transitive form, it is plausible to take the copied verb as expressing causation:

(9)a. Bill boiled the soup

b. Bill caused-to-boil the soup

c. [_{TP} Bill [_{T'} ϕ [_{VP} <Bill> [_v v+boil [_{VP} <boil> the soup]]]]]

¹⁵The general strategy of taking lower copies to be bound items of a higher operator within a phase depends upon the integrity of chains as syntactic objects legible at the SEM interface. Although chains are widely assumed, it is far from obvious that chains in general should be sanctioned; see Epstein and Seely 2006. A rejection of chains would, in effect, be a rejection of any reading of copies as related as operator to a variable.

In such cases, we precisely do not want the originally merged verb showing up in the proposition, as it were, for that, again, would give us the wrong meaning: *Bill is a Bill that boiled the boiling of the soup*. Of course, the interesting fact about such verbs is that the original copy remains interpretable:

(10)a. The soup boiled

b. [_{TP} The soup [_{T'} ϕ [_{VP} boil <the soup>]]]

(Thus, we take the verb *boil* to be the same item in both constructions that assigns a theme role to *the soup*; *boil* acquires the causative reading via copying to the functional *v* head, this option being necessary, if there is another nominal available to serve as the agent.¹⁶)

There are many other instances of movement/copying, including topicalization, passives, unaccusatives, the full range of interrogatives, focus constructions, relative clause formation, ‘tough movement’, quantifier floating. Indeed, the problem looks insuperable if we take projection itself to be copying, as in bare phrase structure. Correlatively, if labels are required, perhaps for purposes of extension and linearization, then they too must be rendered semantically interpretable. To account for all of these varied structures in terms of some form of reconstruction, which does indeed appear to work in some cases for argument copying to an operator position, is an onerous task. I should not want to say that the task is impossible, but I suspect that once the complexity of the problem is clearly in view, the likely return on the effort will be seen to be not worth the outlay.

As an addendum, it is worth noting the difference between traces and copies. Traces look amenable to a variable construal, for they are not proper lexical items; they merely mark a position in a relation (a so-called ‘chain’). Copies are different; they are genuine lexical items with a full set of features—a copy is not simply a variable. From the point of view of the syntax, copies are much to be preferred. Traces must be added to a derivation, but it is opaque from whence they might come. It appears that they are simply posited to account for movement. Copies, on the other hand, come for free. The lexical items are already there and the copying of them is too in so far as they are copied from the lexicon

¹⁶ See Pietroski 2003 for an interesting account of the semantics of causatives in line with the presented syntax. More generally, Pietroski (2005a) has presented a framework where lexical items are interpreted as monadic predicates of event variables. The structures, however, clearly do not support the linguistically structured proposition idea; the structures are, rather, an interpretation the syntax provides that treats Merge as conjunction.

as a token of a type. Again, we reach a familiar conclusion: syntax appears not to care about propositions.

Throughout this section, I have argued that syntactic structure is out of step with the structure that appears to be required for propositional structure: there is too much syntax. This is not the least surprising or disturbing if we do not begin with the thought that syntax must answer to what philosophy or intuition otherwise mandates. On the feature-valuation model I sketched, syntax *is* driven by semantic constraints, but from the point of view of the syntax, this might be seen as a sorting out of what belongs purely to morphology and what is of semantic significance. This syntactic sorting appears not to respect the notion of a proposition. As things stand, therefore, we have no good reason to think that syntax, as currently understood, provides the structure for propositions.

3. Too little syntax: the case of covert variables

In a series of recent articles, Jason Stanley has proposed and defended the thesis that *all* elements of a proposition (*explicature*) are encoded as values of the lexical items (or encoded via syntactic operations on such items) of the sentence that expresses the proposition (relative to context).¹⁷ In Stanley's words, 'all truth-conditional effects of extra-linguistic context can be traced to logical form' (2000, p. 391). Stanley's thesis has attracted not a little criticism.¹⁸ Almost all of the extant responses, however, leave unmolested Stanley's syntactic assumptions; their focus is firmly on his semantic claims.¹⁹ Further, the responses variously defend some species of pragmatism, or otherwise question whether *all* 'truth conditional effects' can be accommodated as values of lexical items. In short, Stanley's semantics is questioned. In contrast, my claim will be that syntax, at least as currently conceived within the generative

¹⁷ Stanley 2000, 2002a, 2002b, 2005; Stanley and Szabó 2000a, 2000b. Stanley's thesis amounts to the denial of *unarticulated constituents*: constituents of a proposition that are not expressed by any lexical item of the sentence that express the proposition (see Perry 1986; Crimmins and Perry 1989). Taylor (2001) also appeals to covert variables in the syntax to account for putative contextual effects on content. Unlike Stanley, however, he neglects to offer any syntactic argument for his thesis.

¹⁸ From Carston (2002a, 2002b), Breheny (2002), Bach (2000), Neale (2000, 2004, 2005), Recanati (2002, 2004), Cappelen and Lepore (2002, 2005a, 2005b), Elgardo and Stainton (2004), Lepore (2004), and Pagin (2005).

¹⁹ Breheny (2002, p. 183), Recanati (2004, p. 108, n. 19), and Cappelen and Lepore (2005b, Ch. 6) express some doubt about the *justification* for (not the *truth* of) Stanley's syntactic assumptions, although neither press the issue. To my knowledge, only Blair (2005) questions Stanley on syntax, in relation to cross-over effects.

tradition, provides too little for this thesis to be true. I should emphasize that my arguments are not intended to lend weight to the positions of Stanley's critics; indeed, Stanley is to be commended for taking seriously the syntactic constraints on semantic interpretation, which are highly problematic for 'contextualist' positions. My complaint is only that such constraints do not play out in favour of Stanley's positive view.

