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Bob Hale, Crispin Wright,
and Alexander Miller

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Editorial Offices
350 Main Street, Malden, MA 02148-5020, USA
9600 Garsington Road, Oxford, OX4 2DQ, UK
The Atrium, Southern Gate, Chichester, West Sussex, PO19 8SQ, UK

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De Jure Codesignation

JAMES PRYOR

This chapter surveys a novel kind of semantic structure that has been posited by Mark Richard, Kit Fine, Ángel Pinillos, and others. Their commitments will be explained as we proceed. I discuss four potential areas of application:

- §1 focuses on anaphora, especially cases that can't be handled by a "bound-variable" analysis ("strict" and donkey sentences). I also distinguish our target view from other treatments of anaphora.
- §§2 through 6 discuss the semantics of attitude reports.
- §6 also contrasts two kinds of complex anaphoric dependency. (This may overlap in part or whole with the phenomena mentioned in §1.)
- §7 explains a difference in how functions in a programming language can be sensitive to the identity of their operands.

1

Our stage is set by a range of stances philosophers and linguists have taken to a series of sentences. Consider first:

(1) `Cicero admired Tully.`

(Throughout this discussion, I'll treat `admire` and other transitive verbs as extensional.) Consider next:

(2a) `Cicero admired` `him` (pointing at a bust of Marcus Tullius Cicero, here also claimed to be doing the admiring).

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or:

(2b) He (pointing at the bust) admired Cicero.

Sentences (2a) and (2b) do not have to be produced, or understood, in ignorance of the fact that Cicero is the one being demonstrated. They might be embedded in a larger discourse like so:

(2c) Cicero was too proud to admire other people, but Cicero admired him (pointing), so he (continuing to point) must be Cicero.[1]

Contrast the way the pronoun him works in (2a) and (2c) with the way it works in:

(3a) Cicero divorced the woman who bore him children.

Here him is meant to be, and should be understood to be, anaphoric on the occurrence of Cicero, and hence to *derive its value from* that antecedent expression.[2] If we generate a sentence that parallels (1) and (2a), but with this anaphoric relation between the pronoun and Cicero, we get:

(3b) Cicero admired himself.

There are complex grammatical constraints on when anaphoric pronouns must or must not take reflexive morphology (himself rather than him). For our purposes, observe that pronouns can be understood as anaphoric without the presence of that morphology, as in (3a), or:

(3c) Cicero's wife disappointed him.
(3d) Terentia betrayed Cicero, so he was unhappy.
(3e) Cicero mourned his daughter.

Examples like these are sometimes hypothesized to be ambiguous. For example, (3e) might state that Cicero has the reflexive property $\lambda x.\ x$ mourned x's daughter, or that he has the relational property $\lambda x.\ x$ mourned Cicero's daughter. The latter reading would be required if the his were demonstrative and just happened to designate Cicero, as in (2a) and (2b); but it's also available when his is anaphoric. If there are these two readings of (3e), they'd underwrite an ambiguity that's widely agreed to be present when the sentence continues:

(4a) Cicero mourned his daughter, but Atticus didn't.

Are we talking about Atticus mourning Atticus's own daughter, or about his mourning the same person Cicero did? For historical reasons, linguists call the first reading "sloppy" and the second "strict."[3] The "sloppy" reflexive *reading* of these sentences should not be confused with the reflexive *morphology* exhibited in (3b). As we've just seen, the reading can be present without that morphology. Further, the morphology doesn't force that reading:

(4b) `Cicero admired himself, but Atticus didn't.`

can also be read "strictly," as saying that Atticus didn't admire Cicero.[4]

Our stage consists of stances philosophers and linguists have taken to examples like (1) and (2ab), on the one hand, versus examples with anaphora like (3a–e), and perhaps also:

(5) `Cicero admired Cicero` (meant and understood to involve a recurring use of a single name).

Bracketing examples like (5) for a moment, one stance toward the (3) examples is represented by Lasnik (1976) and Bach (1987, chs 11–12). These theorists deny that anaphoric relations generally have any distinctive syntactic or semantic manifestation. In essence, they'd treat the (3) examples on the same model as (2ab).[5]

A second stance is exemplified by Salmon, Soames, and some other theorists. They embrace three commitments, the first of which I'll label:

(Z1) So far as possible, analyze "sloppy" reflexive readings using bound variables.

That is, they'd take (3b), when so read, to have the form:

(3b′) ($\lambda x.$ *x* admired *x*) Cicero

It's controversial how far such analyses can be applied. Salmon and Soames deny that (5) has this form. That denial is consonant with the fact that continuing (5) with `but Atticus didn't` only allows a "strict" reading.

Restricting our attention to examples that *do* have "sloppy" readings, it's controversial whether a bound-variable analysis can account for all of them. (3d) is less hospitable to such an analysis than (3abe), and examples with "donkey pronouns" like:

(6) `Every orator who had a daughter loved her.`

are widely agreed to resist it: `her` is outside the syntactic scope of any binding introduced by `a daughter`. Yet such sentences still support "sloppy" readings:

(7) `Every orator who had a daughter loved her, but some consul who had a wife didn't.`

meaning the consul did not love his wife.[6]

Salmon and Soames also hold:

(Z2) Propositions of the form ($\lambda x.$ *x* admired *x*) *a* are distinct from propositions of the form ($\lambda x.$ *x* admired *a*) *a* and ($\lambda x.$ *a* admired *x*) *a*.

and:

(Z3) Attitude verbs like `believe` express a dyadic relation between a subject and the proposition expressed by their complement clause.

In light of (Z2), this has the consequence that attitude reports like (8ab) differ in meaning (and, on their view, in truth-conditions) from reports like (9abc):

(8a) `Anita believes that someone admired himself.`
(8b) `Anita believes that (λ`*`x`*`. `*`x`*` admired `*`x`*`) Cicero.`
(9a) `Someone is such that Anita believes that he admired him.`
(9b) `(λ`*`x`*`. Anita believes that `*`x`*` admired `*`x`*`) Cicero.`
(9c) `Anita believes that (λ`*`x`*`. `*`x`*` admired Cicero) Cicero.`

When the binding quantifier or λ operator is inside the complement, as in (8ab), Salmon and Soames say the report relates Anita to a content from which (in any guise) she could infer:

(10) `Someone admired himself.`

When it is outside the complement, as in (9ab), or the binding pattern is as in (9c), they deny that such a content is attributed to Anita. For all that (9abc) say, on their view, Anita may merely be willing to assent to sentences like (1).[7]

At several points in this discussion, I will need to invoke this notion of "contents from which one could infer (something like) (10)." Let's call them "cyclic" contents and thoughts.[8]

The third stance in our drama takes inspiration from an idea of Putnam's that (1) and (5) have different "logical structures." This idea was cultivated by Mark Richard in the 1980s and 1990s, and has been developed in different shapes more recently by Kit Fine and by others.

This stance agrees with Salmon and Soames that there's interesting semantic behavior present in (at least some readings of) examples like (3b) that's absent from (1) or (2ab). But these new theorists recognize among such behavior a pattern that extends also to the "strict" readings of the anaphoric examples, and may also be present in (6) and/or (5). Roughly, their idea is that *all* these examples have cyclic contents, whereas with (1) and (2ab) no inference to a claim like (10) is semantically underwritten.

The sense in which claims like (10) may be "inferable" for a subject, or "semantically underwritten," is not straightforward. This must mean something more than a mere metaphysical entailment, since we already have that with (1). It must also mean more than that the consequence is intended by the speaker, or follows given what's presupposed in the discussion (see note 1 above). With (2c) we'd already have (10) be entailed in those senses. Instead, claims like (10) presumably must follow *from the conventional meaning* of the sentences we're considering (when used in the way we're considering), in a way that full competence with the sentences (so understood) requires one somehow to be sensitive to. OK, but what does that "requirement to be sensitive" amount to? At this juncture, proponents of the third stance reach for ideas like "what a speaker can know *a priori*," or "what a speaker would be immediately justified in believing" on the basis of their understanding. But the first idea threatens to let in all sorts of substantive mathematics that shouldn't be

part of the linguistic competences we're trying to explicate. And the second idea provokes worries about speakers who have no semantic concepts, or speakers who do but endorse false semantic theories (perhaps justifiably).

We will return to issues about the epistemology of language use later. Rather than begin with any specific theory here, I propose we just content ourselves for now with an intuitive understanding of the proposal that the (3) examples are cyclic on both their "sloppy" and "strict" readings, that is, that they *somehow* semantically underwrite inferences to claims like (10). Proponents of this stance can allow "sloppy" and "strict" readings to have different contents, but they'll say that the behavior just described is at least interestingly shared by them; and they'll insist that the "strict" readings have contents different from those in the (2) examples. As I said, this same strategy may be extended to other examples like (6) and/or (5).[9]

The syntax and semantics of our (3) examples is contested, so it's not obvious that the "strict" readings of these examples *need to* end up with their own distinguished contents, from which claims like (10) could be inferred. But that is the view our third stance will defend.

If patterns like the ones we're considering are acknowledged to be semantically encoded, what difference should that make, for example to the truth of attitude reports that embed these sentences as complements? This is a contested issue that we'll explore below.

Here are some more examples that may exhibit the envisaged patterns, where this also can't be the result of the kind of binding structure exhibited in (3b′). First:

(11a) `Only Cicero mourned his daughter.`

This is ambiguous between:

(11b) `Cicero, and no one else, mourned the daughter of Cicero.`
(11b′) (λ*x*. *x* mourned Cicero's daughter) Cicero; and no one else _____

and:

(11c) `Cicero, and no one else, mourned that person's own daughter.`
(11c′) (λ*x*. *x* mourned *x*'s daughter) Cicero; and no one else _____

where the _____ indicates that a "matching" predicate has been deleted or elided, namely the λ-term from the first conjunct. Yet it's not only (11c) that conventionally encodes that it's the same person who both mourned and was the father. The other reading should also encode this.[10]

Second:

(12) `Terentia didn't admire Cicero, but he/Cicero did.`

It's most natural to analyze this as:

(12′) ¬(λ*x*. *x* admired Cicero) Terentia; but _____ Cicero

Not as:

(12″) ¬(λ*x*. *x* admired Cicero) Terentia; but (λ*x*. *x* admired *x*) Cicero

because on current theories of ellipsis, such non-matching λ-terms would not license the surface form (12).[11] Nor should we analyze it as:

(12‴) (λ*y*. ¬(λ*x*. *x* admired *y*) Terentia; but _____ *y*) Cicero

That would posit a syntax and predicate structure more complex than we have independent evidence for. Yet the second conjunct of (12) does seem to conventionally encode that it's the same person who both admires and is admired. So it should be possible for this to happen even when the binding structure is as in (12′).[12]

Third:

(13) `Terentia preferred Cicero to every politician that he/ Cicero did.`

It's most natural to analyze this as:

(13′) ∀*p* : _____ Cicero. (λ*x*. *x* preferred Cicero to *p*) Terentia

Not as:

(13″) ∀*p* : (λ*x*. *x* preferred *x* to *p*) Cicero. (λ*x*. *x* preferred Cicero to *p*) Terentia

Yet the restriction on the quantifier of (13) does seem to conventionally encode that it's the same person who both prefers and is ranked first. So it should be possible for this to happen even when the binding structure is as in (13′).

In summary, the third stance thinks we need to go beyond the bound-variable analysis to capture the pattern they're attending to, and want to argue is semantically encoded. This chapter will survey some further motivations for, and distinctive claims made by, this stance.

2

Let's go back to the early 1950s. Carnap had recently proposed that sentences are synonymous, and express the same belief, iff they are "intensionally isomorphic": that is, they have parallel constructions with corresponding atomic elements always having the same intension. Carnap had also proposed that a report in English attributing `believes that` *S* is true iff there is a language *L* and sentence *T* such that *S* in English is intensionally isomorphic to *T* in *L*, and the subject is disposed to assent to *T* as a sentence of *L*.

