

Positing Covert Variables And The Quantifier Theory of Tense*

Matthew McKeever
ConceptLab, University Of Oslo
mipmckeever2@gmail.com

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Abstract

One of the crucial issues in the debate about the correct treatment of natural language tense (and modality, but I'll concentrate on tense) concerns covert variables: do we have reason to think there are covert temporal variables in the syntax, as the quantifier theorist maintains? If we do not, then it seems we can quickly discount the quantifier theory from consideration, even in advance of consideration of the range of empirical phenomena deemed to support it. And, indeed, there's a good reason to doubt that there *are* such variables: contemporary syntactic theory, notably, doesn't seem to posit them.

My aim here is to respond to this argument that goes from the premise that positing covert variables is illicit given what we know about syntax to the conclusion that quantifier theories of tense (indeed any theory positing such variables) is false. I will argue that the argument doesn't work, and I will do so by suggesting a non-committal understanding of the process of positing covert variables. According to this view, even if we aren't doing fundamental syntax in so positing, nevertheless there's reason to think we are doing *something* theoretically productive, just because it seems like an important part of the practice of semantics and because it seems like semantics is a discipline which is making progress. The progress of semantics supports the methodology of positing variables at its heart, even if we're not doing syntax and even if, indeed, we don't know what we're doing when we make such posits. The quantifier theorist of tense, accordingly, needn't worry about their syntactic commitments.

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1 Introduction

Natural language tense and modal expressions seem to be devices of generality, and in particular, to be what I'll call quantifier-like.¹ For an expression to be quantifier-like is for it to be modellable in terms of the basic quantifiers of predicate logic, the existential and universal quantifier.

Thus consider a sentence like

- (1) I saw the Godfather II in College

This is true provided *there is some* time in the past (in particular some time lying in the span of time at which I went to college) at which I saw the film. In a similar vein

- (2) I have never seen the Godfather III

is true provided at *all* times in the past, it's not the case that I saw the film. Such glosses can be modelled by a simple formal quantificational language as so, where the variable '*t*' ranges over times and where '*u*' stands for the utterance time:

- $\exists t. t < u \ \& \ \text{College}(t) \ \& \ \text{SeeGFII}(t)$
- $\forall t. t < u \rightarrow \neg \text{SeeGFIII}(t)$

(In a more accurate treatment which would command greater empirical coverage as an attempt to model natural language, we would use generalised quantifiers here, but this will suffice for our initial purposes.)

What is the significance of this fact? Nothing immediately, but we can note that there are other sorts of expressions of natural language that are quantifier-like. Most noticeably, some quantified noun phrases are. Thus we have:

- (3) Every pandas like bamboo
(4) Some chairs are comfortable

Representable as:

- $\forall x. \text{Panda}(x) \rightarrow \text{LikesBamboo}(x)$
- $\exists x. \text{Chair}(x) \ \& \ \text{Comfortable}(x)$

Moreover, as we'll see, people like Barbara Partee (Partee (1973)), Max Cresswell (Cresswell (1991)), and Philippe Schlenker (Schlenker (2006)), among others, have argued that the similarities go much further, and the basis of this similarity, we might think that our semantic theory should treat quantified noun phrases and tenses in the same way.

¹Having once mentioned modality, I will talk in this paper solely about tense; some conclusions generalise, although one ought to be careful about lumping the two together, given their markedly different syntactic realisations.

My aim in this paper is to obliquely defend the quantifier theory of tense by responding in detail to an objection to it. The objection goes roughly as so: the most well known way of modelling quantified noun phrases in natural language, the one we teach to our students and presuppose in our research, is by means of (generalised) quantifiers and bindable elements like pronouns and traces. If tense is to get the same semantic treatment as quantified noun phrases, then it must be modelled by means of quantifiers and variables.

But, on the surface of it, it's at least dubious that tenses should be modelled, syntactically, in this way. If tenses are quantificational, we should expect a syntactic form for 1 very roughly as so:

- PAST_t I see(t) the Godfather II and I'm in college(t)

Where 't' is a temporal variable bound by the temporal quantifier 'PAST'. The problem is it's unclear that we have any reason to make such a posit. There's at least prima facie plausibility in the claim that pronouns are variable-like elements, and traces, although theoretical posits, are to some extent independently motivated by syntactic data (such as wh-movement). By contrast, the syntactic evidence for the existence of temporal variables is questionable. Most obviously, there are no pronoun-like items which function *overtly* as temporal variables (although see Schlenker's interesting discussion of 'then' in Schlenker (2006)). Moreover, if there were temporal variables, there would be temporal quantifiers, and one behaviour we expect of quantifiers is to be iterable, but things don't go well when we try to iterate temporal expressions (for but one example of this, see Dowty (1982)).