Stanley does not present a full-dress account of logical form; rather, he simply appeals to 'correct syntactic theory' (2000, p. 397), and assumes that such a validated theory will (i) vindicate a level of logical form and (ii) support the presence of items at logical form that may serve as vehicles for 'all truth-conditional effects of extra-linguistic context'. Stanley (2002b, 2005) does acknowledge, of course, that this thesis about syntax can not be beamed directly from semantics, as it were: some syntactic motivation must be provided for the claim that the relevant items occur at 'logical form'. It is this syntactic argumentation that will be my focus.

This relatively narrow approach might seem to leave me open to a simple polemical rejoinder: 'So much the worse for generative theory!' My intent, however, as expressed in my introductory remarks, is not to categorically refute Stanley's thesis and his attendant semantic account. My contention is conditional: *if* recent generative theory is correct, Stanley's account is incorrect, or at least lacks the support he claims for it, for there is too little syntax. The imagined rejoinder, then, is at least coherent, but it has little impact. It is crucial for Stanley's account and his general anti-'contextualist' stance that *some* syntactic theory *does* actively support his account, and he assumes that the generative approach does so; as we shall see, he is perfectly explicit in this regard. So, one committed to a Stanley-style approach to the relevant semantic phenomena is free to pick any syntactic account that coheres with it, but, if I am right, recent generative theory is not such an account.

Further, for present purposes, it might well be that Stanley has the semantics right; my concern is only to cast doubt on his syntactic assumptions. To repeat, this is not a marginal issue, but one that goes to the heart of Stanley's approach and those of many others. Stanley (2000, p. 413, 2005, p. 235, n. 8) readily admits that variables of the kind he posits are often appealed to in the semantics literature; what distinguishes his position is precisely the thought that such variables are 'syntactically represented'. If Stanley's position has no support from syntactic theory, then the values of his posited variables would be *de facto* unarticulated constituents. But Stanley's very claim is that there

are no unarticulated constituents. It follows that Stanley's thesis is, in part, a syntactic one; its syntactic assumptions are precisely what distinguishes it from the competition (cf. Cappelen and Lepore's (2005b, pp. 8–9) geography of 'moderate contextualism').

3.1 Stanley on context and logical form

Stanley's thesis is that 'all truth-conditional effects of extra-linguistic context can be traced to logical form' (2000, p. 391), where 'logical form' is a level of syntactic structure underlying surface form postulated by 'correct syntactic theory' (2000, p. 397). Stanley discusses a range of data to support his thesis; the focus of the sequel will be on what I take to be the best case for Stanley, namely, quantifier domain restriction and binding more generally, although many of the points to be raised are intended to have a general import.²⁰

The phenomenon of domain restriction is that quantifier DPs are typically read as being restricted to a domain narrower than is lexically determined by the complement nominal. Since the restriction is not lexically encoded, it *seems* that it must be provided non-lexically, such as by a pragmatic process of 'enrichment' or a determination of 'communicative relevance', and that just means that not every item of a proposition will be encoded at LF. Consider:

- (11)a. Every student got 100%
- b. Every student answered every question
- c. Every student answered every question *on her exam*

A typical statement of (11)a does not communicate the thought that every student in the universe got 100%. The subject DP is typically restricted to cover just some contextually salient domain (e.g. every student in one's class). Likewise, the object DP of (11)b clearly does not typically cover *every* question; again, the domain is restricted. We may assume that (11)c expresses the typical explicature of an utterance of (11)b. The propositional adjunct in (11)c, however, is not lexically

²⁰The following considerations also critically bear on other theorists; for example, King (2001) suggests that where a bare demonstrative forms the subject of a clause, then it has an empty nominal complement; thus, e.g. [_{TP} [_{DP} That ψ] [_S is tall]]. This posit is required to support King's general account of demonstratives as 2-place quantifiers (in fact, his theory posits two more places, but these are not lexically realized). There is, however, no clear syntactic reason for such a phonologically null item outside of ellipsis. Further, it would appear that demonstrative determiners (*that, these, those*) along with many other kinds, although not all, carry the right ϕ -features (3rd person, singular/plural-number, perhaps null Case) to match and value uninterpretable features of T (their external Merge or raising from ν P eliminates the EPP feature of T). Thus, syntactically, it is not obvious that there is a nominal apart from ellipsis. See Longobardi 2001 and Chomsky 2001.

encoded in (11)b. Without further ado, therefore, it appears that the explicature does not compositionally devolve onto the constituent lexical items of the sentence that expresses it.

Although disarmingly quick, I think that such reasoning is inescapable *if* we are working with ‘surface form’ alone (i.e. if we recognize no items in a linguistic structure other than those that are phonologically realized), and *if* we want (11)b to express (11)c as its explicature (relative to context C). As remarked above, however, the received understanding in generative linguistics is that phonological realization does not exhaust the items of a linguistic structure. Indeed, the very idea of ‘logical form’ in the sense Stanley intends enshrines the claim that ‘surface form’ is not the level at which (all) features determinant of semantic interpretation are encoded. The above quick argument is thus incomplete at best. Indeed, we might take it to militate for the hypothesis that ‘surface form’ is not the level at which semantic interpretation applies. Such is Stanley’s approach. He contends that, when we consider ‘logical form’, we may readily postulate phonologically null items that serve to realize the propositional constituents unexpressed at the surface.