In the early 1950s, Church and Mates pressed several objections against these proposals. Among other things, Church (1950) complained that Carnap's analysis wrongly characterizes ordinary beliefs as being "about sentences"; he preferred analyses that instead relate

believers to propositions. Such analyses would translate more convincingly into other languages. Church (1954) argued that intensional isomorphism is anyway too weak for Carnap's purposes, since it tolerates the substitution of non-synonymous expressions with the same intension. Yet such substitutions seem to change the truth-value of attitude reports. Church favored working with a notion of "synonymous isomorphism" instead. Mates (1950) complained that even with a tighter synonymy relation between distinct sentences *S* and *S′*, one can still always embed them:

(14a) Whoever believes that *S*, believes that *S*.
(14b) Whoever believes that *S*, believes that *S′*.

in such a way that speakers can intelligibly assent to one embedding while doubting or dissenting from the other. Here is an example:

(15a) Whoever believes that lawyers are wealthy, believes that lawyers are wealthy.
(15b) Whoever believes that lawyers are wealthy, believes that attorneys are wealthy.

I know that lawyers and attorneys are the same. But conceivably, some subjects may suspect that they aren't. I may regard such subjects as counter-examples to (15b), and for that reason be unwilling to assent to it. Carnap's strategy (even as refined by Church) must count subjects like them (and me) as nonetheless *having* beliefs reportable with complement clauses that they'd resist assenting to.[13]

Putnam (1954) defends Carnap's strategy against some of these objections. Against Church, he claims that multiple analyses can be correct and yet not intertranslatable. Crucial for our purposes is Putnam's response to Mates, which involves re-defining "intensionally isomorphic" so as to be sensitive to differences like those exhibited between (15a) and (15b), or (5) and (1). Here is Putnam's own example:

> "Greek" and "Hellene" are synonymous. But "All Greeks are Greeks" and "All Greeks are Hellenes" do not *feel* quite like synonyms. But what has changed? Did we not obtain the second sentence from the first by "putting equals for equals"? The answer is that the *logical structure* has changed. The first sentence has the form "All F are F," while the second sentence has the form "All F are G" – and these are wholly distinct … (Putnam, 1954, pp. 153–154)

Putnam's idea here is that the meaning of a sentence is not *just* a function of the meaning of its parts, but also of the sentence's "logical structure," and that Mates had been assuming too simple an account of that. If we instead work with a notion of structure that's sensitive to the recurrences of terms (see Putnam's n. 10), so that the difference exhibited by All *F* are *F* and All *F* are *G*, and presumably also by Cicero admired Cicero and Cicero admired Tully, are semantically significant, then these sentences as wholes will be non-synonymous, even though their individual words pairwise are synonymous.

This idea of Putnam's was seconded by Kaplan and elements of it can also be found in Geach.[14] Below, I'll first explain the modern development of the idea in Richard's and Fine's work, and then §5 will more briefly survey some other places it appears. The authors we'll consider don't all work out Putnam's guiding idea in the same way, and they use varying terminology: "logical potential" (Taschek), "co-relativized" terms (Richard), "coordination,"

"representing as the same," and "strict coreference" (Fine), "explicit coreference" (Taylor), "coindexed expressions" and "grammatically determined coreference" (Fiengo and May), and "*de jure* coreference" (Schroeter, Pinillos, Recanati). The last phrase seems to be trending most strongly, so I've based this chapter's title on it. I prefer to say "codesignating" rather than "coreferring," though, as to some the term "referring" has specific connotations that aren't essential to the phenomena we're exploring (even after we restrict our attention to singular terms, as we will here). These readers would resist talk about variables or anaphoric pronouns *referring*. "Designating" is more readily understood to have the needed generality.

3

Richard's engagement with Putnam's idea begins in his (1983). This paper became famous for its phone-booth scenario, which presented an intuitive difference in acceptability between:

(16a) I believe that you are in danger.
(16b) The man watching you believes that you are in danger.

despite the fact that these differ only in the substitution of codesignating expressions *outside of* any opaque context.[15] That point was only subsidiary to Richard's main goal in the paper, though, which was to explore the idea that:

(17a) I can inform you of her danger via the telephone.
(17b) I can inform her of her danger via the telephone.

differ semantically in such a way as to not be substitutable in the complements of attitude reports. For Richard at this time, sentences (17a) and (17b) do have the same *content*, but he makes the attitude verbs they may be embedded under become sensitive to their differing *character*. (We won't dwell on the details of how this works, because as we'll see, Richard soon moved to a different and more general strategy.)

Sentences (17a) and (17b) are of the form ...you...her... and ...her...her..., where all the pronouns are codesignating, and thus they parallel our examples:

(1) Cicero admired Tully.
(5) Cicero admired Cicero.

The only differences are that in Richard's examples, the pronouns occur in what may arguably be an intensional context (inform _____ of her danger) – this plays no role in his discussion – and his examples concern pronouns rather than proper names. This limitation was essential to the proposal he advanced in the 1983 paper, but was lifted when he developed these ideas further (and differently) in his 1990 book.

Richard (1987) begins to extend his claims about (17ab) to the open (quasi-English) expressions:

(18a) Juan said that (he observed *x* and then *y* but he wanted to observe *y* and then *x*).
(18b) Juan said that (he observed *x* and then *y* but he wanted to observe *x* and then *y*).

and:

(19a) `Juan said that (`*`x`*` chased `*`y`*` and then `*`y`*` chased me).`
(19b) `Juan said that (`*`x`*` chased `*`y`*` and then `*`x`*` chased me).`

arguing that each pair may differ in truth-value even when the variables *x* and *y* are assigned the same designation. The formal mechanisms Richard proposed in (1983) won't explain this result, since the variables don't differ in character. We don't get Richard's developed account of how this might work until Richard (1990). (Similar examples are discussed there at pp. 153, 200ff.)

Richard (1986–1987) offers an intermediate story, introducing a construction he calls "R-structures" (these are akin to the DRSs used in Kamp, 1984–1985; 1990; and Asher, 1986; 1987; 1989). We won't dwell on the details of these either. But I do want to point out several elements in this paper that will reappear in later contributions by Richard and others. First, Richard proposes:

> Let us talk of two occurrences of a term being co-relativized, when they are treated as if they were occurrences of the same existentially bound variable.
>
> (Richard, 1986–1987, p. 251)

Second, Richard associates several "levels" of information with a report (1986–1987, pp. 253ff.). The first level is initially described as just the Russellian content of the complement (pp. 244, 253) but then later is said to be that complement's R-structure (p. 256). This means that it will encode patterns of how terms recur in that complement. The second level encodes patterns of how terms recur in *multiple* reports, within a larger discourse. This roughly corresponds to Fine's "weak *de dicto*" reading of reports, which we'll discuss later. The third level encodes patterns of how terms recur *also in our unspoken background convictions* about the attitudes of others (pp. 257ff.). We will see something like this in Fine and other authors, too. In this paper, Richard only regards the first of these levels as giving the strict truth-conditions of the report (pp. 244, 256).

The account of attitude reports Richard arrived at in his (1990) is complex, and will be best understood against the background of other views in the air at the time.

Instead of the Fregean strategy of having beliefs and other attitudes be relations to a single fine-grained object, many philosophers in the 1980s began following Kaplan's and Perry's suggestion to think of attitudes as relations to *both* a coarse-grained Russellian proposition, *and* something that added additional grain, such as a *guise* under which that proposition is grasped, or a *sentence in Mentalese* which expresses it, or a series of *mental files* deployed in thinking it.[16] For concreteness, let's work with the Mentalese sentence version of this idea. So the underlying facts about a subject, that a belief report would aim in some way to describe or summarize, would be that he stands in some belief-like accepting/endorsing relation to a coarse-grained Russellian proposition about Cicero and admiring *with* some Mentalese sentence like `Cicero admires Tully`. *Exactly how* belief reports aim to describe or summarize these underlying facts, different theorists gave different accounts of. To help separate what these accounts agreed about from the different proposals they offered of the predicate `believe`, let's call the underlying belief-like accepting/endorsing relation "doxizing." One doxizes a Russellian proposition with a Mentalese sentence.

Salmon (1986a) offered a straightforward account of the relation between doxizing and the predicate `believe`: in the terms we're employing here, his view was that a report:

(20) `Anita believes that` *`S`*`.`

is true iff *there exists some* Mentalese sentence *T* such that Anita doxizes the Russellian proposition expressed by *S* with that sentence *T*.[17] Salmon is not always explicit whether this is meant to be an account of the *logical form* of the belief report, or only its truth-conditions. On the first construal, belief reports would themselves contain existential quantifiers over Mentalese sentences (or guises); these would just have no overt pronunciation. Also, the predicate `believe` would itself really contribute a triadic semantic value, contrary to its surface appearance. Some have interpreted Salmon this way;[18] but the balance of evidence speaks more strongly for the other construal, where `believe` really only contributes an atomic dyadic relation, as it appears to (see Salmon, 1986a, pp. 5–6, and clauses 35–36 on pp. 146–147; also his 1986b, pp. 32–33), and the existential quantifier only enters into Salmon's explanation of the report's *truth-conditions*, not its actual syntax or semantic structure.

Many theorists agreed with Salmon that in at least some cases, belief reports are completely non-committal about how (with what kind of Mentalese sentence) their subject doxizes the attributed proposition. But these theorists resisted saying that it was *always* so. In many other cases, they claimed, belief reports make more specific commitments about the third argument of the doxizing relation. Many went no further than that, saying only that the particular complement clause used in the belief report is meant to somehow "display" (without designating or literally expressing) the kind of Mentalese sentence the subject doxizes with.[19] But a handful of theorists made specific proposals about how belief reports constrain or supply the extra argument.

The earliest of these proposals came from Schiffer, who suggested that (20) be analyzed as:

(20′) $\exists T\,(\phi T \wedge$ Anita doxizes σ with $T)$

where σ is the Russellian proposition expressed by *S*, and *T* is a Mentalese sentence or guise meeting some condition ϕ (see esp. Schiffer, 1977; 1992). Because ϕ is a contextually supplied element that isn't overtly designated, views of this style became known as "hidden indexical theories" (HITs). I will refer to this as "Schiffer's HIT," though he doesn't straightforwardly endorse the view. He merely says it's the best option if you want to embrace compositionality about what complement clauses express. Over the course of many papers, Schiffer voices a series of objections against "his" HIT.[20] Three of these I'll mention just in passing: we shouldn't advocate such a view for speech reports, there are no good candidates to play the role of "guises," and the view would need supplementation to deal with empty names. Three other objections it would be useful to refer to later, so I'll label them:

S1 the "meaning-intention" problem: Ordinary speakers aren't aware of quantifying over, describing, or designating guises. So it's implausible that they have the semantic intentions they'd need to have, if a HIT of attitude reports were correct.

S2 the "logical form" problem: Schiffer's HIT analyzes `believe` in terms of the triadic predicate `doxize`. But Schiffer says that syntactic evidence, and ordinary intuition, speak against analyzing `believe` as having more arguments than just a subject and a complement (plus perhaps a time, which we're here ignoring). This complaint is discussed further in Ludlow (1995), Schiffer (1996), and Ludlow (1996).

S3 the "validities" problem: The argument `Anita believes everything Juan says; Juan says that Cicero is alive; so Anita believes that Cicero is alive` seems intuitively to be valid. But if the first premise says merely that if Juan says σ, Anita will doxize σ under *some guise or other*, then the premises don't entail a conclusion that constrains how Anita doxizes. If on the other hand, the first premise says that Anita will doxize σ *under the same guise that Juan says it*, then the conclusion follows only if it involves the same guise that's invoked in the second premise. And Schiffer's HIT doesn't guarantee that it will. (On his account, the premise and the conclusion will each have their own ∃.)