That is the objection to the quantification theory I want to consider in this paper: that there are no syntactic grounds for positing temporal quantifiers and variables. If this is so, then the analogy with quantified noun phrases is weak, and we should seek another analysis of tense, and not take its quantifier-like behaviour as an insight into its actual semantics. There's a tension between the superficial quantifier-like behaviour of tense and what syntax gives us.

I will attempt to dissolve this tension, and to show that there is, in fact, nothing to worry about: semanticists, and thus tense theorists, needn't worry if their analyses don't square with what syntax tells us.

My response has two steps. The first notes that positing covert variables, or covert structure more generally, is fundamental to how working semanticists operate. If it were no good, then semantics would essentially be barren—as I'll show, even simple objectual quantification, according to its textbook analysis in Heim and Kratzer (Heim and Kratzer (1998)), would be no good.

But semantics isn't no good—it appears as if formal semantics is a research program which is making progress. In the past half-century or so we have developed a range of technical tools and proposed theories using them sensitive to a lot of linguistic data. These twin constraints of empirical and technical adequacy are ones formal semantics shares with uncontroversially successful disciplines, and absent reason to the contrary, if we think the latter make progress we should think the former does too. But then if one shares my sanguine assessment of formal semantics, and accepts that positing covert material is a central part of its methodology, one should be inclined to accept that positing such material is licit: it is allowed because it's a

feature of a research program which is successful. And so the objection to the quantifier theory of tense is defanged.

That's all very well, one might think, but it doesn't address the main problem, which is that syntax gives us no reason to think that these things we posit are actually there. Otherwise put, these three claims seem to be in tension: positing covert variables is a central practice to semantics; positing covert variables is (syntactically) unmotivated; semantics is not barren.

However, I don't think they are in tension, rightly understood. In particular, they aren't provided one changes the way one understands what it is to posit covert variables, and in particular provided one gives up on the claim that positing covert variables involves doing syntax. Although it may *seem* like that's what we're doing, when we write our trees in semantics classes and papers, it's not. We're doing something different, and so it's no objection that our posits don't chime with syntactic theory. The obvious and crucial question, though, is then: what *are* we doing when we posit covert variables?

Well, I don't know. But I don't think that matters. We study and develop theories about many things we don't know really know the essence of. At the risk of an overblown example, we really have no idea what's going on when we use the Schroedinger equation to describe the evolution of a quantum system, but use it we still do, because it works. People working at the foundations of physics can and do ask what *is* really going on, but for most people, even working physicists and engineers, it doesn't matter. If they can use it in their code or rely on it when building some tiny implements, that's enough for them. It's not really an objection to a physicist or the engineer using the equation to say that the quantum world is completely mysterious and they don't *really* know what they're talking about. Similarly, it's not an objection to the semanticist, I claim, to say that positing covert variables is mysterious, because it seems to work, in the sense that it seems to be an important part of the method of semantics, and semantics seems to make progress.

So that's my view: in positing covert structure, we're doing *something*, and we don't know what it is, but that doesn't matter. It seems to work: we develop testable theories constrained by linguistic data and formal principles (such as compositionality), we refine and refute those theories, and develop new ones. Given that it seems to work, even if we don't know what we're doing, we should feel no qualms continuing to do it. The quantifier theorist of tense, accordingly, should be unworried by claims that their theory rests on indefensible theoretical posits.

The structure of the paper is as follows. In the next, second, section, I briefly review the operator vs quantifier debate in the theory of tense, and show why the data seems to support a quantifier view. I'll point out how this requires covert variables, and then go on to consider another theory which also requires such variables, namely Stanley and Szabo's well-known view of quantifier domain restriction. I'll then, in section three, consider a challenge to Stanley and Szabo's view, from John Collins (Collins (2007)), according to which we have no syntactic grounds for positing covert domain restricting variables, and show how it generalises to temporal variables. Section four is devoted to responding to this challenge by presenting in more detail the argument sketched above, and section five considers some responses and objections.

2 The Case For Variables

In this section, I want to review the case for positing covert variables both in the debate about tense and in semantics more generally.

In order to do this, a bit of familiar history. I noted in the introduction that tense in English appears quantifier-like: at least at first glance, it seems like we can model tenses by means of the quantifiers of predicate logic. I suggested, somewhat quickly, that on that basis one might seek to give tense and natural language quantified noun phrases the same analysis.

However, this presentation somewhat ignored the history of the debate, which in fact went a different way: the quantifier analysis of tense was not the first one, and properly to understand the nature of the debate it's necessary to see this.

So, first, note that just because something is quantifier-like, it doesn't mean that it must be analysed in terms of quantifiers and variables bound by them. The reason for this is that, according to the way I'm using it, modal operators are themselves quantifier-like. That is to say, a formula like:

- $\Box p$

Is well-glossed in terms of the first order:

- $\forall w. \text{Accessible}(w) \rightarrow \text{True}(p,w)$

Clearly, the modal operators of, formal example, basic modal logic aren't the same sort of thing as the quantifiers of, for example, first order logic. They may be truth conditionally equivalent to sentences of quantificational logic, but they aren't themselves quantifiers.