Stanley claims that, at ‘logical form’, the ‘object’ DP in (11)b contains a covert function-variable complex that is ‘associated’ with the nominal question, or ‘*co-habits* a node with it’, where the argument variable is bound by the subject DP *every student*, and the function on the argument is determined by context (perhaps a speaker’s intention), in this case a function from individuals (students) to exam questions.²¹ In general, we have the following schematic proposals, where (12)a gives the semantics for the schematic syntax in (12)b, which enables the binding relation displayed in (12)c:

$$(12)a. \quad \mathbf{D} [\langle \mathbf{N}, f(j) \rangle]_c (\mathbf{B}) = \mathbf{D} [(\mathbf{N} \cap \{x: x \in c(f)(c(i)) \}), \{\mathbf{B}\}]$$

$$b. \quad [_{\mathbf{XP}} [_{\mathbf{DP}} [\mathbf{D} \mathbf{N} + f(j)]] \dots]$$

$$c. \quad [[\text{Every} \langle \text{student}, f(j) \rangle]_i [\text{answered} [\text{every question } f(i)]]]$$

The restriction on a Det **D** is the intersection of the set determined by the lexical nominal complement and the set determined by the values the functional complex takes relative to context *c*. Where not bound, as in (11)a, the variable complex takes contextually determined values, which explains typical domain restriction. That there is such a variable

²¹ In certain respects, the proposal is a development of Partee’s (1989) model of implicit variables. Partee, however, is very cautious indeed about her proposal (especially see pp. 267–8). Further, her semantic model is DRS, and, in general, she seeks no support from a notion of ‘logical form’.

complex is supported by the fact that it can be bound, as apparently is the case in (11)a. If there were no item in the syntax, then there would be nothing to be bound, and the bound readings would thus be impossible, or at least anomalous. So goes the thought.

Thus, relative to the context we are imagining, (11)b gets to express the proposition expressed by (11)c, whose structure is schematized in (12)c, because the object DP has its otherwise contextually fixed variable *i* bound by the higher DP *every student*. So, Stanley's point here is not that context does not determine truth conditions, but rather that it does so only in (roughly) the same way as it determines indexical values. That is, context determines values of elements of the structure that take variable values, but context does not introduce new elements that are not encoded syntactically, either lexically or as a consequence of a syntactic operation such as movement. Let us assume that the existence of such covert variable complexes would serve to provide the right values to deflect the contextualists' radical pragmatics construal of the 'underdetermination' data. Indeed, let us further assume that no extant pragmatic process could secure the bound reading of (11)b. The question still remains whether there are such items at 'logical form'.

Stanley offers two general considerations in support of the existential claim. I shall present them and raise doubts about each in turn. Then, in the following section, we shall see that his underlying assumptions about syntax are at odds with recent generative theory.

3.2 Stanley's syntactic arguments

Stanley (2000, p. 412) appeals to an 'innocent ... presupposition':

for explicit quantifier expressions, within a clause, semantic and syntactic binding coincide. That is, bound readings within a clause are due to the existence of a variable binding operator standing in a certain structural relationship to a co-indexed variable in that clause.

Granting the 'presupposition', where we find semantic binding, there we should also find syntactic binding. Consider, then, (11)b,c. The explicature of (11)b (relative to context) is expressed by (11)c, which contains a semantic binding relation between the matrix DP *every student*, and the pronoun *her* in the prepositional phrase *on her exam* adjoined to the complement DP *every exam*. Yet, trivially, (11)b appears not to express any such relation syntactically, for it lacks the bound pronoun. But, by the 'presupposition', there *must* be a syntactic binding relation. The hypothesis that the nominal of the complement DP contains a functional complex whose variable argument may be bound by the antecedent matrix DP satisfies the presupposition. Thus, on the

assumption that the coincidence of syntactic and semantic relations holds generally, we have an argument for the hypothesis. Further, we have a refutation of a contextualist account of the phenomenon, for pragmatics does not determine syntax, but if Stanley's presupposition is correct, then the semantic reading rides on the back of syntax.

In general, the methodology here exemplified is to show that clauses that putatively express 'unarticulated constituents' have a (semantic) reading that involves the constituent being bound by a higher operator; it follows from the coincidence of syntactic and semantic binding that there is a variable in the syntax that is bound by the explicit quantifier. Thus, for the quantificational cases at hand, we are interested in those constructions where one or more quantificational DPs occur whose explicatures appear to demand a reading under which the domain of one varies with the domain of the other. (11)b provides an example (repeated here as (13)a). Here are two more examples, with the relevant readings supplied:

- (13)a. Every student answered every question.
 'For every x such that x is a student, every question asked x was answered by x .'
- b. In most of Bill's classes, he fails exactly three Frenchmen.
 'For most x , such that x is a class of Bill's, Bill fails exactly three Frenchmen in x .'
- c. In every room in Bill's house, every bottle is in the corner.
 'For every x such that x is a room in Bill's house, every bottle in x is in the corner.'

So, the evidence is that the domains of the DPs—*every question*, *three Frenchmen*, *every bottle*—are fixed by a preceding DP. In (13)a, not *every question*, just *every question asked every student*, was answered; in (13)b, not *any three Frenchmen* were failed, but three each in *most of Bill's classes*; and in (13)c, not *every bottle* is in the corner, but only every bottle in *every room*. Thus, the hypothesis is that the three latter DPs contain a variable whose value may be fixed by the nominal of the preceding DP. For example, the object variable of the complex 'associated' with *question* is satisfied by students, which then serves as the argument of a function contextually fixed that maps to the domain of the quantifier—perhaps, the set of questions students were asked, or maybe the set of questions on the students' exam. Let us accept these readings.