Crimmins and Perry *do* endorse a version of HIT.[21] On their account, though, speakers often directly specify the subject's Mentalese sentence *T*,[22] rather than contextually supplying a condition ϕ that restricts a quantifier over *T*. Though *T* is directly specified in *that* sense, it still is not designated by any pronounced syntax: in Perry's terminology, it's an "unarticulated constituent" of the report.[23]

Recanati (1993, chs 18–20; 1995) defended a view with important similarities to Schiffer's and Crimmins's HITs, but also some notable differences (Crimmins, 1995c, pp. 201–203, compares their views). Like the others, Recanati wanted belief reports to often assert, and not merely implicate, constraints on how (with what Mentalese sentence or guise) the subject doxized. Also like the others, and unlike Fregeans, he wanted component expressions of the report's complement to have their ordinary designations – thus making it unproblematic to say things like `Anita believes that Cicero is alive, but he isn't`. The strategy Recanati pursues to reconcile these goals is not to make the predicate `believes` take a third argument, which the report quantifies over or contextually supplies. Instead, he proposes that the complement clause as a whole usually expresses an enriched, "quasi-singular" proposition that correlates ordinary designata (like Cicero) with the subject's Mentalese representations of them.[24] For example:

(21) `Anita believes that Cicero is alive.`

might express:

(21′) ⟨a dyadic believing relation, Anita, ⟨the property of being alive, (Cicero, *the mental representation of Cicero that Anita associates with the name* `Cicero`)⟩⟩

Recanati denies that the enrichments are determined by the conventional meaning or the truth-conditional content that `Cicero is alive` would express in isolation. Rather it depends on the context of the report, including previous conversational moves and other specifics about how that sentence is embedded. (For example, it may depend on how we designate Anita.)

Recanati's view will help us contextualize Richard (1990). The spirit of the theory Richard develops there and in later work[25] is reminiscent of Carnap's account from §2 above:

> [W]hen we ascribe a belief saying `so and so believes that` *S*, we offer the sentence *S* as a *representation* or *translation* of what realizes one of so and so's beliefs. Attitude ascription thus presupposes something like a "translation manual," one keyed specifically to the individuals to whom attitudes are ascribed … The semantic rule governing belief ascriptions is something like: *x* `believes that` *S*, used in a context *c*, is true just in case *S*, relative to context *c*'s translation manual, translates some belief-realizing state of (the referent of) *x*.[26]

Concerning Richard's use of "translate," this should not be understood to mean that the expressions in the report have to be *conventional* translations of any expressions the subject would accept (see, e.g., 1990, pp. 134–135). Concerning Richard's use of "represent," this can also be confusing. He uses that term in several ways. "Representations" are groupings of token Mentalese expressions from which (what I'll call) private RAMs are composed (pp. 181–190; this is defined below). This is related to Richard's "representational systems," which are sets of all a subject's private RAMs (p. 137). Another use of "represent" (invoked in the above quote) is for the relation that a public RAM stands in to the private RAM that is its image under a given correlation function (pp. 139, 144). Importantly, Richard (unlike Crimmins) doesn't think that reports *designate* or directly specify subjects' Mentalese representations.

We need to look at the details of how Richard spells this out. They place him somewhere between Schiffer and Crimmins, on the one hand, and Recanati, on the other. Like Recanati, Richard wants complement clauses to express something richer than mere Russellian propositions. But unlike Recanati, he wants a compositional story about what the complement clauses do express. And unlike Recanati and Crimmins, he thinks reporters generally don't transact with other subjects' specific Mentalese representations. So rather than have the complement of (21) express a proposition enriched with Anita's Mentalese, Richard proposes it's instead enriched with *the English name* `Cicero` that was used in the report.[27] At first (1989; 1990; 1993) he called these enriched propositions "Russellian Annotated Matrices" (RAMs), but later he calls them "articulated" (1995) or "sentential" propositions (2006). He calls the components of these RAMs – the pairings of individual words with their designations – "annotations."

How do we get from a proposition enriched with English vocabulary, which the report's subject need not understand, to information about how the subject does mentally represent Cicero? Let's call the RAM expressed by the report's complement the "public" RAM. Let's call the Mentalese RAMs, with which subjects are claimed to doxize, "private" RAMs. Richard posits "correlation functions" that take us from the public RAM to a private RAM. We can understand him as proposing that (21) expresses:

(21″) $\exists f\,(\phi f \wedge \text{Anita doxizes}^{\star}\ f(\sigma^{\star}))$

Here, doxizing* is a dyadic relation Anita stands in to private RAMs that she "accepts."[28] $\sigma^{\star}$ is the public RAM expressed by the complement `Cicero is alive`: the fusion of a Russellian proposition with those very English words. f is a correlation that takes us from $\sigma^{\star}$ to some private RAM. The report doesn't directly specify f, but it does supply restrictions ϕ, in a context-dependent and not-overtly-pronounced way. In this regard, the view resembles Schiffer's HIT.

I've introduced Richard's theory this way to highlight respects in which it agrees with Schiffer, Crimmins, and Recanati. But in fact this is not quite the form that Richard himself employs. Rather than have the context supply an argument ϕ, Richard instead folds the ϕ-role into the relation I called "doxizing*," giving a single (contextually varying) relation that I'll call "doxphizing*." So Richard's own version of (21″) looks more like:

(21‴) $\exists f\,(\text{Anita doxphizes}^{\star}\ \sigma^{\star}\ \text{under}\ f)$

The truth-conditions of this are the same as (21″). Because doxphizing* includes the contextual restrictions on acceptable correlations, Richard's account is one where the predicate

`believe` expresses different relations in different contexts. (On Schiffer's and Crimmins's HIT, by contrast, `believe` is not context-sensitive, though the report as a whole is; and on Recanati's account, it's the report's complement that's context-sensitive.)

Let me point out some details and refinements.

- Richard understands his *correlations* as functions that map annotations to annotations, in a way that in general must preserve Russellian content and sometimes has to also obey other contextual restrictions (captured above by ϕ or doxphizing*). What will be important for our discussion is that these are *functions* part of whose effect is to map *expression types* in the report language to types of Mentalese expressions (see Richard, 1990, ch. 3, n. 11; Crimmins, 1992a, pp. 191–192; Sider, 1995, p. 503; Soames, 1995, n. 12; 2002, ch. 7, n. 12).

  A consequence of this is that when (non-demonstrative) expressions recur in the report's complement, as `Cicero` does in:

  (5*) `Anita believes that Cicero admired Cicero.`

  then the report commits to the subject also having recurring Mentalese expressions in her private RAM (see Richard, 1990, pp. 138–140, 201–202, 217–219). Hence, (5*) attributes a cyclic thought to Anita, one from whose content she could infer:

  (10) `Someone admired himself.`

  This prediction should seem familiar from our summary of Richard's three earlier papers, at the start of this section. It corresponds to a claim I'll label F3 when discussing Fine. (Richard also says that complements containing pronouns like `himself` always attribute cyclic thoughts: 1990, p. 218.)

  As we'll discuss, these commitments are controversial. For example, we observed that Salmon and Soames only allow complements like (3b)/(3b′) to attribute cyclic thoughts. They deny that the complements of (9bc) or (5*) have that structure.

  The rest of Richard's account does not *force him* to understand correlations in the way described here. He could instead define them on expression *occurrences* (see Crimmins, 1992a, pp. 195–196; 1995a, p. 387), or he could work with *relations* here instead of functions (see Sider, 1995, §6). Then recurring expressions in a complement needn't attribute cyclic thoughts. But Richard made this choice deliberately: in part to underwrite the truth-conditions he predicted in the earlier papers, and for further reasons he spells out in (1993, pp. 117ff.).

  Eventually, though, Richard came to have second thoughts about this, and in (2013b, pp. 8–11) he suggests that for iterated reports, the commitment described above only holds for *one reading*. He also countenances other readings, that permit recurring expressions in the complement to be "indexed to a subject higher in the sentence," and thus to possibly "translate" diverse Mentalese expressions from a more proximal subject's private RAM.[29]

- The fact that Richard's presentation makes `believe` indexical gives him other troubles with iterated reports. Suppose I observe Juan asserting (21), and then, in another context, report `Juan said that Anita believes that Cicero is alive`. Since my context is not the same as Juan's, it's unlikely that we have the same restrictions on acceptable correlations. That means that `believe` in my mouth won't express the same doxphizing* relation that it did in Juan's mouth. This motivates Richard to propose

that `believe` in the complement of my report contributes *its character* rather than its content to my report's public RAM (1990, pp. 165–167, 245–246; 2006, pp. 259–60). (And so too for other attitude verbs.) This is why I said above that correlations "*in general* must preserve Russellian content": here we have some exceptions, where it's instead the expression's character that needs to be preserved.

This raises several issues. First, why make a special exception just for attitude verbs? Richard ends up suggesting a parallel treatment of gradable adjectives and some uses of expressions like `foreign` and `the local` (2006, pp. 260–262; 1993, n. 3). Second, this aspect of Richard's account leads him to a *relativist* view of attitude reports (1990, §4.4), where *the content* of a report needs a further contextual parameter before it is truth-evaluable. Richard welcomes this commitment (see further his 2008; 2015, chs 5–7), but others may not.

I will ignore this aspect of Richard's theory.

- In (21″) and (21‴) we had an $\exists f$ in the content of a single report. In fact Richard's considered view is that sometimes we only quantify over correlations *at the discourse level*, and thus might reuse the same correlation *f* in several reports (1990, pp. 143, 175–180, 235–243; see also 1986–1987, pp. 69ff.). This corresponds to a move I'll label F4 when discussing Fine, and helps answer Schiffer's objection S3 to HITs, mentioned above (see Richard, 1986–1987, pp. 69, 75; 1990, pp. 148–149; 1993, pp. 118–120; contrast Crimmins, 1995a, pp. 392ff. esp. n. 11). It also means that Richard is proposing that `believe` expresses a *triadic* relation (with an extra argument place for the correlation *f* supplied by the discourse). So he has to confront Schiffer's objection S2.

Most of the critical attention to Richard's theory focused on difficulties about how context is supposed to supply restrictions on correlations. Some authors made complaints reminiscent of Schiffer's objection S1 to HITs (see Saul, 1993; Clapp, 2000; Soames, 2002, ch. 7; also Richard, 1997, §11). Soames (1995; 2002, ch. 7) complained that Richard's initial account of restrictions gave reports implausible modal profiles. Sider (1995, §§2–4) and Soames (1995; 2002, pp. 179–191) described cases where conversants unwittingly accept conflicting restrictions in a single context, because they don't recognize a subject whose attitudes they're reporting under different guises (see also Nelson, 2005). These complaints pose serious challenges to Richard's theory, but they are orthogonal to the aspects of his theory that are interesting for this chapter, so I won't pursue them.

A handful of other critical discussions merit special mention. First is Soames (1987b, esp. §§6, 8). This addresses an early undeveloped version of Richard's views, but many of the complaints raised there are also ones Soames will raise against *Fine's* Putnam-inspired account. Second is Soames (1994), which discusses "sloppy" donkey anaphora (pp. 120ff.) and some examples we'll return to in §6 below. The third critical focus is a complaint that Church (1954, p. 165) made against Putnam's original proposal. Given the different "logical structures" that Putnam attributes to `All Greeks are Greeks` and `All Greeks are Hellenes`, the latter would turn out to be *inexpressible* in a language that has only a single predicate synonymous with `Greek` and `Hellene`. Church found this intolerable. I'm not sure that judgment is underwritten by our actual practice and assessment of translation. But it is a judgment that Salmon shares and attaches great weight to (Salmon, 1986a, ch. 4, n. 4; 2001, pp. 582ff.; 2010, n. 11; 2012, pp. 437–438). See also Soames (1987b, pp. 113–114). Richard addresses those kinds of complaints at (1990, pp. 155, 160–162, 167–171).

4

(Unless otherwise indicated, page references in this section are all to Fine 2007.)