It's accordingly a live option that, in the face of quantifier-like behaviour of a given sort of expression, one analyses it as a modal operator. And this is precisely what we find in the work of Prior (e.g. Prior (1968)).

Prior thought one could analyse tenses in terms of a modal logic with four operators. He added to quantificational logic the four operators P, F, H, G, which express, respectively, past existential quantification, future existential quantification, past universal quantification, and future universal quantification. Thus consider

- (5) I saw the Godfather II in College
- (6) I have never seen Godfather III
- (7) I will understand tensor calculus some day
- (8) I will always remember this day

These could be translated into Prior's temporal logic (or rather an Anglicised version of it) as so:

- $P(I \text{ see Godfather II and } I \text{ am in college})$
- $H(\neg I \text{ see Godfather III})$

- F(I understand Tensor Calculus)
- G(I remember this day)

This seemed reasonable. If our choice were between a quantifier and an operator analysis of tense, then in light of the superficial difference between objectual quantifiers and tenses, one might think it wise to opt for the latter.

Indeed, even those who realised the inadequacies of the Priorian theory—of which more immediately below—were impressed by this fact. Hans Kamp, whose seminal work on ‘now’ showed there must be more to the theory of tense than is permitted in a simple, even multimodal modal logic like the above, wrote

I of course exclude the possibility of ... symbolizing the sentence by means of explicit quantification over moments ... Such symbolizations ... are a considerable departure from the actual form of the original sentences which they represent—which is unsatisfactory if we want to gain insight into the semantics of English. (Kamp (1971), fn1)

However, time went on and it became more and more evident that the operator theory had some serious empirical difficulties. Barbara Partee (Partee (1973)) famously showed some similarities between tenses and pronouns, noting that just as we use pronouns to refer to some particular object, so we sometimes use tenses to refer to some particular time. To use a variant of her example, imagine we’re on I-35 driving back to work, having just eaten lunch at home. I remark in a panic:

(9) I didn’t turn the stove off!

The existential truth conditions given by the operator analysis don’t seem right: it’s quite tempting to say that we are talking about some particular time, namely the time just after I finished cooking dinner and asserting, of it, that I didn’t turn the stove off then.

More work in this vein continued, as a range of sentences appeared which weren’t immediately amenable to an operator analysis. We may think of David Dowty’s (Dowty (1982)) work on adverbs, which shows the operator analysis yields incorrect predictions if we analyse adverbs as operators, and later Max Cresswell (Cresswell (1991))’s work which—building on that of Kamp and Frank Vlach (Vlach (1973))—showed that there are sentences not analysable in terms of a modal logic anything like the one above. A particularly neat example is furnished by sentences involving interactions between tense and universal quantifiers. Consider:

(10) Some day, each of my students will be on the board of *Linguistics And Philosophy*

The most salient reading of this sentence is that at some one point in the future, each of the speaker’s *current* students will be on the board of the journal—the speaker is imagining a situation in which his school rules supreme by capturing the editorial board. We can’t get this from the modal analysis though. It can give us the following disambiguations:

- F (Each of my students be on the board of *Linguistics and Philosophy*)

- [Each of my students]_x F (x be on the board of *Linguistics and Philosophy*)

Neither of these give us the most salient reading. The first gets us that at some time in the future, each of the speaker's then students will be on the board. That is, it talks about the wrong students—the future ones, which are not necessarily the current ones. The second has it that each of the speaker's students are such that in the future he or she will be on the board—but it doesn't mean that all the students will be on the board *together*: one could be on the board at t_2 and only t_2 , one at t_3 and only t_3 , and so on. This doesn't secure the editorial board domination reading we're looking for.

By contrast—although I won't get into the details here—if one were to analyse tenses as quantifiers binding temporal variables, these problems wouldn't arise. On the basis of this (and other data), it has come to seem that a quantificational theory is more apt for an empirically adequate treatment of tense, and that we should thus adopt it. As King says

If the complex temporal facts present in natural language are most readily and easily represented by viewing tenses as involving explicit quantification over time and as expressing relations between times, that is a good reason for thinking that tenses really work this way. (King (2003), 218)

However, recall the Kamp point mentioned above—he felt uncomfortable with the quantificational analysis because it didn't do justice to the surface form of the sentence. What has happened to that?

Well, it seems that positing covert stuff has become more acceptable in the intervening years. Indeed, if one looks around at practicing semanticists and philosophers of language, you will see a lot of positing syntax.² Just to pick more or less at random from my recent reading, we might think here of predicativism about names (defended most recently by Fara (2015)), which involves positing a covert definite articles which precedes bare names, or the covert type-shifting operation appealed to in Moss (2015) to get her theory of epistemic modals to work.