Stanley does not say what kind of syntactic relation is supposed to realize the putative semantic binding. The idea seems to be that co-indexation under c-command suffices. One might raise concerns here over the status of indexation, but let this worry pass.²² If Stanley means that where we have a binding reading, there we have a c-commanding DP which supports that reading, then the claim appears trivial. Of course we shall have a c-commanding DP for the phonologically null item, for we have a c-commanding DP for the DP to which the item is adjoined on the fully realized readings as offered in the above examples.²³ That is, c-command itself does not determine that the putative covert item is bound; that the item is c-commanded by the higher DP would only tell us that it could be bound. But even this triviality appears to fail for Stanley's proposal. The bound item is supposed to be in some sense essentially associated with the relevant nominal, although not merged with it, that is, the association is not syntactic. Therefore, the complex is not projected in the tree as either an adjunct or argument. But if this is so, then how is syntax supposed to make any deliverance on the matter given that the item is not even in a position to be bound, for it does not project to be in a position of the higher DP's c-command domain? Here, we are effectively being asked to imagine that c-command extends to something outside of the syntactic configuration, while Stanley wants us to think that syntactic theory supports the proposal. In other words, there appears to be no syntactic principle or generalization that admits the imagined binding relation; a fortiori, there is none that militates for the existence within the syntax of the phonologically null item.

Stanley (2005, p. 245, n. 15) does suggest that the covert variables might be in adjoined positions, which marks a departure from the notion of 'co-habitation' or 'association' from the earlier papers, although it does mirror the supposed lexical elaboration of the propositions expressed by sentences with covert variables (cf. Marti 2006). Adjunction has been a theoretically slippery notion for many years (see, for example, Chomsky 1981, 1986b, 1995a, 2004; Kayne 1994; Uriagereka

²² Minimalist scruples, in the form of inclusiveness (see below), might lead us to view indexes as merely notational, for their only mandate appears to be the construals they mark; that is, indexes are extra-syntactic posits. For perhaps the most fully developed non-index account of binding, see Safir (2004a, b). For a defence of indexes as inherent features of nominals, see Baker (2003), especially pp. 96–7.

²³ Here I assume that distinct scope readings are supported by distinct c-command relations. It might be noted, familiarly, that c-command is not necessary for semantic binding:

- (i) [Every lover [of Mozart;_i]] thinks he_i was the greatest of composers
- (ii) [The men [who prepared the fish;_i]] hated their_i smell

1998, 1999). If we think of adjunction as essentially modification, then the proposal does fit with what Stanley wants.²⁴ Further, since adjuncts do not enter into either feature elimination or theta-role assignment relations, then the proposal would lift explanatory syntactic burdens from Stanley's shoulders. However, the proposal is problematic. Although adjunction is not well understood, it seems that adjunction extends phrasal projections without changing the category of the projection, that is, adjuncts are adjoined as *phrasal* sisters of the projections of heads and inherit the phrasal category of their sisters—they exist in a different dimension. Thus, adjunction falls outside of SPEC–head–complement configurations. (In the adjunction proposal presented by Marti (2006, pp. 142–43), the putative variable adjuncts are mistakenly presented as arguments in first Merge position.) Two problems arise. Firstly, it is difficult to see how the covert variables could be understood to be phrasal. An adjunct appears to inherit its phrasal status from its sister. The idea of a variable, however, as understood in mathematics/logic, just makes no sense in terms of syntax. Every lexical item is a bundle of interpretable/uninterpretable features to which operations within the syntax are sensitive. But a variable, as understood by Stanley, has no features at all; it is essentially a semantic element, as it were, an item which can take a contextual value (and so be free in the syntax) or be bound, but this is not something to which the syntax can be sensitive.²⁵ Even if we put this worry to one side, a greater second problem remains.

Adjuncts are essentially *optional*. For example, the English *wh*-adjuncts (*how*, *when*, *where*, *why*) mark, respectively, manner, time, place, and purpose. None of this information enters into thematic assignment or agreement relations. This can be seen by the fact that un/

²⁴ Being an adjunct is not a property of a kind of lexical item, but a configurational position items can realize. The operation of adjunction may be viewed as a deviant form of Merge. Merge extends a structure to create a new kind of projection. Adjunction, on the other hand, adds a second tier, as it were, to an extant projection. Being hierarchically outside of the SPEC-head-complement configuration, adjunction creates familiar ambiguities, for the site of adjunction is thematically open and not marked by agreement (e.g. Bill shot the elephant *in his pajamas*).

²⁵ This is *not* to say that items cannot be construed as variables. Such an understanding is what animated the early work on 'logical form' (Chomsky 1975, 1977; Chomsky and Lasnik 1977; May 1977). It bears emphasis, however, that the 'variable' construals proposed covered PRO and trace *t*, i.e. items that are bound. Free variables, of the kind required by Stanley to take contextual values, were strictly ruled out within the syntax (Chomsky 1977, pp. 10–11; Chomsky and Lasnik 1977, pp. 429, 432). Similar remarks apply to covert operators posited to account for 'tough constructions' (inter alia), i.e. the operator has to move in order to bind a lower copy/trace, with the whole structure forming a complex predicate. By more recent theory, we can say that free variables are uninterpretable.

acceptability is invariant over phrases and phrases+adjunct. In effect, adjunction is neutral as to the interpretability of a structure.²⁶

Marti (2006) takes optionality of adjuncts to be a boon for the thesis that variables are adjuncts; but how precisely are we to understand this claim? There are three choices, each problematic. First, the variables might be strictly optional like genuine adjuncts. The problem with this option is that Stanley's very argument is that the semantic binding phenomena *necessitate* the covert variables, given 'innocent' assumptions. Thus, optional adjuncts fail in the work Stanley would ask of them; that is, if we take the relevant structures to be invariant over grammatical criteria of acceptability, whether they feature covert variables or not, then there simply is no syntactic argument for thinking that they are extant in the structures. Second, the variables might be understood to be necessary. This would, however, just render the 'adjuncts' non-adjuncts. As remarked, adjuncts do not enter into feature valuation or thematic relations, and so do not determine acceptability. Third, the variables might serve as adjuncts as and when required. Following Marti (2006), Stanley (2005) appears to favour this option: he suggests that arithmetical nominals do not contain covert variables because their semantics do not require domain restriction.²⁷ But we are after a syntactic reason to think that covert variables exist. Even if one were to sideline the other problems, which look quite recalcitrant, to posit an empty adjunct to capture a semantic reading is not to furnish a syntactic reason.