Fine's engagement with Putnam's idea[30] began in his (2003), which developed into chapter 1 of his 2007 book. The problem that concerns him there is how to account for the semantic role of the variables *x* and *y*. One the one hand, they seem to function semantically the same. We understand the two sentences:

$$\forall xFx \quad \forall xFy$$

to have the same meaning, and so too the two predicates:

$$\lambda x.Fx \quad \lambda y.Fy$$

and arguably we should also so regard the two open formulas:

$$Fx \quad Fy$$

and the mere variables themselves:

$$x \quad y$$

On the other hand, the predicates:

$$\lambda x.Rxx \quad \lambda y.Rxy$$

have different meanings, and arguably so too should the open formulas:

$$Rxx \quad Rxy$$

Fine's way of distilling this issue is to say that whereas the two single variables *x* and *y* have the same semantic role, the *pair* of variables *x*, *x* has a *different* semantic role from the pair *x*, *y*. Fine explores existing accounts of the semantics of variables and argues that none of them do justice to that idea in a philosophically satisfying way. For example, one account will give each of the variables *x* and *y* as their semantic value the set of objects they range over. But applying that to the pairs *x*, *x* and *x*, *y* would give them the same meaning. We could solve that by giving *x* as its meaning instead *the pair of itself plus* the set of objects it ranges over; and similarly for *y*. Or less artificially, we could give *x* as its meaning a function from assignment functions *g* to the object that *g* assigns to *x*; and similarly for *y*. Those approaches *would* give the pairs *x*, *x* and *x*, *y* different meanings, because they'd give each of the variables on its own a different (though analogous) meaning. Fine's objection to this is not a *direct* insistence that *x* and *y* on their own must get *exactly the same* meaning. Rather it's that such proposals are philosophically unsatisfying, because:

> [T]he posited difference between the semantic values for *x* and *y* simply turns on the difference between the variables *x* and *y* themselves … [W]hat we secure on this approach, strictly speaking, is not a *semantic* difference, one lying on the non-conventional side of language, but a *typographic* difference, one lying purely on the conventional side of language. (p. 11)

Fine criticizes other proposals in a variety of ways.

His own solution gives up the idea that expressions can be given any meaning *on their own* that settles what the meaning of pairs (or longer sequences) of expressions is. Instead of determining the meaning of *x*, *y* as built from the intrinsic meanings of its parts, he proposes a theory that determines the meaning of *x*, *y* as in the first place given by how those expressions are *semantically related* (see esp. pp. 3, 22–24). Note that a relation between *x* and *y* (such as codesignating Cicero) can be re-factored into a function from the pair *x*, *y* to the presence or absence of the relevant semantic attribute; thus we can also understand Fine to be assigning semantic values to *sequences* of expressions, rather than (only) to expressions taken individually. (Fine prefers to call these "semantic connections" rather than "semantic values," but I will stick with the more traditional vocabulary.) As Fine's account evolves to handle binding variables with quantifiers, we get the revision that it's not mere sequences of expressions, unadorned, that get assigned values, but rather these together with a "coordination scheme" mandating that some free occurrences of the same variables in the sequence be interpreted the same. (This is to capture the different ways that our interpretation of $(\forall xFx) \wedge Gx$ and $\forall x(Fx \wedge Gx)$ will depend on the interpretation of the sequence *x*, *Fx*, *Gx*.) Fine proposes to generalize the approach that assigns to each variable the set of objects it ranges over. Instead he will assign to each *coordinated sequence of* expressions a set of *sequences of* semantic values.

Fine does not give a full and explicit semantics for his language, but here is what I understand him to be proposing.[31] See also Pickel and Rabern (forthcoming, §2).

- $[\![\ldots, R, \ldots]\!] = \{(\ldots, R\text{'s extension}, \ldots) \mid$ for each interpretation of the rest of the original sequence of expressions $\}$
- $[\![\ldots, \underline{x}, \ldots, \underline{x}, \ldots]\!] = \{(\ldots, d, \ldots, d, \ldots) \mid d$ any object in the domain *x* ranges over, for each interpretation of the rest of the sequence $\}$
- $[\![\ldots, x, \ldots, x, \ldots]\!]$, where the occurrences of *x* are not linked $= \{(\ldots, d_1, \ldots, d_2, \ldots.) \mid d_1, d_2$ any objects in the domain *x* ranges over, for each interpretation of the rest of the sequence $\}$
- $[\![\ldots, Rt_1t_2, \ldots]\!] = \{(\ldots, \text{true}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, \Delta, d_1, d_2, \ldots)$ such that $(d_1, d_2) \in \Delta\} \cup \{(\ldots, \text{false}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, \Delta, d_1, d_2, \ldots)$ such that $(d_1, d_2) \notin \Delta\}$, where Σ is the coordinated sequence $\ldots, R, t_1, t_2, \ldots$ (preserving any coordination links from the original sequence $\ldots, Rt_1t_2, \ldots$ in the natural way)
- $[\![\ldots, \neg\phi, \ldots]\!] = \{(\ldots, \text{true}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, \text{false}, \ldots)\} \cup \{(\ldots, \text{false}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, \text{true}, \ldots)\}$, where Σ is the coordinated sequence $\ldots, \phi, \ldots$ (preserving any coordination links in the natural way)
- $[\![\ldots, \phi \wedge \psi, \ldots]\!] = \{(\ldots, \text{true}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, \text{true}, \text{true}, \ldots)\} \cup \{(\ldots, \text{false}, \ldots) \mid [\![\Sigma]\!]$ contains one of $(\ldots, \text{true}, \text{false}, \ldots)$, $(\ldots, \text{false}, \text{true}, \ldots)$, or $(\ldots, \text{false}, \text{false}, \ldots)\}$, where Σ is the coordinated sequence $\ldots, \phi, \psi, \ldots$ (preserving any coordination links in the natural way)
- $[\![\ldots, \forall x\phi, \ldots]\!] = \{(\ldots, \text{true}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, d, \text{true}, \ldots)$ for every object *d* in the domain *x* ranges over$\} \cup \{(\ldots, \text{false}, \ldots) \mid [\![\Sigma]\!]$ contains $(\ldots, d, \text{false}, \ldots)$ for some object *d* in that domain$\}$, where Σ is the coordinated sequence $\ldots, \underline{x}, \underline{\phi}, \ldots$ (preserving any coordination links from the original sequence $\ldots, \forall x\phi, \ldots$ in the natural way, and additionally merging in further links between the new occurrence of *x* and any free occurrences of *x* in ϕ)[32]

Chapter 2 of Fine (2007) turns to giving semantics for languages with names or constants. There Fine aims to apply the fundamental ideas of the account just sketched to examples like our:

(1) `Cicero admired Tully.`
(5) `Cicero admired Cicero.`

and he also proposes a semantic difference between these, even if the expressions `Cicero` and `Tully` would have the same meaning taken individually.[33] An obstacle to this proposal is that whereas variables are associated with *a range* of values, and so we can "coordinate" different variable occurrences by mandating that their ranges vary in lock-step with each other, names take only *a single* semantic value. So we can't use the varying-in-lock-step mechanism to distinguish the semantic value of the pair `Cicero`, `Cicero` from that of the pair `Cicero`, `Tully`. Fine admits that this makes his use of "coordination" to talk about semantically required codesignation for arbitrary expressions a bit awkward (pp. 39ff.).

As a result, the semantics we get in Fine's chapter 2 looks different from the account sketched above. Fine here works with an extension of the familiar structured Russellian account of meaning.[34] The extension is that when an object appears several times in a structured proposition,[35] there is the option of adding a "coordination wire" linking those different appearances.[36] This would enable us to represent the meaning of (5) as an extended, possibly-wired structured proposition where the two appearances of Cicero *are* linked, and the meaning of (1) as a possibly-wired structured proposition where they aren't.[37]

Some comments about this.

- The presence of wires in the meaning semantically encodes that multiple arguments to a (possibly complex) predicate are supplied by the same value. In principle, one might also consider the possibility of semantically encoding that multiple arguments are supplied by *different* values. Soames mentions such an idea in (2012, p. 255) (but see also his n. 14; and Salmon, 2012, p. 409, which offer related but different ideas). This possibility is not pursued in any of the work surveyed here. In the possibly-wired structured propositions Fine is working with, the alternative to a semantic wire explicitly requiring several arguments to be the same is instead just semantic neutrality, not requiring the arguments to be the same but not forbidding it either.
- We need to distinguish the possibly-wired structured proposition where multiple appearances of Cicero are explicitly unlinked, from the coarser, Russellian proposition that *carries no information about* whether those appearances are linked.
- Fine's use of the term "uncoordinated" is not consistent. He most often uses this to mean (i) a possibly-wired structured proposition that explicitly lacks some link (pp. 54–55, 78, 83, 96, 111, and perhaps 69); but other times he uses it to mean (ii) a coarser, Russellian proposition that is silent about what arguments may be linked (pp. 52, 56–59, 77). Fine also calls (i) "negative coordination" (p. 56), and Salmon (2012, n. 40) calls it "withheld coordination."
- Fine doesn't immediately say how to integrate the treatment of names in chapter 2 with a semantics for quantifiers. King (2007) has an account of structured propositions using "wires" to capture which quantifiers bind which argument positions.[38] This is ostensibly

> a different use of wires than Fine is working with, but if Fine chose to say that multiple variable occurrences bound by a single quantifier should always be coordinated, then King's wires could be viewed as a special case of Fine's wires.[39]

When several expression occurrences have a meaning that positively links their designata in the way described here, Fine says that the expressions "strictly corefer," in the sense that it is *semantically required that* they codesignate (see pp. 43, 46–47, 50–51, 59, 123; and his 2010d); and he says also that they "represent their objects *as the same*," which he distinguishes from the mere attribution of identity to those objects (pp. 39–40). Generally, recurrences of a single name like `Cicero` will bear these relations, but that is not necessary nor sufficient for doing so.[40] Fine offers as a "good test" for the presence of these relations considerations like this:

> An object is represented as the same [by several expression occurrences] in a piece of discourse only if no one who understands the discourse can sensibly raise the question of whether it is the same.[41]

Fine doesn't officially introduce any operators or predicates that are truth-conditionally sensitive to the presence of semantic wires of the sort we've described. In fact, he says:

> [T]he coordinative aspect of the coordinated content of a sentence, such as "Cicero wrote about Cicero," is entirely lacking in any special descriptive or truth-conditional character and relates entirely to how its truth-conditions … are to be grasped. (p. 59; contrast ch. 4, nn. 10 and 11)

But one would naturally expect the semantic differences Fine posits between (1) and (5) to contribute in some way[42] to a difference in truth-conditions between attitude reports for which they are the complement clauses:

(1*) `Anita believes that Cicero admired Tully.`
(5*) `Anita believes that Cicero admired Cicero.`

And in chapter 4, Fine does discuss two readings of attitude reports on which (1*) and (5*) may have different truth-conditions. His "weak *de dicto*" reading has coordination among expressions in a report's complement – as in (5*) but not (1*) – iff cyclic thoughts are being attributed (pp. 91, 102ff.). His "strong *de dicto*" reading has an even tighter connection between the expressions in the complement and expressions in the subject's linguistic or mental repertoire. However, Fine acknowledges at several places that he hasn't given, and doesn't know how to give, a compositional semantics for these reports.[43]

One noteworthy feature of the example reports Fine discusses in his chapter 4 is that they sometimes involve coordination across the complements of multiple attitude verbs, perhaps even ones involving different subjects.[44] If we assume that all and only occurrences of the same variables are coordinated in the following, Fine would regard these reports as having different truth-conditions (on some readings):

(19c) `Juan said that` *x* `chased` *y*, `and Anita wondered if` *y* `chased me.`
(19d) `Juan said that` *x* `chased` *y*, `and Anita wondered if` *x* `chased me.`

even when *x* and *y* are assigned the same designation.

A full appreciation and assessment of Fine's views about attitude reports has to consider his account of how the analogue of linguistic coordination occurs in thought, too – what I'm calling cyclic thought-contents. Fine counts it as a strength of his approach that the same fundamental ideas drive the story of both linguistic content and cyclic thoughts. But for several reasons, including limited space, I propose to ignore this part of Fine's story, and to keep our focus on proposals about the semantics of public languages (whether natural or formal). Let's continue on with merely an intuitive grasp of cyclic thoughts. They are the kinds of thoughts that subjects who consider and assent to (5) normally manifest, but subjects in `Paderewski` cases don't. (I discuss these thoughts further in my 2016.)

I've mentioned several key elements in Fine's account that are referenced elsewhere in this chapter. Let's gather and label them:

F1 Officially, Fine doesn't introduce operators or predicates whose truth-conditions are sensitive to coordination in their arguments. But one assumes that a full story about his "weak" and "strong *de dicto*" attitude reports would involve such, presumably by way of a difference in what possibly-wired structured propositions are expressed by different complement clauses (though see note 42 above).

F2 There are natural reasons for Fine to say, as he seems disposed to say, that multiple variable occurrences bound by a single quantifier are always coordinated.