Let me consider one more example, a very famous and influential one in philosophical circles, which I will later make use of. According to Jason Stanley and Zoltan Szabo (Stanley and Szabó (2000)), we need to account for the well-known problem of covert domain restriction by positing covert variables in the syntax. Their argument for this claim is, in essence, to say that covert domain restriction is a quantificational phenomenon, and since quantification is handled in natural language semantics with variables and operators which bind them, we need such technical tools in this case.

The crucial observation here is that sometimes the implicit restrictions on the quantifier domain associated with an expression vary with the values of a higher quantifier which appears to control that expression. Here's an example. The head teacher of Eton, praising the performance of all his final year students, which are divided among several classes, can say:

(11) In every class, every student passed.

²It's worth noting that syntax has also, as one might expect, moved on in the intervening years, a fact worrying given the standard semantics textbook is now 20 years old, and as such rests on 20 years old syntax. I thank a reviewer for this journal for pointing out this fact about the development of syntax.

The first thing to note is that the first quantifier ‘every class’ is covertly restricted: we are not asserting anything about every class *in the whole world*, but rather about every class *in the final year at Eton* (even that needs to be further restricted to the school year when the teacher is speaking, but let’s ignore that). However, if that were all there was to covert domain restriction, one might think a pragmatic analysis in terms of loose speech or something similar might suffice. But there’s more: the second quantifier also is covertly restricted, but not once and for all. Rather, there are different restrictions relative to different classes. We are saying something like *in every class, every student in that class passed*. Less colloquially, one could rephrase that as *in every class x , every student in x passed*. That is to say, it seems like the domain restriction is something like *being in x* where x is a variable bound by ‘every class’ (or rather, technically speaking, by its associated lambda binder).

The truth conditions of the sentence, then, are equivalent to truth conditions which, when they make explicit the domain restriction associated with the lower quantifier, include a variable in the restricting phrase which is bound by the higher quantifier. Stanley and Szabo in effect argue that when this occurs, there is *in fact* a covert variable in the syntax which gets bound. Whenever we have the semantic effects of variable binding, we have its standard syntactical realisation as well.

They posit, accordingly, logical forms very roughly like so:

$$(12) \quad [_s[_{pp} [\text{In } [_{dp} \text{ every class } Fy]_x], [_s[_{dp} \text{ every student } Gx] [_{vp} \text{ passed}]]]]$$

If the property variable F gets assigned something like the property *comprising the final year at Eton*, the variable ‘ y ’ gets assigned nothing, and the variable G gets *being in x* , we will get the right results.

The details—which have been the subject of some debate—aren’t so important right now. The important point to note is that both we have here another case of positing covert variables, and also a rationale for doing so: we may posit them, Stanley and Szabo say, when we witness the semantic effects of covariation. If they were right, then granted we do witness the semantic effects of covariation in the tense case, we would be right to posit temporal variables to account for tense in natural language.

3 The Case Against Covert Variables

However, there are problems with this claim. Most pertinently for the current paper, the syntactic bona fides of these covert variables has been called into question.

In a sense, this worry has been in the debate from the start—it’s just been overlooked. Recall the passage from Kamp quoted above—he didn’t consider quantificational approaches to tense because it certainly didn’t appear that the syntax supported them. Kamp (or the timeslice of him writing in 1971) wouldn’t be too moved by Stanley and Szabo’s view that semantic binding implies syntactic binding. The question is: should he, and we, be moved?

Well, it seems like the way to answer this question is just to look and see what syntacticians say. Does syntax support Stanley and Szabo’s posit? As it happens, a recent paper by John Collins (Collins (2007)) considers exactly this question.

The answer, however, is not positive. Collins shows that Stanley and Szabo don't succeed in making the case that there are covert variables. Collins's discussion is quite long and involves some syntactical technicalities which it would take us too off course to consider, so I'll just pull from his discussion two points which I think are especially problematic to the friend of variables.

So consider: what would one need to do to make the case that there were variables in syntax? It seems, minimally, there are at least two things: one would have to say what sort of syntactic items these variables were, and what sort of syntactic relations they enter into with the rest of the sentence. Collins thinks Stanley and Szabo fail on both these counts.

As to the first, he notes that a variable isn't a syntactic notion. Positing variables in the syntax is something like a category mistake: it's like positing water in the syntax.³ He points out that lexical items are bundles of features, as, for example, the pronoun 'he' encodes the features of third person, singular, and male. But variables have no such features (Collins (2007), 832), and so one will fail to answer the question as to what sort of syntactic items they are.

As to the second point, can we make sense of the syntactic relations the variable complexes like ' Gx ' in the above form stand in to the rest of the sentence? Again, Collins suggests the answer is no. He points out that Stanley himself has wavered on this question, sometimes loosely suggesting that they 'co-habit' the nodes of the DP they are associated with. The problem is 'co-habitation' isn't a syntactic idea, and so won't suffice. He also suggested they might function as adjuncts, but Collins shows this too is problematic. The basic thought is that we want a syntactic reason for positing these adjuncts. But Stanley seems to claim that two sentences can be syntactically the same yet one might permit and the other not permit the domain restricting adjunct. For example, in reply to a paper by Cappelen and Lepore (Cappelen and Lepore (2002)), Stanley argues that the first sentence below contains covert location variables whereas the latter does not:

(13) Everywhere I go, it rains.