Overall, Stanley's argument here, as far as I can see, is premised upon the silent assumption that the nature of syntax should be read off our stable semantic intuitions. The specific issue of binding is a red herring

²⁶ Adjuncts do enter into binding relations and negative polarity/parasitic gap licensing, which appears to establish that adjuncts enter into c-command relations. The fact of their inability to affect acceptability, however, remains unaffected. Chomsky's (2004) pair-set account of adjuncts views them as entering the derivation late at the interface. The account is designed to cater for adjuncts' extra-dimensionality while establishing a c-command relation. None of this complexity appears to offer succour to Stanley.

²⁷ Cappelen and Lepore (2005b) appeal to arithmetical predicates as a counterexample to Stanley. Their argument runs on the thought that the covert variables must be bound or have values fixed referentially, but, of course, mathematical truths are not context sensitive. Stanley (2005, pp. 243–4) responds by claiming that arithmetical predicates do not have variables associated with them because they do not support bound readings (cf. Marti 2006). He gives no syntactic reason for this claim (cf. Pagin 2005, p. 325, n. 22, where Pagin appears to endorse Stanley's response but rejects Stanley's broader thesis for independent reasons). It has, alternatively, been suggested to me that variables could take a universal value in the cases under consideration. There are at least two problems with this proposal: (i) apart from universal quantification, it is difficult to see how a variable could take a universal value and (ii), in the absence of an implicit universal quantifier, the values would be contextually determined, but what kind of context could determine a universal value?

in so far as the ~~examples offered do not in fact exhibit syntactic binding as there is no merged item to be bound; rather, it is particular readings that purport to exhibit bindings; and even if the readings are correct, nothing would *ipso facto* follow as regards syntax save for the c-command relation, which is already in place.~~ None of this is yet to say that Stanley's proposal is wrong; it is only to say that it is opaque what principle within syntax should lead us to think it is correct. The syntax appears to contain too little.

Stanley's second general argument from syntax to support his binding proposal is more rhetorical. Stanley (2002a, p. 152–3) asks us to consider the following pair (inter alia):

(14)a. *The ship sank [PRO to <PRO> collect the insurance]²⁸

b. The ship was sunk [PRO to <PRO> collect the insurance]

Stanley takes (14)a to be 'ungrammatical' because 'the available local [controller is not an expression] that denote[s] things capable of collecting insurance' (2002a, p. 153). In other words, PRO—the phonologically null SPEC of TP *to collect the insurance*—requires an antecedent or controller that may realize an agent theta role of the verb *collect*, but the only available expression is *the ship*, which does not play the required agent role. (Note that this assumption depends on certain ideas about what ships can and can not do. For example, in the context of a child's story about ships with mental states, (14)a would be acceptable. Let this complication pass.) Why, in contrast, is (14)b grammatical? Stanley's explanation is that there is a phonologically null prepositional phrase, *by* α , whose complement is the thematic agent of passive *sunk*. Such an expression, α , may serve as the controller of PRO in the subordinate clause. For example:

(15) The ship was sunk {by the crew $_i$ } [PRO $_i$ to <PRO> collect the insurance]

Stanley claims that his argument from binding rehearsed above is 'of the very same structure', that is, the postulation of phonologically null items

²⁸ If we grant 'PRO', we ought not to conceive of it as a peculiar lexical item that essentially serves the special role of realising semantic arguments for control infinitives etc. We take the lexicon to contain a generic item PRONOUN, which is merged at SPEC *vP* to satisfy theta requirements. The merging of infinitive T *to* (with EPP and Case valuing features) attracts PRONOUN to its SPEC, where its Case is valued as [null], and the uninterpretable features of T are eliminated. This entails that the item has no spell-out, it remains covert. 'PRO' simply marks the fact that there is a non-spelt-out item in the structure. Thus, the structures support control construals without the derivation being driven to realize any such readings. For a contentious overview of PRO and control, see Hornstein 2003.

in the syntax explains otherwise inexplicable phenomena. Thus, if one denies the binding argument, one should, by parity of logic, deny the above reasoning. Stanley's argumentative strategy here is questionable.

First off, Stanley is correct that passive verbs imply agency in a way that ergatives (*sink, open, close, break, etc.*) do not. Compare the following cases parallel with the *sank/sunk* case:

- (16)a. Bill broke the glass
- b. The glass broke
- c. The glass was broken deliberately
- d. *The glass broke deliberately
- e. The glass was broken in order to create a disturbance
- f. *The glass broke in order to create a disturbance

The adjuncts in (16)c and (16)e imply agency and are perfectly acceptable; the same adjuncts in d. and f. create unacceptable structures because the ergative verbs do not imply agency. It seems, therefore, that semantic agency is certainly obligatory in these cases, but it just does not follow that this is recorded syntactically.

The phonologically null phrase to which Stanley appeals is usually referred to as an *implicit argument*. Such items are perhaps the *worst possible* for Stanley's analogising purpose, for such arguments are typically taken not to project in the syntax and are so precluded from syntactic relations: plausibly, relations that involve implicit arguments are ones of construal rather than being clearly syntactic (see, for example, Williams 1985; Chomsky 1986a; Rizzi 1986; Jackendoff 1987; Brody and Manzini 1988; Bhatt and Pancheva 2006). For example, theta conditions do not demand the occurrence of thematic agents for passive predicates. Likewise, no agreement or EPP factors demand their occurrence. To see the point here note that, in (14)b, whoever does the sinking need not be the person or persons who collect the insurance; it is left open that, say, the crew sank the ship under the orders of the owner, who collects the insurance; that is, control is optional, and so a phonologically null phrase is not necessary. Likewise, a clever burglar who engineers it so that a security guard breaks a glass (in order to cause a distraction) might rightly be said to be the agent of the distraction. The case of adverbial adjuncts is clearer still: *deliberately* (and similar intentional adverbs) imply agency, but they do not require a syntactically realized agent. In sum, the effect appears to be semantic, not syntactic.