F3 On some (readings of) attitude reports, expressions in their complement are coordinated iff they attribute cyclic thoughts. I'll only be referring elsewhere to the left-to-right half of this claim.[45]

F4 Fine allows coordination across the complements of multiple attitude verbs (even ones with different subjects). Recall that Richard also aimed to achieve this, by saying we sometimes only quantify over correlations at the discourse level.

I've proposed already to ignore one central element to Fine's thinking, namely:

- his specific account of cyclic thoughts.

I will also pass over the connections Fine draws between semantic wires and semantic requirements (see the citations a page ago), as well as:

- how he connects these wires to ideas about what's "transparent" to a competent language-user, or his epistemology of language use more generally (pp. 49, 60–65). I'm sympathetic to many of Fine's claims about what's "semantically required," but would resist treating knowledge of such requirements as necessary conditions for competence or understanding.[46]

In a moment, I'll identify a third element in Fine's thinking that I'll also be ignoring. And in §7 below, I'll demonstrate a language that (*contra* point F1 above) does have predicates whose application is sensitive to semantic wires among their arguments.

Fine developed his account further in his (2010d), and in responses to his critics.[47]

Fine presents his account as a form of "Referentialism" or "Millianism" (pp. 5, 37, 45, 53), and contrasts it to Fregean accounts (pp. 35–37, 42, 58–60). Some of his commentators challenge this, and suggest his account may be closer to Fregeanism than it is to familiar

forms of Millianism (see also Richard's, 1990, pp. 147–154 comparison of his own view to Fregeanism). The taxonomic question here isn't especially interesting in itself.

More pressing is another complaint voiced by several critics, namely that Fine's account doesn't seem to offer any way to semantically distinguish:

(1) `Cicero admired Tully.`
(22) `Tully admired Cicero.`

or:

(23a) `Cicero was an orator.`
(23b) `Tully was an orator.`[48]

If these sentences occur as *part of a larger discourse containing other occurrences* of the names `Cicero` or `Tully`, then Fine's account as developed so far *does* distinguish discourses containing (1) from ones containing (22), and similarly (23a) from (23b) (see Fine pp. 52–53). But what if the sentences *aren't* part of such larger discourses (see Soames, 2010, p. 470; 2012, pp. 245, 250)? There is considerable pre-theoretic pressure to still see some semantic difference between (1) and (22), and reluctance to allow our story about how (1) relates to (22) to fundamentally differ from our story about how it relates to (5). Of course, standard Millians already deny there is any semantic difference between (1), (22), and (5). But they've built up a collection of explanatory tools to make those denials more palatable. Fine's account may have encouraged us to think we wouldn't need those tools; and if he after all needs to resort to them to accommodate intuitions about (1)/(22) and (23a/b), this may undermine some of the motivation he offered for the extra semantic machinery. This is a complaint Soames voices several times (in his 2010, pp. 473–474; 2012, pp. 234, 237–238, 262–263; see also his 1994, pp. 132–133).

One point Fine can make in reply is that his account would at least distinguish *embeddings* of (1) and (22) in attitude reports, on his "strong *de dicto*" reading of those reports. Admittedly, we're only offered a skeletal story about how that reading works. In Fine (2010b) he develops a more ambitious response, which involves coordination of expression occurrences in utterances of (1) not only to discourses in which it's *actually* contained, but also to all discourses in which it *might possibly* be contained.[49] If one gets over the extravagant multiplying of meanings it requires, this move offers powerful armament to Fine: it enables him to treat isolated utterances of (1)/(22) or (23a/b) in the same way he's already prepared to treat discourses containing them and other occurrences of `Cicero` or `Tully`. I worry if the armament may be *too* powerful, and no longer sufficiently explanatorily disciplined. Pinillos (2015, pp. 327–330) presses other worries. But I won't try to sort these worries out. Instead, I'll just declare:

- this ambitious appeal to merely possible discourses

to be another element of Fine's thinking that I'll mostly ignore. I will point out, though, that similar proposals are made by Taschek (see notes 48 and 49 above), and in different ways by Richard (the "third level" of his 1986–1987) and even Pinillos.[50]

My own reaction to the complaints about (1)/(22) and (23a/b) is to acknowledge that the novel semantic structure that Fine and/or Richard advocate may not be a cure-all for every challenge to Millianism. There may be sufficient reason to admit it nonetheless.

I won't attempt a systematic overview of other complaints that have been raised against Fine's account. But the papers by Soames cited in note 47 above deserve special mention, for two reasons. First, many of the complaints in these papers overlap ones in Soames (1987b), demonstrating shared challenges faced by Fine and by Richard. Second, some of Soames's complaints that we haven't yet discussed can be usefully framed against the way I've expositied Fine's account here.

One assumption behind some of Soames's arguments (such as 1987b, p. 113) is that attitude verbs will be truth-conditionally sensitive *at most* to coordination patterns local to their own complements. Bracketing the question of Soames's *entitlement* to that assumption (in 1987, before his opponents' views were elaborated), I observe that it is one that Richard and Fine resist: see move F4 and note 42 above. Admittedly, Fine does not give us any compositional semantics where this assumption is withheld. Such a semantics *can* be given (at least, if we suppress Fine's qualms about semantics being too "typographic," and also help ourselves to the machinery of "dynamic semantics"). But I won't have the space to give it here. Our discussion in §7 below will also fail to vindicate the ambitions behind F4.

Another assumption behind some of Soames's arguments (1987b, pp. 116–117; 1994, pp. 130ff.; 2010, pp. 473–474; 2012, pp. 243–244, 247–249, 250–251) is that attitude verbs on his opponents' accounts *will be* truth-conditionally sensitive to coordination patterns in their complements. That is, he assumes claim F3 above. But in fact, the full account offered by Fine permits attitude verbs to sometimes (on their "pure *de re*" reading) ignore those patterns. (We'll discuss this further in §6 below; see also the `value` operator I'll introduce in §7. As mentioned in note 43 above, Soames complains about this aspect of Fine's account. His discussion raises serious challenges that I won't attempt here to address.)

5

There are many differences in detail and in declared motivation between Richard and Fine.

One difference I'll point out is their approach to cyclic thoughts. Richard simply assumes we think with Mentalese sentences, and handles such puzzles accordingly. In Pryor (2016), I sort-of follow him, though at a greater level of abstraction: I appeal to the graph-theoretic structure of a subject's attitudes, rather than to any *concrete linguistic implementation* of that structure. Fine by contrast wants an explanation at the level of the *semantic content* of our thoughts, and so has to expand our conception of that content. Essentially, he can be understood as advocating that we count graph-theoretically distinctive relations among our attitudes as hitherto unacknowledged aspects of content. As I said before, Fine also wants his picture of the content of thought to be unified with his semantics for natural language.

Another difference is that Richard's account of the semantic differences between simple sentences like (1) and (5) – namely, they express different English-annotated RAMs – isn't "relational" in Fine's sense, but is instead of a kind that Fine will reject as too "typographic." Its advantage is that it enables Richard (unlike Fine, see point F1 above) to give a compositional (albeit context-sensitive) semantics for attitude reports. Given current technology, it's not clear how to offer such a semantics without provoking Fine's qualms about being too "typographic," or encumbered with conventional artifacts.

Despite these differences, there is also a good deal of shared ambition and predictions between Richard's and Fine's accounts. Fundamentally, both aim to explain how (1) and (5) can differ semantically, even when it's stipulated that `Cicero` and `Tully` don't differ in respects relevant to the content of isolated unembedded sentences like (23ab), where they

occur singly. When it comes to attitude reports, Richard can join Fine in denying that it's anything "intrinsic" to the content of the complement that constrains what thought is being attributed (Richard, 1990, p. 135) – though they develop this denial in different ways. Finally, the overlap in Soames's criticisms of them (mentioned at the end of the previous section) attests to the similar dialectical terrain they occupy.

In addition to the threads in Kaplan and Geach identified in note 14, other work that anticipates Fine's proposals – and does so especially closely – is Taschek (1992; 1995; 1998). I've already mentioned some points of contact in notes 44, 48, and 49; another is their shared rejection of a Compositionality Principle that tracks only the semantic contributions expressions make when they occur singly. An important respect in which Taschek diverges from Fine is that he takes the difference between (1) and (5) to merely be one of "logical potential," not to be part of their "information content."[51]

There is a range of other work since the late 1980s that has points of contact with Richard's and Fine's accounts. The other authors cannot be understood to defend a common position or thesis, but in each case there is some clear affinity with the Putnamian idea that guided Richard and Fine.

Some of this other work focuses on cyclic thoughts. For example, Campbell (1987–1988) talks of an inference "presuming" or "trading on" identity, without the subject needing any *identity judgment* or premise. Lawlor (2002) talks of "anaphoric thinking." See also Perry (1980)'s discussion of "internal identity," and work in the "mental files" tradition like Recanati (1993; 2012, esp. chs 8–9; 2013, esp. §4 which replies to Goodsell, 2013; and Recanati, 2015, §I).[52]

Other work does focus on *de jure* codesignation in language; however, some of the authors deny that the phenomenon makes a *semantic* difference. See for example the work by Schroeter cited in note 41. Taylor (2003b; 2015) contrasts "explicit" versus "coincidental" codesignation, and says that the former is "syntactically or structurally" marked. These differ in what they "manifest" or "display," and in their "dialectical significance," but not in their contributions to propositional content (see esp. Taylor 2003b, §§2 and 8; 2015, pp. 257–259). As explained above, Taschek (1992) and Richard (1986–1987) also hesitate to make such differences be semantically encoded.

Unlike them, Pinillos (2011; 2015) *does* claim that the phenomenon he's exploring makes a semantic difference. He uses the phrase "*de jure* coreference," meaning by it something narrower than I express with "*de jure* codesignation," since he says bound variables don't refer and so never corefer (2011, pp. 318–320, but see also his n. 10). But the underlying semantic mechanism Pinillos is studying is one he thinks *does* apply to bound variables, and to empty names (2011, pp. 317–318; 2015, pp. 324–325), as well as to ordinary names and to anaphoric but unbound pronouns, like `his` in the "strict" reading of (4a). Pinillos calls this mechanism "p-linking" (2011, pp. 317ff.). Though his terminology may suggest otherwise, Fine also wants his notion of "semantically required" or "strict" coreference to apply to bound variables and to empty names (see notes 39 and 35 above). One fundamental difference between Fine and Pinillos is that the former takes his phenomenon to be an equivalence relation, at least when we confine ourselves to intrapersonal cases (Fine, 2007, pp. 55–56, but see note 44 above). It is central to Pinillos's account, on the other hand, that the underlying semantic mechanism can be intransitive and non-Euclidean even in that domain. He gives examples like the following:

(24) `As a matter of fact, the orator Cicero is my brother Marcus; you will get to meet the great Marcus Tullius Cicero tonight.`

Pinillos would claim that the first and third name occurrences, and also the second and third, are p-linked, but the first and second are not.[53]

Pinillos states several "axioms" governing the behavior of p-linking. Some of these govern the relation between it and coreference, or between it and variable binding (Pinillos endorses a claim like F2 from §4 above). Most interesting is his Axiom 2 (2011, p. 318), which Pinillos understands to have something like F3 as a consequence. This is closely related to Pinillos's Principle of Attitude Closure, which he takes to be partly criterial for the phenomena he's exploring. This Principle says that when the complement of a belief report exemplifies *de jure* coreference, that report will entail a corresponding report using the complement's existential generalization. For example:

(5*) Anita believes that Cicero admired Cicero.

will entail:

(10*) Anita believes that someone admired himself.

Pinillos says this Principle may hold for more attitudes than just belief, though not for every attitude.

We'll discuss F3 and Pinillos's Attitude Closure in more detail in the next section.

Pinillos's other criteria for the phenomena he's exploring are principles that are reminiscent of Fine's claims about the epistemology of language use. Pinillos says that anyone who fully understands a sentence exemplifying *de jure* coreference will be in a position to infer its existential generalization, for example, from (5) to:

(10) Someone admired himself.

or from (3d) to:

(25) Terentia betrayed someone who was unhappy.