(14) Every I go, $2+2=4$.

The thought is we need them in the former case because the verb 'rain' has an implicit location argument—it never just rains, but it rains somewhere. By contrast, the verb phrase ' $=4$ ', expressing the property *being identical to the number four* has no such implicit arguments: mathematical truths aren't location sensitive in this way. That is to say, the presence or absence of these adjuncts is determined by the demands of semantics, and so it is undermotivated from the point of view of syntax.

So here is where we stand. We first considered the argument for positing covert temporal variables. We then went on to consider another case of positing covert (domain restricting) variables, and a criterion for when it's licit. After that, we saw reasons to doubt the syntactic legitimacy of such variables. Let's grant that Collins's argument against Stanley holds, and, as he himself surely thinks, generalises beyond that one case. Collins therefore thinks, naturally enough, that if these theorists are positing syntax that isn't there, then their theories are falsified. It is this that I want to challenge in the remainder of the paper. Even granting that Collins is right about syntax (which I independently do), his conclusions about semantics can be resisted.

³As he notes, this is a different to positing items like PRO or traces, which have stronger syntactic grounding.

And in particular, turning to the tense debate, which Collins wasn't discussing, let's assume that positing of temporal variables (for example) is as syntactically unmotivated as positing domain restricting variables. If that is so, the Collins line would have it that because positing temporal variables is no good, so, by extension, the quantifier view of tense is also no good. The remainder of the paper attempts to argue against this, showing that one can reconcile the semantic criterion for positing variables with the demands of syntax.

4 Why There's No Cause For Concern About The Above

For the remainder of the paper, I want to try to dispel any worries caused by the tension I've just created. The first step of my argument goes as so: if positing covert stuff is misguided, semantics is misguided. The second step argues that semantics isn't misguided. Accordingly, positing covert stuff isn't misguided, and so quantifier theorists of tense needn't worry that by positing covert temporal variables they are doing something wrong.

4.1 If positing covert stuff is misguided, semantics is misguided ...

My basic worry with anti-positing covert stuff arguments is that they threaten to impugn an awful lot of work on semantics; indeed, followed through, if we take a hard line against such positing, we are left with next to none of the foundations of the discipline that has arisen in the last fifty or so years.

My argument is quite simple. Its central premise is that objectual quantification, as typically understood, involves positing covert elements. The most easy way to see this is to note its dependence on *indexes* conceived of as bits of syntax to achieve its aims, although I think the point applies also to the lambda terms used for binding, as well, although perhaps less forcefully, to the traces posited as arguments to verb phrases.

However, that's to get ahead of myself. I better, at least briefly, review the textbook theory. Consider a sentence like (treating 'Everybody' as a single word so we don't have to bother with a story about its composition):

(15) Everybody is happy.

Our goal is to give a compositional categorematic theory, by which I mean one which assigns to each expression in the sentence a meaning, and determines the meaning of sentences and other complexes of words on the basis of their parts using as few rules as possible. I assume we've already seen the rationale for treating 'is happy' as a function from objects to truth values, notated $\langle e, t \rangle$, and for assuming that it combines with—for example—the denotations of name arguments by means of function application.

The neatest thing would be to say the same here: that the parts are composed by function application. Given that, and assuming 'Everybody' isn't type e , the only option is to treat it as a function from functions to objects to truth values to truth values, that is, notated, of type $\langle \langle e, t \rangle, t \rangle$.

However, things must be more complicated. Consider:

(16) John loves everybody

Assuming ‘loves’ is $\langle e, \langle e, t \rangle \rangle$, then we’ll get a type mismatch trying to compose the verb phrase. ‘Loves’ looks for an e but finds an $\langle \langle e, t \rangle, t \rangle$. So what we do is, we assume the possibility of movement. An expression can appear at LF in a place other than where it appears on the surface, and when it moves, it leaves behind what’s called a trace. This gives us:

- Everybody John loves t_1 .

That doesn’t help overmuch. If ‘ t_1 ’ is type e , then the lower sentence ‘John loves t_1 ’ should denote a truth value, rather than the desired $\langle e, t \rangle$ (which is what the quantifier looks for). So we use another device, a device of abstraction which turns a sentence into a function by treating the trace as a variable and having this device bind it. We then get:

- Everybody λt_1 John loves t_1

Now we have the right typing, and composition can proceed as we would like it to. However, think of how far we’ve deviated from the surface syntax of the sentence—movement, lambda binders, and indexes are now posited as part of the syntax where they weren’t before.