Of course, an explicit provision of a prepositional phrase with a suitable agent complement does make for obligatory control. In effect, an implicit argument is posited as an elaboration of the lexical content to support a control construal; implicit arguments are not independently identifiable syntactic or lexical items.

To buttress the point here, consider that explicit thematic arguments can be provided that satisfy all syntactic requirements yet do *not* serve as suitable controllers.

- (17)a. The ship was sunk by an iceberg [in order [PRO to <PRO> collect the insurance without arousing suspicion]]
 b. [We engineered it so that] the glass was broken by the guard [PRO to <PRO> keep everyone busy while we robbed the safe].

These are fine to my ears, even though an iceberg is not the kind of thing that can collect insurance and the guard is clearly not the agent of the subordinate clause. Both DPs (*an iceberg/the guard*) play a kind of *instrument* role, normally introduced by *with*, which in turn ‘implies’ an agent or perhaps serves as an analogue of one. We can look at the structures as specifying an event in the matrix passive, with the subordinate clause explaining the reason for the event.²⁹ In both cases PRO is not controlled. If the clauses are preposed, then PRO is clearly arbitrary as opposed to controlled and the proposed readings are readily available:

- (18)a. [In order [PRO to <PRO> collect the insurance without arousing suspicion]], the ship was sunk by an iceberg
 b. [PRO to <PRO> keep everyone busy while we robbed the safe], we engineered it so that the glass was broken by the guard
 c. [PRO to <PRO> collect the insurance], the ship was sunk

All of this suggests that the implicit control readings do not lead to any covert item in the syntax.

²⁹ Some informants have reported a slight infelicity, but this is ameliorated once the ‘rationale’ clause is fronted (see below). This suggests that the degree of unacceptability is pragmatic, not syntactic.

Control is also affected by semantic features. Consider:

- (i) Mary did not want to be parked outside the pub all evening
 (ii) The Anglo-French decision to ban the exports angered the Italians

In (i), it is understood that it is not Mary herself that is to be parked but her vehicle, which is not mentioned. Similarly, in (ii) there is no controller at all, but we understand the agent of the ban to be the English and the French, rather than arbitrary. These uses might be peripheral in the sense of Chomsky 1981. My present point is just to suggest that determination of control is not well-suited to Stanley’s analogical purpose.

Even if we were to take PRO to be obligatorily controlled, we would still be left with the need to posit another *by*-phrase, for an iceberg can not be the controller, but evidently, any such phrase would not be an argument of passive *sunk*. Also note that a passive relative—for example, *towed by the crew*—merged with *iceberg* also fails to provide an appropriate controller. In short, it appears to be a control construal that leads us to posit the phonologically null argument. On such a construal, we take the implicit argument in the cases at hand to be an abstract agent that answers to the semantic selection requirements of *collect* on a control construal. There is nothing antecedent in the syntax to support this posit, and the construal does not demand that we put anything into the syntax to support it. Indeed, the alternative constrictals just discussed show that the containing prepositional phrase does not project.³⁰

So, let us return to Stanley's challenge: if one rejects the binding argument for phonologically null functional complexes being associated with quantificational DP nominal complements, then one must, by parity of logic, reject the phonologically null controllers in examples such as the above. To be swayed by such reasoning, we must view the cases as analogous; that is, Stanley's putative covert items must correspond to implicit arguments. For example, no analogy holds between, say, the positing of phonologically null PRO and Stanley's items, for PRO is supported by straightforward syntactic considerations from theta theory and the uniformity of SPEC-head relations, as well as, of course, its role in explaining binding and construal data. In the cases at hand, PRO is required to satisfy the theta requirements of *collect* and to value the uninterpretable features of non-finite *to*. Stanley's items have no such general syntactic support. It would thus be perfectly rational for one to accept the positions of PRO in the above structures and reject Stanley's proposal. But if Stanley's items are a variation on implicit arguments, then we lose any argument from syntax. Again, implicit arguments are just posits to account for the control data, they do not syntactically project. But Stanley's whole idea is that some principles of syntax mandate his covert items. This is not so, at least not if we accept the analogy offered.

³⁰The implicit argument data are somewhat variable. I have discussed the best and most familiar case. Stanley (2002a) also appeals to the following pair, with a covert prepositional phrase apparently required in (ii):

- (i) *The record broke [in PRO winning the race]
 - (ii) The record was broken [in PRO winning the race]
- (ii) sounds marginal to my ears.

3.3 Stanley on logical form and syntax

So far we have seen that there is no convincing *syntactic* argument for the existence of variables of the kind Stanley posits within LF structure. This section will further support the case against their existence by way of considering Stanley's conception of logical form.

Nowhere does Stanley say what he means by 'logical form' save that it is a syntactic level that inputs to semantics. In one sense, this lack of specificity is perfectly innocent. Stanley's proposal, on the assumption that syntax does interface with semantics, is that at the interface the syntax should record the kind of readings that the contextual variables make available. So, the proposal is neutral about which syntax would best provide for this service. However, the lack of specificity raises serious worries that the proposal is quite unhinged from *any* account of syntax. The problem is simple: What reason do we have, semantics apart, for thinking that the 'best' syntax admits the covert contextual variables? The question is not rhetorical. No level of syntax is a mere receptacle for whatever we may identify at the semantic level. *Anything occurring in the syntax must enter into syntactic relations, not just semantic ones.* There is an all too easy equivocation between construing 'logical form' as a level of meaning and construing it as a level of syntax that is sometimes called 'logical form'. As previously mentioned, recent theory within the minimalist program has ditched LF, and other frameworks have never sanctioned such a level. So, far from the best syntactic theory mandating contextual variables at logical form, the 'best theory' may not even admit logical form.