Pinillos says also that anyone who fully understands a sentence exemplifying *de jure* coreference will know of the relevant expressions that if they both manage to designate, they designate the same thing.[54]

My own stance towards these "criteria" is very guarded. Because I have different background views about the epistemology of language use than Fine and Pinillos – which dispute seems like it should be orthogonal to the issues we're exploring – I'm inclined to doubt that any linguistic properties meet the epistemic tests they propose. Perhaps refined and more qualified versions of these tests will prove more acceptable. But even in advance of knowing what those will look like, I do still find myself sympathetic to the possibility of a genuine semantic phenomenon in the neighborhood they're exploring. So my preference is to detach the epistemic criteria from the semantic proposals.

Goodsell (2014) shares some of my qualms about the epistemic commitments of Pinillos's criteria. She also complains that these criteria wouldn't be able to characterize *de jure* codesignation across multiple speakers or in non-declarative constructions (p. 298); also that, as Pinillos states them, the criteria don't sufficiently respect the fact that one can understand a belief report without oneself, or the report's subject, knowing that all its constituent expressions refer (pp. 304–305).

Finally, I'll note that Fiengo and May (2006) has similar motivation and points of contact with Richard's and Fine's views. But it also differs in complex ways that I can't explore here. (Additionally, Fiengo and May embrace some of the controversial views about the epistemology of language that I prefer to separate from the semantic proposals we're considering.)

6

Claim F3 and Pinillos's Attitude Closure have come up several times in our discussion. The status of these claims is contested.

Suppose Diego has a long dream in which, contrary to fact, he's married. Inside the dream, he often finds himself waking to the sight of scattered socks, and supposes this may have been done by a mouse. Being terrified of mice, he begins a nightly campaign to wake his more fearless wife so she can catch the sock-mover. But they never encounter a mouse. Finally, it emerges that Diego and his wife have all along been moving the socks themselves, while sleepwalking. Thus ends Diego's dream. Diego, now awake for real, reports his dreamt campaign with bemusement:

(26) `All along, I was hoping my wife and I would catch ourselves!`

The pronoun `ourselves` gets its reference from `my wife and I`, so these expressions should be *de jure* codesignative. But then the acceptability of the report would conflict with any claim like F3. In the scenario described, it was *not* the case that Diego all along bore a hopeful attitude that represented the chasers and the agents chased as one, and from whose aim he could infer `Someone catches themself`.

This example also puts pressure against Salmon's and Soames's views, summarized in §1 above. The fact that Diego has no real wife suggests that `my wife and I` occurs inside the report's complement, as it appears to, rather than taking wide scope. (The reflexive morphology of `ourselves` also suggests this.) So on a bound-variable analysis of referential dependency, the report has the form:

(26′) `All along I was hoping: (λ`*x*`. `*x*` would catch `*x*`) (my wife and I)`

And on Salmon's and Soames's accounts, any such report would *also* attribute a hope that represents the chasers and the agents chased as one.

Soames several times appeals to similar examples, with the form:

(27a) `Juan told Maria that he wasn't Juan/that man/him.`[55]

In all its variations, this is an acceptable report of a scenario where Juan attempted to mislead Maria about his identity: that is, he said to her something like `I am not Juan`, or `Juan is shorter than me`. Another of Soames's examples has the form:

(27b) `Each man told Maria that he wasn't that man.`[56]

Soames assumes that the singular terms in the complements of (27ab) should each be *de jure* codesignative with the subject of the report. Presumably, they will then be *de jure*

codesignative with each other; and if F3 is in place, they'll report Maria to have been told something from which she could infer `Someone is not himself` – contrary to Soames's description of the scenarios.[57]

Spelling this out more carefully, the assumptions that raise trouble are that:

(i) if two expressions are *de jure* codesignative with a third, they are also *de jure* codesignative with each other

(ii) if the expressions in the complement of (27ab) are codesignative with each other, those reports characterize Maria as having been told contents from which she could infer `Someone is not himself`. This is the claim I earlier labeled F3.

Some advocates of *de jure* codesignation deny that (ii/F3) always holds: see for example Recanati (2012, §9.2) and Goodsell (2014, pp. 302–304). Fine himself only endorses F3 for some readings of attitude reports.

Pinillos acknowledges examples like (27ab) (2011, pp. 315–316 and n. 41). Instead of rejecting (ii/F3), he prefers to reject assumption (i). He denies that *de jure* coreference (or p-linking) is an equivalence relation; so the singular terms in these reports' complements needn't be p-linked with each other, after all.

Like Pinillos, Soames also sees these examples as demonstrating a failure of Euclideaness and/or transitivity. He thinks we should see the referential dependencies in (27b) and some versions of (27a) as having the form:

(27c) `____ told Maria that he wasn't that man.`

This contrasts with other examples where referential dependencies have the form:

(28a) `Cicero expected that he would eventually admire himself.`

In (28a), `himself` can't be directly referentially dependent on `Cicero`, since if it were, it could not have reflexive morphology:

(28b) `Cicero expected that Terentia would eventually admire *himself/him.`

The main difference between Soames and Pinillos is that only the latter says these referential dependencies will always be semantically encoded. As I explained in §1 above, Soames thinks the dependencies show up in the semantics iff they're generated by variable binding. That will only partially overlap with Pinillos's ambitions for the notion of *de jure* codesignation.

Higginbotham and Heim also argued for a distinction in dependency patterns like (27c) versus (28a). Their examples have an advantage over Soames's, in that his case for denying `that man` is dependent on `he` in (27ab) turns on assumptions like (ii/F3) above (or, on his own view, on (Z2) and (Z3) from §1). Since the status of these assumptions is unclear, demonstrations that don't need them are more forceful.

Heim (1998, examples 26a–g and 27a–d) gives contrasts like these:

(29a) `Every student claimed that he called his mother before the teacher did.`

(29b) Every student claimed that he called his mother before the teacher did.

On Heim's view, the referential dependencies exhibited in (29a) invite the "strict" reading, where it's the teacher's calling *the students*' mothers that is being discussed. The referential dependencies in (29b) invite the "sloppy" reading, where it's the teacher's calling *the teacher's own* mother that is being discussed.

Higginbotham (1985, examples 68–74) gives contrasts like these:

(30a) They told each other they would succeed.

(30b) They told each other they would succeed.

On his view,[58] the referential dependencies exhibited in (30a) describe scenarios where the subjects said to each other `I will succeed` or `We will succeed.` The referential dependencies in (30b) describe a scenario where they said `You will succeed`, as is also unambiguously reported by:

(30c) `They told each other to succeed.`

However, though Heim and Higginbotham argue that these dependency patterns should be distinguished, they do not argue for their being semantically encoded. So far as I can see, their discussions just cited could be developed along the lines of either the Salmon/Soames stance from §1 above, or the Richard/Fine/Pinillos stance. (The former will have been more familiar to Heim and Higginbotham.)

7

We can get further insight into the Richard/Fine/Pinillos proposals by considering some ideas from computer programming. One obstacle to this is that many readers will have little fluency with the kind of programming we need. But we can work around that. Those readers who are acquainted with programming may be thinking of it on the model of a sequence of instructions. That is the "imperatival" model of computation. But there are other models, too, such as the "declarative" or "functional" model. The existence of this model of computation may be less familiar, but *what it understands computation as* is closer to ideas that philosophers and linguists are well-acquainted with, like formulas of predicate logic. On the declarative model, arithmetic expressions like $2 + 7 \leq 9$, $2 + 7$, and 2 all count as programs, which evaluate to true, 9, and 2, respectively. The set of inequalities $\{2 + x \leq 9, x > 5\}$ might be a program that evaluates to a set of assignments binding x to 6 or 7. The regular expression `/ima[a-z]*ng/` might be a program that evaluates to the set of English words {`imaging`, `imagining`}. And so on. There's nothing in either of these models of computation that prevents it from expressing or achieving all that the other can; their styles of doing so will just tend to be different. We'll work with the declarative model.

If readers have ever encountered a formal semantics for a programming language (from either of the models of computing just described, or another), it's likely to be of the form computer scientists call "operational semantics." This will have surface resemblance to a

proof theory, or a system of rules for rewriting the program text. (Those analogies have defects, but that's not important for our purposes here.) But there are other forms of semantics for programming languages. The one most similar to what philosophers and linguists think of as semantics is called "denotational semantics." If you've ever seen a denotational semantics for even a simple (yet Turing complete) programming language, you will have been struck by how much more mathematically sophisticated it is than anything appealed to in natural language semantics.

All of this – associating "programming" with the imperatival model of computation, thinking of formal semantics for programming languages in operational rather than denotational terms, and the sophistication of actual denotational semantics – can encourage the impression that the semantics of programming languages is profoundly different from, and will teach us little about, what we understand as semantics for languages like English (or Esperanto, or arithmetic).

However, I invite you to consider some notions from declarative programming that I will structure as a series of extensions to (a decidable fragment of) arithmetic. I will present these in a way that aims to maximize their familiarity. I won't present a formal semantics for these notions here. The extensions to arithmetic that one sees in a full-blown declarative programming language fall into "lightweight," "middleweight," and "heavyweight" categories. The "lightweight" additions don't require anything fundamentally new for their semantics. The "middleweight" additions do; but no more radically new than philosophers and linguists are already familiar with from Lewis (1979) and discussions of "dynamic semantics." The "heavyweight" additions include things like unlimited recursion, and giving a denotational semantics for these does require sophisticated techniques. But none of what we'll be doing here needs to make use of those notions. We can limit ourselves only to "lightweight" and a few "middleweight" additions.

Let's begin with restricted quantification, with which I assume familiarity. Throughout I'll use x, y, … as variables ranging over the domain $\mathbb{N}$. Later I'll use xs, ys, … as variables ranging over lists of $\mathbb{N}$, and f, g, … as variables ranging over functions of one or two arguments (usually mapping $\mathbb{N}$s to $\mathbb{N}$s, but sometimes with other types).

The sentences:

(31a) $\forall x < 7 \left(2 + x \leq 9\right)$

(31b) $\forall x = 7 \left(2 + x \leq 9\right)$

express truths, given the standard interpretations of $<$, $\leq$, $=$, $+$, and the numerical constants. We'll represent that by writing:

(31b) $\forall x = 7 \left(2 + x \leq 9\right)$ $\Rightarrow$ true

Our $\Rightarrow$ notation also permits us to state what values sub-sentential expressions have, on the assumed interpretation. For example:

(32a) $2 + 7$ $\Rightarrow$ 9

and:

(32b) $2 + x$ $\Rightarrow$ 9

on an assignment that binds variable x to the value 7.

An interesting difference between (31a) and (31b) is that the latter uses a restrictor that we know in advance is satisfied by exactly one value in the relevant domain. It will be useful to introduce a special syntax for this case:

(31c) `let` $x = 7$ $(2 + x \leq 9)$ ⇒ true

A natural extension of this syntax permits us to partially specify our assignments in the case of sub-sentential expressions, too, like (32b):

(32c) `let` $x = 7$ $(2 + x)$ ⇒ 9

We can specify assignments more fully by embedding uses of `let`:

(33a) `let` $x = 3$ (`let` $y = 4$ $(2 + x + y)$) ⇒ 9

which we might abbreviate as:

(33b) `let` $x = 3, y = 4$ $(2 + x + y)$ ⇒ 9

Now let's see how to express the same things using the syntax of the programming language Scheme.[59] This is how you write $2 + x + y$ in Scheme:

(34)
```
(+ 2 x y)
```

and this is how you write the equivalent of (33ab):

(33c)
```
(let ([x 3][y 4])
     (+ 2 x y))
```
⇒ 9

For later comparison, it will be helpful to note that just as you can say $\forall x\, \exists x\, (x = 9)$ in predicate logic, and inside the parentheses only the innermost effect on the binding of x will be operative, so too can you say:

(35a)
```
(let ([x 0][y 0])
     (let ([x 3][y 4])
          (+ 2 x y)))
```
⇒ 9

How this is described in programming circles is that the innermost `let` introduces new local bindings for x and y that "shadow" their more global bindings. These local bindings are in place only until the parenthesis that closes the innermost `let` expression.

(35b)
```
(let ([x 0][y 0])
     ((let ([x 3][y 4])+) 2 x y))
```
⇒ 2

What happened here is that the local bindings to x and y were in place only for the interpretation of the symbol + (where they made no difference). When we evaluate the operands of +, we use the more global bindings for x and y.