Now, the motivation for movement is arguably solid, or, if it’s been rendered troublesome by recent developments in syntax, is at least an instance of using a syntactically responsible methodology (since it seems independently required to make sense of such mundane sentences ‘Who did you meet?’ where the object of ‘meet’, ‘who’ appears to have moved from its position in the argument place of the verb to the front of the sentence). The other items are less motivated. Indeed, in a more recent paper (Collins (2017)) Collins explicitly makes this point with regard to lambda binders: they are ‘parachuted into the syntax with no independent rationale’ (13) merely to get the typing for quantification right. The same thing applies to the indexes on variable-like elements.

I think this last issue, especially, can often be overlooked because of its familiarity, but it’s worth emphasising just because it shows that positing comes in very early to our semantic education. In the Heim and Kratzer treatment, it’s not only the invisible traces that have indexes; so do pronouns. But this is to make quite a leap from what the surface syntax gives us. Pauline Jacobsen makes the point forcefully:

‘An ... important observation—and one which I feel is worth heavily stressing—the fact that pronouns are not literally variables. After all, we don’t say “He-sub-i saw him sub-j”. Rather (modulo number, gender and case), we have an invariant pronoun which—quite unlike a variable—does not come in the phonology with an index. Crucially, variables are distinct from each other, but pronouns are not. Thus the common wisdom that pronouns transparently reveal themselves as variables is simply incorrect. (Jacobson (1999), 145)

I agree this is worth heavily stressing.⁴ Pronouns aren’t variables, yet to account for even simple cases of pronominal binding, according to the textbook we use to introduce semantics to

⁴A referee points out that indexes have come under attack in minimalist frameworks in the last few years, so if you’re of a particularly minimalist bent and none of this moves you, just consider the same argument as in the text targeting the lambda binder.

our students, we need to assume that they are, and in particular that they carry unpronounced numerical indices. And pronominal binding is surely an essential part of the theory of natural language quantification, and the theory of natural language quantification is a fundamental, deeply rooted part of semantic theory. Positing covert items, then, goes deep: it's not just something we need to account for *recherché* sentences involving tenses (not that such sentences are typically *recherché*: tense is as fundamental a feature of language as quantification).⁵

What is the point of the discussion in this section? It's that positing covert syntax to get one out of a bind (pun intended) is pretty crucial to prevailing semantics methodology. If we were forced to give it up, it would eat into the very heart of our textbook semantics: indeed, pretty much anything past Heim and Kratzer chapter two would be out, which is to say most of semantics would be out.

4.2 ... But semantics isn't misguided

That would be very bad news. In fact, though, I think the magnitude of its badness comes to our rescue. It can't be, I think, that most of semantics is out. Granted that if positing covert variables is misguided, semantics is misguided is true, the thing to do is tollens on that conditional.

That's not just cockeyed optimism. Formal semantics seems like it's doing quite well: in the past fifty or so years, our understanding of a wide range of semantic phenomena has increased dramatically. We have developed sophisticated technical apparatuses to deal with increasingly larger fragments of natural language, and devised new theories using them to account for ever larger amounts of linguistic data. Moreover, these theories are falsifiable, and indeed frequently falsified. And they are constrained, not only by the data, but by theory-internal criteria. We look for theories that are technically simple, and this applies also to posited things: too many posits, and readers will tend to balk at a theory. It's not the case that anything goes.

I think we should take this productivity as a—defeasible, surely—sign that semantics is not fundamentally broken. And to the extent that many of these advances depend on a theory which posits covert items, we should view these advances as an indirect justification of that practice.

That's pretty much all there is to my argument. I will respond to some perhaps slightly less obvious objections in the next section but for now I want to consider a more salient one. I say that positing variables is motivated, but syntax, as practiced by syntacticians, says that it's not. Surely the syntactician should win—how do I square my position with the disagreements of actual researchers in the field?

I do so by saying that, despite what we say and write when we write down trees and posit items, we're not *really* doing syntax. We're doing something else. There's some feature of linguistic reality related to meaning and in some sense, the structures which we posit describe it. This naturally suggests two questions: what is this bit of reality? Granted it's not syntax, why think it exists?

As to the first question, I suggest that we don't know what it is. Some of us might think that it's syntactic reality, but we're wrong to do so, as the syntacticians tell us. But that doesn't

⁵Again, I should say it goes deep *according to the most standard textbooks we teach from and the assumptions we tend to start from in doing semantics*. I certainly don't mean to say that one is compelled to treat pronouns like variables; indeed, in a couple of pages we'll see an alternative approach that doesn't require this. Just that, as a matter of sociological fact, most working semanticists and philosophers of language tend to treat them as such.

matter—it's not necessary, to study a bit of reality, that one have a good conception of what it actually is. Our lives both scientific and otherwise are replete with dealings with things the real natures of which are opaque to us.