If we sideline the recent 'level-free' developments, we might ask: What is a syntactic relation as opposed to a semantic one? The question cannot be answered *a priori*, but only on the basis of theory. Let us make two assumptions:

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- (i) *The Merge Assumption*: Every syntactic relation is a function of a binary combinatorial operation.
 - (ii) *The Inclusiveness of Interpretation*: All output of the syntactic computation must be exclusively interpretable at one or another interface (morphology/phonology or semantics) on the basis of just the lexical features that enter into the computation.
- 

Assumption (i) provides us with a binary operation, *Merge*, that simply builds complex structure from atomic items in a pair-wise fashion. All syntactic relations must be derivable from *Merge*. Thus, for example, movement/copying will be simply (Internal) *Merge* of an already

merged item to extend the structure; c-command, as we saw earlier, will reflect the fact that the c-commander is merged with the head projection that contains all that it c-commands (i.e. sister-hood becomes a simple reflex of Merge). Assumption (ii) tells us, in effect, that the computation is essentially and only driven to ‘sort’ lexical features between those that are, respectively, interpretable at ‘sound’ and ‘meaning’.³¹ Following these two assumptions leads us to a conception of *principled explanation*, where the structure of the faculty is explained either by non-language particular principles of computation (Merge) or external demands from the interfaces (see Chomsky 2001b, 2004, 2005a). Thus, in one sense, as earlier suggested, the syntactic derivation is driven to answer semantic demands, especially as regards the dual nature of Merge. It would be a mistake, however, to think of minimalism as a functionalist semantic agenda. For any item at LF, we need to ask: How did it arrive at LF—what is its merge history? What features of the item are un/interpretable? Without answers to these questions, the putative item is simply beamed from semantic intuition.³²

The speculation behind the goal of principled explanation is that the faculty computation answers to the most minimal/economical design specifications (*per* assumption (i)) to satisfy the demands of the external components (*per* assumption (ii)); that is, the computation proceeds *as if* it follows the instructions of the external components. *Pace* Stanley, the faculty does not in fact follow any such instructions. As explained above, the computation operates simply to value or eliminate features, with the effect that its outputs blindly and optimally meet the demands of the external components. What features drive the computation?—minimally, those to which the computation is sensitive; any other features will not be recorded in the syntax. For example, a noun carries person, number and Case features (what Chomsky (1981, 1995a) calls ϕ -features). It carries the first two because it must enter into agreement relations; and these features are semantically interpretable, of course. Case is also necessary (although mainly silent in English) because it has a morphological reflex depending on the configurational position of the noun. But Case is not semantically

³¹ The assumption here fuses two earlier assumptions for convenience: *full interpretation* and *inclusiveness*. See Chomsky 1986a, 1995a,b. Both assumptions are methodological (guides to research); they are not a priori truths.

³² Neale (2004, p. 113, n. 59, and 2005, p. 230, n. 107) rightly chides his earlier self (Neale 1994) and various others for treating LF as a receptacle for semantics as opposed to a level of syntax. It is curious, therefore, that Neale should focus on what an element is ‘doing’ at LF vis-à-vis semantics instead of focusing on how the syntactic derivation treats the element such that it has its particular LF position.

interpretable. Thus, for any noun entering a derivation, the computation must value Case so that the semantic interface ‘recognizes’ it as uninterpretable. Similar reasoning applies to other categories. Now, obviously, a noun such as [cat] carries many features beyond its ϕ -features. Many features appear to be structural (e.g. [cat] can be used as count, mass, and abstract). But it would seem that the syntax is not sensitive to any of [cat]’s substantial semantic features, beyond minimal requirements of theta role as determined by verbal configuration. For example, the internal structure proposed by Pustejovsky (1995) appears not to enter into the syntax. In short, much of the lexical information appears to be carried, as it were, through the syntactic derivation. It only becomes visible after the structure is handed over to the external semantic component. In general, then, the derivation works as if it were solving an equation between lexical items and divergent external requirements from sound and meaning. The minimalist assumption is that the derivation is optimal (as encoded in our two assumptions above). The derivation works *as if* it ‘knows’ just what is interpretable to the respective interfaces, with uninterpretable features and copying serving as a joint mechanism to satisfy the interface conditions, but the optimality is achieved independent of any access to what sense, if any, the external components may make of the structures that input to them. Here is the general moral: an item of syntax must be such that it is (i) merged and (ii) carries un/interpretable features to which the derivation is sensitive. In general, as Chomsky puts it, ‘Derivations are driven by the narrow mechanical requirement of feature checking [that is, valuation of uninterpretable features] only, not by a “search for intelligibility” or the like’ (Chomsky, 1995a, p. 201). Let us now consider how Stanley understands ‘logical form’, again ignoring the recent level-free approaches.

First off, Stanley appears to understand LF as a syntactic receptacle for whatever semantics demands, where semantics here is the inchoate notion of a proposition or truth conditions. Thus, Stanley writes:

Syntax associates with each occurrence of a natural language expression a lexically and perhaps also structurally disambiguated structure which differs from its apparent structure, and is the primary object of semantic interpretation. In accord with standard usage in [generative] syntax, I shall call such structures *logical forms*. (2000, p. 393)