The Scheme programming model countenances several types of simple and compound values. Simple values include things like numbers, truth-values, and various functions; we won't concern ourselves with other simple values. Compound values include things like lists of other values; we won't concern ourselves with other compound values. In Scheme, a list can contain a variety of values, such as numbers, truth-values, and yet other (embedded) lists. But we will mostly work with homogeneous lists of numbers. Here is one way to build a list in Scheme:

```
(36a)  (list 2 3 4)                          ⇒  (2 3 4)
(36b)  (let ([x 3][y 4])
           (list 2 x y))                     ⇒  (2 3 4)
```

The syntax of Scheme includes *keywords* like `let`; simple expressions including *symbols* like `+`, *x*, and *y*, and *literals* like the numerical constants and `true` and `false`; and compound expressions which are lists of other expressions. Scheme *evaluates* symbols to the values those symbols are bound to, at that position and stage in the program. It evaluates literals to themselves. It evaluates lists of expressions by evaluating the head of the list, and if the result is a function, applying it to the argument values that the rest of the list evaluates to. (If the head of the list doesn't evaluate to a function, the list is not evaluable.) Lists beginning with keywords are evaluated in special ways.

Here are some more operations on lists. Note that here we bind a variable to a list rather than to a simple value. I'll follow the convention of using plural variable names like *xs* for this.

```
(37a)  (let ([xs (list 2 3 4)])
           (head xs))                        ⇒      2
(37b)  (let ([xs (list 2 3 4)])
           (tail xs))                        ⇒    (3 4)
(37c)  (cons 2 (list 3 4))                   ⇒  (2 3 4)
```

The `cons` function is so-named because it *cons*tructs a list, from a specified head element and rest of the list. This should now make sense:

```
(38)   (let ([xs (list 2 3 4)])
            [ys (cons 1 (tail xs))])
           ys)                               ⇒  (1 3 4)
```

The symbol `+` comes pre-bound to the standard addition function. Scheme also lets you build custom functions, like so:

```
(39)   (λ (x y) (+ 2 x y))
```

This evaluates to a function that accepts two arguments which it then adds to 2, and returns the result. (Scheme doesn't have any canonical way to display this function, so I have omitted the ⇒.) We can apply that function to values like so:

```
(40a)  ((λ (x y) (+ 2 x y)) 3 4)             ⇒  9
```

This behaves the same as (33c). We can also bind variables to functions that we build in this way:

```
(40b)  (let ([g (λ (x y) (+ 2 x y))])
         (g 3 4))                                  ⇒  9
(40c)  (let ([g (λ (x y) (+ 2 x y))]
             [x 3])
         (g x 4))                                  ⇒  9
```

λ-expressions can contain variables that are bound outside of them (variables that are "locally free"):

```
(41)   (let ([y 4]
             [f (λ (x) (+ 2 x y))])
         (list (f 3) (f 13)))                      ⇒  (9 19)
```

These free variables are understood in what programmers call the "lexical" rather than the "dynamic" style. That is:

```
(42)   (let ([y 4]
             [f (λ (x) (+ 2 x y))]
             [y 0])
         (list y (f 3) (f 13) y))                  ⇒  (0 9 19 0)
```

The innermost binding of *y* to 0 affects the interpretation of that variable in the final list expression; however, it doesn't affect the interpretation of *y* inside the definition of the applied function *f*. The other understanding, where the result of the above would be (0 5 15 0), is also coherent; but it's not what Scheme uses here.

Contrast (42) to:

```
(43)   (let ([y 4]
             [f (λ (x) (let ([y 0]) (+ 2 x y)))])
         (list y (f 3) (f 13) y))                  ⇒  (4 5 15 4)
```

Here the more local binding for *y* is inside the definition of *f*, so it *does* affect the results we get when applying *f*; also, that binding is in effect only *inside* the definition of *f*, so it *doesn't* affect the interpretation of *y* in the final list expression.

This finishes our introduction to (the core of) the "purely declarative" part of Scheme. The ideas exhibited here all fall under the heading of what I called "lightweight" additions to arithmetic. Next we move on to some "middleweight" additions.

Lists of the sort we've worked with so far are *static* values. Once built, such a value never changes. In this respect they are just like numbers and truth-values. However, Scheme also has a notion of a *mutable* list, which can at different stages of its existence contain different elements. We can build and mutate such a list like this:

```
(44)   (let ([xs (mlist 2 3 4)]
             [_ (set-head! xs 1)])
         xs)                                       ⇒  (1 3 4)
```

A few comments about this. First, note the different syntax: `mlist` rather than `list`. Second, this example contrasts with (38) where we bound *ys* to a version of *xs* with a new head, but *xs* itself would still have evaluated to the list (2 3 4). Third, the usefulness of `set-head!` is in the *side-effect* you get from applying it, not from its return value. In most Schemes, `set-head!` doesn't return anything useful, so we assign its return value to the "throwaway" variable _. Fourth, in introducing such values and operations into the language, we've abandoned what programmers call "referential transparency." This loosely coincides with what philosophers mean by that phrase. Consider:

```
(45)  (let ([f (let ([ys (mlist 0)])
                 (λ (x) (let ([y  (head ys)]
                              [_ (set-head! ys (+ 1 y))])
                          (+ 2 x y))))])
        (list (f 3) (f 3) (f 3)))                          ⇒  (5 6 7)
```

If you reason through the evaluation of this expression, you'll see that at each application of *f* to the constant argument 3, we end up adding a different head-value from the changing list *ys*. So the result of applying *f* is not determined by the values supplied to it as arguments.[60]

Scheme also has some built-in functions that return truth-values as their result. For example:

```
(46a) (let ([y 4])
        (equal? y 3))                                      ⇒  false
(46b) (let ([ys (list 2 3 4)])
        (equal? ys (list 1 3 4)))                          ⇒  false
```

Instead of `equal?` we could also have written `egal?`. With non-mutable values, there is no difference between these. But with mutable lists, we face a choice. Two variables might be bound to distinct mutable lists, which happen currently to contain the same elements. A mutation to one of the lists would not in itself have any effect on the other list. Should we count such lists as equal, since they now contain the same elements? Or should we say that they're unequal, since they are distinct, independently mutable containers? Scheme handles this by saying such lists are `equal?` but not `egal?`. Thus:

```
(47)  (let ([xs (mlist 2 3 4)]
            [ys (cons 1 (tail xs))]
            [zs xs]
            [_ (set-head! xs 1)])
        (list zs (egal? xs zs) (egal? xs ys) (equal? xs ys)))
                                                   ⇒  ((1 3 4) true false true)
```

We've been working with the notion of a mutable list *value*. But so far, our understanding of how *variables* work has stayed the same. Once a variable is bound to a value, it stays so bound. That binding may perhaps be "shadowed" by a more local use of the same variable symbol, but it continues to exist and will again be visible after the syntactic scope of the more local expression expires. But in fact, Scheme also has a notion of *mutable variable bindings*. It allows us to say:

(48)
```
(let ([y 4]
      [f (λ (x) (let ([_ (set! y 0)]) (+ 2 x y)))])
     (list y (f 3) (f 13) y))
```
⇒ (4 5 15 0)

Contrast (43), where the final element in the result was 4. Here we don't "shadow" the outermost binding of *y* with a new binding. Instead we *mutate* that very outermost binding. The result is that even *outside* the definition of *f*, after *f* has been applied, that new mutated binding will still be visible.

One of the reasons I chose Scheme for these examples is that it has *both* mutable values *and* mutable variable bindings. It's most common for programming languages to have only one of these. Here is an example of using both notions in Scheme:

(49)
```
(let ([xs (mlist 2 3 4)]
      [zs xs)
      [_ (set-head! xs 1)]
      [_ (set! xs (list 0 0 0))])
     (list xs zs))
```
⇒ ((0 0 0) (1 3 4))

In (47), we used the variable *xs* to mutate the *single list value* that both *xs* and *zs* were bound to; then when we evaluated *zs,* this change was visible. The same is true in (49); but after mutating that list value, we next mutate the *variable xs* to become bound to a new list. *zs* still stays bound to the old list.

At this point, the imaginative reader who's read the rest of this chapter will wonder whether there might be a way to make variables be even more intimately related than *xs* and *zs* were in the above examples: to not merely happen at one stage in the program to designate or be bound to the same value, but to be made *de jure* codesignative, even through mutations of either variable's binding.

The answer is that yes, this is possible, but it requires going beyond the most familiar features of Scheme.[61]

Consider:

(50)
```
(let ([xs (list 2 3 4)]
      [zs xs])
     (alias ([ws xs])
            (let ([_ (set! xs (list 0 0 0))])
                 (list xs ws zs))))
```
⇒ ((0 0 0) (0 0 0) (2 3 4))

Here, as before *zs* is merely `egal?` to *xs*, so when we mutate the binding of the latter, the former still says bound to the original list. However, we have used the new keyword `alias` to make *ws* be so intimately related to *xs* that changes to the binding of either of them also affect the other. Aside from the `set!` operation, none of the other language features we've seen (so far) differentiate between `alias` and `let`.

We can also introduce an operation that works like λ, except that it accepts only variables as operands, and it associates the parameters in its definition with those operands using `alias` rather than `let`:

```
(51)  (let ([f (aliasλ (ws) (let ([_ (set! ws (list 0 0 0))])
                              ws))]
            [xs (list 2 3 4)])
           (list xs (f xs) xs))
```
⇒ ((2 3 4) (0 0 0) (0 0 0))

Because of how the parameter *ws* internal to the definition of *f* gets associated with the variable *xs* when we evaluate (`f xs`), mutating *ws*'s binding also ends up mutating the variable *xs*.

With these resources in hand, we can define a third kind of equality predicate:

```
(52)  (let ([aliased? (aliasλ (u v)
                        (let ([u0 u]
                              [_ (set! u (mlist 0))]
                              [result (egal? u v)]
                              [_ (set! u u0)])
                          result))]
            [x 3]
            [z x])
           (alias ([w x])
                  (list (egal? z x) (aliased? z x)
                        (aliased? x x) (aliased? w x))))
```
⇒ (true false true true)

This makes use of the expression (`mlist 0`), which creates a new mutable list. Such a value is guaranteed to not be `egal?` to any other value that variables are already bound to. What this definition of `aliased?` does is save the original value of its parameter *u* as u_0, then mutate the binding of parameter *u* and see whether the other parameter *v* ends up being `egal?` to the new value. It saves the result of that test as *result*, then restores *u* to its original value (in case the invoking context needed it), and returns *result*.

This `aliased?` predicate is like *f* in example (45), in that its result is not determined merely by the argument values passed to it (*z*, *x*, and *w* all have the same value, 3). But `aliased?` is more interesting, in that it *does* track whether its operands stand in the *de jure* codesignating relation that different occurrences of *x* (in the same binding context) stand in to each other, and that *w* and *x* also stand in. Thus the last two tests in the final list expression evaluate to true; whereas *z* on the other hand is merely `egal?` to *x*, not also `aliased?`

Defining `aliased?` in the way just demonstrated makes essential use of `(mlist 0)` and `set!`, which involve the two kinds of mutation we explained above. This is a good way to get an intuitive handle on how such a predicate could even be possible. But it may give the impression that in less "dynamic" languages, that don't have any of these mutation novelties, a notion like `aliased?` would be inexpressible. To counter that impression, I'll report that it's possible to define `aliased?` in many implementations of Scheme in a way that doesn't make use of mutable values or bindings. The mechanisms required to do this are complex; see the URL in note 59 for details. Regardless of how feasible it is to implement `aliased?` in these specific programming languages, it could always be given a semantics as a native programming idiom even without the language also having mutable values and bindings. (But the strategies for giving a formal semantics for these several notions will be similar.)

It should be possible to use `aliased?`, `alias`, and `aliasλ` all together. We demonstrated how the first and second should work together in (52). Here is how they should interact with the third:

```
(53)  (let ([x 3]
            [g (aliasλ (u v)
                       (list (aliased? u u) (aliased? u v)
                             (aliased? u x)))]
            [z x])
           (alias ([w x])
                  (list (g x x) (g w x) (g z x))))
                       ⇒    ((true true true) (true true true) (true false false))
```

Because *w* and *x* are aliased, and passing them to *g* aliases them to *g*'s internal parameters, all of the tests in *g* come out true when any combination of *w* and *x* are supplied as operands. But *z* merely happens to currently be bound to the same value as the other variables. It isn't `aliased?` to them. (Of course, *z* is `aliased?` to itself.)