Thus, to repeat my overblown example from earlier, we really have no idea what's going on when we use the Schroedinger equation to describe the evolution of a physical system, but use it we still do, because it works. People working at the foundations of physics can and do ask what's *really* going on, and in so doing produce different interpretations of the formalism, but for most people it doesn't really matter. It's not really an objection to a physicist using the equation to say that the quantum world is completely mysterious and they don't know what they're talking about. Similarly, it's not an objection to the semanticist to say that positing covert variables is mysterious. It seems to get the job done, and that's enough.

I don't mean this to be a sort of instrumentalist or anti-realistic position. There really is some bit of reality out there that is described, somehow, by the Schroedinger equation. We just have no idea what it's like. Similarly, in drawing our trees and positing our variables, we are latching on to a piece of linguistic reality; we just don't know what it's really like (the one thing we know, perhaps, is that it's not fundamental syntax). And, again, we have the assurance we're latching on to something by the effectiveness of semantics.⁶

5 Some Objections and Replies

In this section I want to consider briefly consider some more objections to the argument presented above.

The first objection pertains to the difference between positing object language variables and positing temporal variables.⁷ My argument basically went as so: if you think positing temporal variables is bad, you should think positing object denoting variables is bad too. But you shouldn't think the latter so you shouldn't think the latter. But the conditional can be questioned. Is it not a perfectly coherent position to think that positing object variables is okay, but positing temporal variables is not, because there is more evidence for the object variables? As already noted, there are items that appear overtly in English and other languages that behave like variables in some respects, namely pronouns, and there is some syntactic reason for positing traces (namely movement). Given that, you might not think that adding lambda binders and indexes is not such a big deal. By contrast, there doesn't appear to be any such evidence for temporal variables: they seem to be appearing from nowhere, and there seems to be no antecedent reason to believe that they exist. So one might think the two cases are just different, and so we can't go from the claim that there are no temporal variables to the claim that there are no object variables.

I take this point, and think it's a reasonable objection to the view that I've put forth here. I think one's attitude towards this matter will depend on how strongly one weighs the respective evidence: if you are impressed with the claim that there are object variables, then the further posits of Heim and Kratzer might not seem so bad. On the other hand, if you are not impressed with that claim, for example because you think pronouns are notably different from the variables

⁶Let me just make it clear: I'm under no illusions that the effectiveness of semantics is anywhere near that of quantum physics.

⁷I thank an anonymous reviewer for this objection.

of logic, or because, say, movement data don't, well, move you, then you'll object to lambda binders and indexes. I belong to the latter camp, and so I think the syntactic bona fides of object and temporal variables is about the same, but I don't think the former view is absurd.

Let me turn to another objection. I argued that positing variables and covert structure in general was acceptable because it was needed to get semantics off the ground. But, of course, some argue that it's *not* needed for this purpose. Indeed, I quoted with approval perhaps the most famous such semanticist, Polly Jacobsen, whose 'variable-free' semantics is animated, at least in part, by the passage quoted above, as well as by a distrust of movement and other such covert syntactic operations. She seeks to provide a direct-compositional semantic theory, where that is one that runs off something like the surface structure of the sentence.

Accordingly, one might object that the lesson to be learned from all this is simply that we should opt for her semantics. However, I don't think that someone animated by fears about semantic's overreach into syntax ought to take much comfort from Jacobsen's theory, because, at least as she typically presents it, it involves a lot of semantically guided syntactic revisionism as well.

To make this point, it's necessary to say something briefly about the very basics of her system. For the standard theory, as we've seen, pronouns denote objects relative to assignment functions; binding is then a question of abstracting away their assignment sensitivity. For Jacobsen, on the other hand, pronouns denote identity functions which then combine by means of function composition. In function composition, one takes a function F from type a to type b and G from type b to type c and forms a new one from type a to type c defined as so $\lambda s_{\langle a \rangle}.G(F(s))$.

Thus consider a very simple sentence:

(17) Mary loves him

Composing the verb phrase we'll have

- $[\lambda y. \lambda x. x \text{ loves } y] \circ [\lambda x.x] = [\lambda y. \lambda x. x \text{ loves } [\lambda x.x](y)]$

However, there's an immediate concern here. The problem is that if we were to do function application in the next step, we would end up with Mary as the lovee rather than the lover. But function composition won't work either because 'Mary' denotes an object of type e , rather than a function.

If we were to type-raise Mary to its generalised quantifier denotation, so that it denoted $\lambda P.P(\text{Mary})$, and then if we were to use function composition on the $\langle e, \langle e,t \rangle \rangle$ with that $\langle \langle e,t \rangle, t \rangle$ yielding (the not very fun to read) $\lambda y. \lambda P.P(\text{Mary})[\lambda x. x \text{ loves } [\lambda x](y)]$ we will get the right results.

However, the question is how does the semantics *know* that this type-raising is the right thing to do? And Jacobsen's answer is that it's in the syntax. She posits a syntactic type-raising operation so that strictly speaking, 'Mary' as it occurs in that sentence doesn't belong to the syntactic type of determiner phrases but rather a special syntactic type for generalised quantifiers. In general, to get her semantics playing nice with her syntax, Jacobsen posits a wide range of syntactic categories such that, for example, pronouns and names, strictly speaking, don't belong to the same category.