Stanley further explains that context ‘help[s] us to decide which logical form is the one that has been uttered’, where this solves the equation: ‘utterance + X = logical form’ (2000, p. 399). If this is intended as an account of the notion of ‘logical form’ (= LF) as it occurs in contempo-

rary generative linguistics, then it is off target.³³ (i) LF is a mental structure; we cannot *utter* logical forms; nor can we utter something *with* a logical form. PF is an orthogonal structure to LF. (ii) Logical forms are not the ‘objects’ of interpretation. We can understand an utterance, and in so doing, we will presumably map information from LF onto an internal structure formed from our perception of the acoustical properties of the utterance, but we cannot conceivably interpret an LF structure of another speaker; LF is ‘inside the head’ it is not something expressed externally. (iii) Logical forms (LF structures) are not disambiguated structures corresponding to natural language sentences. The idea that there is such a map was proposed by May in his 1977, but rejected by him in his 1985, and is, anyhow, by no means the consensus. As previously noted, following minimalist constraints, QR has fallen under a certain suspicion. May’s Quantifier Raising (QR) operation, for example, raises object DPs whose accusative Case has already been valued. On a phase derivation model, the DP should thus be ‘frozen’ or ‘transferred’ before it can be moved higher up the structure to capture distinct scope readings. Perhaps the quantifier carries some further uninterpretable feature. These are matters of live debate. (iv) Perhaps most fundamentally, it is a mistake to think that there are logical forms, as if they were a kind of object. LF is a level at which operations on lexical features accessible to external systems input to those systems. It is wholly an abstraction to turn the level into a set of objects independent of the operations of the faculty, as if they were determined by an association with natural language sentences under favoured semantic readings.

In short, Stanley conceives as LF as part of an interpretive process, with speakers assigning ‘LFs’ to one another, which, with context, determine truth conditions. Given this view, it is little wonder that Stanley feels so free to posit elements at LF that have no syntactic sanction; they are posited wholly on the basis of the needs of contextual interpretation, not the internal demands of interface legibility.

Consider the structure Stanley and Szabó (2000a, p. 251) propose:

(19) [_S [_{NP} [_{Det} Every][_N <man, f(i)>]][_{VP} [_V runs]]]

³³ No level of syntactic structure has ever essentially implicated a truth conditional conception of semantics: Stanley’s equation is devised from a certain conception of semantics and how that conception might integrate into syntactic theory (e.g. Higginbotham 1985; Larson and Segal 1995). Indeed, Chomsky (2000b, p. 132, 2003, p. 295) has remarked that his implicit notion of ‘meaning’ has always been more akin to Austin’s or Wittgenstein’s. Notice that where truth conditional semantics has been most clearly fruitful is in exploring certain structural properties of constructions that have little to do with mind–world relations or inchoate notions of communication and publicity. For discussion, see Chomsky 1977, 1996, 2000b; Hornstein 1984; McGilvray 1998; and Pietroski 2003a.

Stanley and Szabó explain that the variable complex ‘f(i)’ is ‘associated’ with the nominal and ‘co-habits a node with it’. This proposal might attract some plausibility if one is treating structures like (19) as representations mapped onto utterances by an interpreting speaker, but it is not a serious proposal about LF unless a story is told about *from where* the variable complex arises. That is, it is one thing to say where the complex *should* occur, it is quite another thing to say how it arrives in that position. Without an explanation of the *origins* of the complex, the structure is effectively unconstrained by syntax. The problem for the proposal is that, *derivationally*, the complex has no place in LF structure: it is parachuted in from semantics. This essential syntactic constraint will be missed if LF is construed as a *representation* of properties of a sentence, rather than as the output of a derivational process.

By minimalist assumptions, every element at LF must be drawn from a selection from the lexicon; no element can be present that is not a lexical feature. Well, are variable complexes in the lexicon? All we are told is that they are ‘associated’ with nominals and ‘co-habit’ nodes.

First, as Stanley (2002b) tentatively suggests, we might try to read the variables as *incorporated* into the nominal projection (see Baker 1988, for the classic account of incorporation). This would make the relation between variable and nominal much like the relation between the noun *deer* and the verb *hunt* in the complex intransitive verb *deer-hunt*. In general, cases of incorporation reflect thematic assignment. But Stanley’s proposed covert variables are not constrained to satisfy thematic roles; quite the opposite: they appear to be intended to be interpreted as adjuncts, which, as we saw above, are essentially optional elements of structure. It thus seems that there is no basis to view the relation between overt noun and covert variable as one of incorporation.

A related option is take the relation to be one of *conflation* in the sense of Hale and Keyser (2002). Conflation may be understood as ‘lexical internal incorporation’. For example, we may view the unaccusative *redden* as a deadjectivalised verb, of the form [_V red-en [_{ADJ} <red>]]. Yet the internal structure hypothesized by Hale and Keyser does not project in the ‘sentential’ syntax; in particular, the structure does not fall under c-command relations from outside the particular lexical items. Thus, no binding relation will be supported. Again, it seems that we lack an extant model for what Stanley requires.

One might suggest the idea that the complexes are ‘triggered’ by the merging of N with Det. This is another non-starter. Nothing can occur in the merged object that is not present in the constituent objects; this

follows from our assumptions. The nominal can be restricted, of course, but only from further merging of lexical items, not from the mere fact that a DP is a merged object.

4. Concluding remarks

We have seen that syntactic structure, as understood in recent generative theory, is out of step with the kinds of demands philosophers make of it. It contains both too much and too little. As hopefully made clear, this dissonance is by no means problematic for the linguistic theories, for they do not presume to capture the same intuitions or notions that animate philosophical research. They might be off course in other ways, but it would be obtuse to condemn them on grounds quite outside their empirical and theoretical range. Equally, my plea is not for a veil of silence to fall between linguists and philosophers—quite the opposite. The work of King, Stanley, and others exemplifies the fecundity of a philosophy of language oriented to our best science of language. If my considerations show anything, it is perhaps only that a closer integration is required if linguistic results are to play a genuine role in shaping philosophical accounts of language, as they must, if such philosophy is to be a serious area of inquiry.³⁴

School of Philosophy
University of East Anglia
Norwich NR4 7JT
UK

JOHN COLLINS

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