I won't go into all the details: the point, finally, is that Jacobsen's theory is just as syntactically revisionary as Heim and Kratzer's: it just makes different revisions. So we can't get round the problem of positing covert stuff by switching to a variable free theory.

Let me turn to a third objection, which is a more general worry about the nature of my argument. My argument is based on a very sanguine conception of formal semantics, namely that it is a productive research program that is making progress. You might wonder what my evidence for this claim is. It's not like the example I used earlier, of physics, in which we have a guarantee that we're on to something because, for example, we can build machines that work because they make use of the theory. Semantics is less testable in this way. Moreover, you might think that there have been plenty of research programs the proponents of which thought were making progress but was in fact not. Smart people sat around learned societies and cafes, just as we sit around universities, developing their theories about phrenology or Lacanian psychoanalysis and arguing, perhaps ingeniously, with each other. But they were completely misguided in their belief that they were making progress in accounting for personality or neurosis. Who's to say we're any better?

Ultimately, I don't think I can conclusively argue against this here. It's a possibility. What I can do is reiterate what I said before: the theories we develop attempt to take into account a range of often cross-linguistic data, while striving for simplicity and technical competence, and being subject to criticism by many smart coresearchers. It certainly *seems* as if these are exactly the sort of virtues one would want a theory of a bit of reality to exhibit, and so I would be tempted to say that the onus is on the opponent of formal semantics to make their case.

The fourth objection, which I owe to a reviewer for this journal, complains that the view developed here rides roughshod over important distinctions between semantics and pragmatics. Here's one way to put it: the view that I've been defending seems to be obliquely committed to the claim that wherever we have the semantic effects of binding, we have its standard syntactic realisation.

That's an uncomfortable commitment to have. It compels one, to take but one example, to say that donkey and discourse anaphora are to be accounted for syntactically. But there are, as is familiar, a range of viable theories of such anaphora that make no claims about syntax but which rely, on some level, on pragmatics (examples include Neale (1990) and Ludlow and Neale (1991)). More generally, it's just false that when a sentence exhibits some semantic behaviour, that behaviour must be accounted for syntactically. Pragmatic options are available. A theory that doesn't respect that fact is no good.

I agree. I'm not defending a general semantic behaviour implies syntactic realisation view. Pragmatic options are indeed available. I am committed to the narrower point about binding, and in that case, I *am* defending the claim that semantic behaviour implies syntactic realisation. That leaves me with a not unsubstantial dangling commitment: to defend a syntactically realised semantics of donkey and discourse anaphora. But thankfully the literature can come to my aid, and I can point to Elbourne (2005) as an example of such a theory.

Finally, the fifth objection, also owed to a reviewer for this journal, has it that by turning my back on syntax as it's practiced by working syntacticians, I have thereby deprived myself of a useful if partial means of developing and deciding between competing theories. Surely, the reviewer says, it's not the case that *anything* goes when we're writing down our trees in our

papers. But if we aren't required to ensure that our trees are compatible with what contemporary syntax tells us, what constraints *are* we obliged to follow?

Well, that's a very good question, and one to which I don't have a full answer. But I think there are some things we can say, things which in fact reflect our perhaps unreflective current practice. Generally speaking, less is better. We are inclined, I think, to judge theories more harshly the more they rely on covert posits. If someone's theory of whatever semantic phenomenon requires wildly complicated trees with tons of hidden type-shifters, we're inclined to disbelieve that theory. Generally speaking again, neutrality is useful. If a given semantic theory can only be cashed out, say, in Pietroski's (Pietroski (2005)) syntactic-semantic architecture, then we should be less inclined to believe it than one that can also be implemented using Heim and Kratzer's assumptions, or again Jacobsen's. More generally, there are criteria of simplicity, explanatory power, empirical adequacy, and so on that we use when deciding theories and I think we can appeal to them when drawing our trees, so that it's not the case that anything will go.

6 Conclusion

The quantifier theory of tense depends crucially on positing covert items in syntax. This paper has considered an obvious objection to it based on this fact: that in making such posits the semanticist is overreaching and doing, poorly, the job of the syntactician, and we have no independent syntactic grounds for thinking that such items exist. My response, in essence, is that positing covert items is too big to fail: too much of semantics rests on it for it to be misguided. This isn't, I suggested, merely groundless optimism. We should be confident in the methodology of positing because semantics seems to make progress and that methodology is central to semantics. Moreover, I argued for a sort of agnosticism about it is we are doing when we posit covert items—granted that it's not fundamental syntax, what is it? I suggested that we don't know, but that this isn't an objection, because we frequently study things the nature of which we don't understand. The quantifier theorist, then, needn't worry about the syntactic commitments their theory makes.

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