The predicate `aliased?` is not (or not merely) a quotative operator, as it doesn't just test for whether its operands are syntactically matching. Rather, it's sensitive to the patterns of *de jure* codesignation in those operands. It provides an example of the kind of predicate that we observed Richard offering (and Fine at least gesturing at). (But it doesn't have the interesting behavior I labeled F4 in §4 above.) These authors think a linguistic context like `Anita believes that _____ admires _____` may evaluate to true when `Cicero` and `Cicero` are supplied as operands, but false when `Cicero` and `Tully` are supplied – even if those names themselves have the same semantic value. In the same way, (`aliased? x x`) may be true but (`aliased? x z`) false, despite *x* and *z* being bound to the same value.

Here is another connection to our earlier discussion. In the following:

```
(54)  (let ([value (λ (y) y)]
            [x 0])
           (list (aliased? x x) (aliased? x (value x))))
                                                    ⇒    (true false)
```

we define an operation `value` that extracts the value that *x* is currently bound to, returning it in a form that doesn't count as `aliased?` to *x* (or any other variable). We might then think of contexts like `Juan told Maria that _____ wasn't _____` as being false when the *de jure* codesignative expression-occurrences `he` and `him` are supplied as operands, but as being true when `he` and (`value him`) are supplied (see note 57). Perhaps the key to understanding Soames's (27a) is to interpolate such an (unpronounced) operation. (This is similar to Fine's strategy of positing a special *de re* reading of the report, and vulnerable to some of the same objections.)

If one is new to the programming idioms explained here, what we've walked through may seem exotic and to shed little light on the intelligibility or prospects of predicates with `aliased?`-like behavior in natural language. However, as I said at the start of this section, there is little in the "lightweight" additions that one should find semantically novel. And of the "middleweight" additions, we made use of mutable values and variables only as a stepping-stone to get to an understanding of how `aliased?` and its partners work. In principle, as I said, we don't need to define them with mutation. They can exist in languages that are mutation-free.

As I acknowledged earlier, programming languages can and often do make use of genuinely exotic ideas that require more sophisticated semantic techniques than readers of this chapter may be familiar with. But none of those ideas – unlimited recursion, continuations, fancy types – are needed to make sense of `aliased?` and its friends.

I hope this section contributes towards "domesticating" the kind of predicates that fans of *de jure* codesignation are friendly to, that is, making them seem less alien and somewhat less "magical." As we've seen, such predicates are needed anyway for some formal languages, ones that ought not seem miles away from natural language. In my view, the work summarized in this chapter's earlier sections creates a case for exploring whether this kind of semantic structure is present in natural language too: perhaps in ways that encode a semantic difference between demonstrative and the "strict" anaphoric uses of pronouns from §1, or perhaps in ways that affect the truth-conditions of attitude reports, or perhaps in ways that encode the different patterns of anaphora posited by Heim and Higginbotham in §6 for unembedded sentences.[62]

Notes

1 Compare Higginbotham (1985, examples 62–63); Heim (1998, pp. 213–216, 241); and McKay (1991, p. 718). On the contrast between merely intended coreference and the referential dependence we see in examples that follow, see Evans (1980, §5), Recanati (2012, §8.1), and Fiengo and May (1994, esp. pp. 3–13, 49–51). Goodsell (2014) articulates a notion of "assumed coreference," also distinguished from the relations exemplified below.

2 When talking about this example, I'll rely on the reader to understand that the sentence is being used and understood in the way described; and similarly for later examples. Of course, the *displayed sentence* might also be used in the earlier way. It will simplify exposition for me to presume (*contra* Lasnik, Bach, and some other authors discussed below) that the demonstrative and anaphoric uses of pronouns are syntactically different.

3 For a survey of some accounts, see Dahl (1973). The "bound-variable" analysis described below is proposed in Keenan (1971), Williams (1977), Reinhart (1983, ch. 7), and elsewhere. For convenience, I will apply the labels "sloppy" and "strict" also to the hypothesized disambiguations of sentences like (3e).

4 This is sometimes denied, for instance by Recanati (2005, p. 308) and Salmon (1986b, pp. 50–52), though Salmon (1992, pp. 59, 65) allows that (3b) may be ambiguous. The "strict" reading is attested by Dahl (1973, p. 84) and Evans (1977, p. 95), who also cites Dummett and Partee. See Partee (1989, n. 4). The "strict" reading will dominate if we replace `Atticus` with a female name like `Terentia`. In that case, it's controversial whether the "sloppy" reflexive reading would be available at all.

   Sometimes it's suggested that `Cicero mourned his own daughter` gives us a way to force the "sloppy" reading. But McKay (1991, n. 21) convincingly shows that "strict" readings are still possible with such sentences. See also Reinhart (1983, ch. 7, n. 13).

5 Bach is in one way less radical than Lasnik, as he allows that expressions like `himself` and `his own` may have the distinctive syntax and/or semantics that other theorists attribute to our (3) examples more broadly. In another way, Bach is more radical, as he argues that pronouns apparently bound by quantifier phrases should be understood on the model of (2ab), too. See also Recanati (2005).

6 Compare Tomioka (1999, examples 7 and 8, and n. 4; also examples 23, 24, 27). For further discussion, see Soames (1989–1990, pp. 211–212), McKay (1991, pp. 721–723 and n. 30), Salmon (1992, pp. 65–66; 2006a), and Soames (1994, pp. 120ff.).

7 See Salmon (1986b; 1992; 1989, pp. 216ff.; 2006b; 2010, pp. 447–449); Soames (1985, n. 12; 1987a, n. 24 and pp. 58–61; 1987b, esp. §8; 1989–1990, pp. 204ff.). Salmon counts the propositional

forms in (Z2) as "logically equivalent" (2010, p. 451; also 1986b, pp. 51–55 and n. 23), but not synonymous, and he denies that ($\lambda x.$ *x* admired *x*) *a* will always be *inferable* in the sense we're considering from the others.

At (2010, pp. 453–454, 456–457), Salmon discusses whether we should count λ-conversions as synonymy-preserving when the bound formula is monadic.

Other endorsements of parts of the stance I attribute to Salmon and Soames include Kaplan (1986, pp. 269–271), Higginbotham (1991, pp. 360–361), and McKay (1991). See also note 3 above; and Bach (1987, pp. 256–257), though the rest of Bach's chapters 11–12 argues against extending the view beyond pronouns like `himself`. Salmon also cites Wiggins (1976b, pp. 164–166; 1976a, pp. 230–231).

8 This is meant to suggest the image of their structure somehow looping back on itself. Compare the idiom `(cons #1=expr #1#)` in some implementations of the programming language Scheme, which can be used to specify cyclic data structures.

Some authors already call these "reflexive" contents – but we've used that term above in other senses: for the reflexive morphology of `himself`, and for the "sloppy" reflexive readings of some of our examples. Salmon and Soames would also use the term for bound predicates with the structure of (3b′), and for our present notion. But since it's controversial how much these different phenomena coincide, it's conceptually more hygienic to introduce a new label here. (There is also the mathematician's notion of a "reflexive relation," which is not directly relevant to what we're discussing.)

Fine calls cyclic contents "coordinated," but I suspect this term is too closely associated with his own views. (And Fine also uses the term to refer to a kind of linguistic structure, as well as a kind of content.)

9 Fine says only a few words about anaphoric pronouns (see note 33 below), but the bound-variable analysis does not seem to constitute his understanding of them. It's clear that it doesn't constitute his understanding of (5) (see Fine, 2007, pp. 69–70). It's not clear whether Fine regards (3b) and (5) as semantically equivalent. Soames (2010; 2012) interprets him as giving them different contents, albeit contents the understanding of which requires recognizing that it's true iff the other is. As explained above, Soames's own view is that (3b) does have the form (3b′), and (5) does not, and neither is (3b′) inferable from the content of (5). But on his view, standardly someone who utters (5) will *also thereby assert* the content (3b′) (see Soames, 1987b, §8; 2002, esp. chs 3 and 8; 2005). See also Richard (1990, pp. 216–217).

Soames proposes treating the "strict" readings of anaphoric (3) sentences semantically on a par with the (2) examples (1989–1990, pp. 210–211). See also Heim and Kratzer (1998, pp. 240–241).

10 See Pinillos (2011, example 18). I'm ignoring accounts of (11a) in terms of focal alternatives. On some variations of the account sketched here, the first conjunct of (11bc) would be presupposed rather than asserted. Similar examples could be given using `Even Cicero ...` or `It was Cicero who ...`

11 See Heim and Kratzer (1998, §9.3) for an introductory overview.

12 Compare Evans (1980, examples 49, 53, 55) and Heim (1998, examples 13, 20).

13 Church (1954) accepts this consequence, and argues that the doubt really available to these subjects is not about lawyers but rather about the *predicates* `lawyer` and `attorney`. But Church did not think attitudes were *in general* about linguistic expressions.

14 Kaplan's views were presented in unpublished lectures delivered in 1985 (reported in Soames, 1987b, p. 112 and n. 19; and Salmon, 1986a, p. 164), and also in a few lines in Kaplan (1990, n. 6). There are also passages where Kaplan embraces the competing Salmon and Soames view: see note 7 above.

Geach's views are complex. One challenge is that he denies that sentences always have a single "subject-predicate analysis" (even in the absence of syntactic ambiguity; see Geach, 1962/1980, §§24, 27; 1975b, pp. 144–146). At the same time, he claims that some pairs of sentences are "logically equivalent," "say the same thing," or "have the same import," and yet "contain different predicables" or "predicate different things" of their respective subjects (1962/1980, §§78, 80). It's

hard to know how we are to identify *the* predicate of a sentence in some of these cases. In some places, though, Geach explicitly affirms that sentences like (5) predicate the same thing of Cicero that `Atticus admired Atticus` (and `Atticus admired himself`) predicate of Atticus (1962/1980, §26; 1965, p. 112; 1975b, pp. 139–140, 141, 147). Given Geach's analysis of reflexive pronouns (1962/1980, §§80–84), this means he'd see them all as "containing" the predicate $\lambda x.\ x$ admired x. In other cases, though, he denies that recurring names induce this predicate structure. For example, `Only Satan pities Satan` differs in meaning from (the "sloppy" reading of) `Only Satan pities himself` (1962/1980, §80). Geach does acknowledge a "strict" reading of the similar sentence `Not only Socrates loves his wife`, but although he allows that it's "logically equivalent" to `Not only Socrates loves Socrates's wife`, this is one of the cases where he says the paired sentences have different analyses (1975a, pp. 197–198). He interprets the former as "containing" the predicate $\lambda y.$ not only y ($\lambda x.\ x$ loves y's wife), as also proposed by Evans (1977, pp. 95–97); whereas the latter has $\lambda y.$ not only y ($\lambda x.\ x$ loves Socrates's wife).

15 Richard's diagnosis was that (16a), despite its counter-intuitiveness in his scenario, is as true as (16b). But in later work he retracted this.

16 Pryor (2016) proposes that *graph-theoretic* constructions are a good way to abstract the shared core of these proposals. See also Strawson (1974/2004, pp. 45–46).

17 Compare Carnap's account of reports summarized at the start of §2 above. Salmon in fact worked with *guises* rather than Mentalese sentences; and he called the doxizing relation "BEL."

18 For example, Jacob (1991, pp. 93ff.).

19 See Fodor (1990a, esp. pp. 71–73), and Fitch (1984; 1986; 1987, ch. 4). Fitch (1996) defends a more specific view, of the sort associated below with Schiffer.

20 The following complaints are taken from Schiffer (1987, pp. 459–461; 1992; 1994, pp. 285–287; 1995, §5; 2000; 2003, pp. 39–46; 2008). Schiffer's own preferred theory is given in (1994; 2003, esp. ch. 2).

21 See Crimmins and Perry (1989), Crimmins (1992b; 1995b). Crimmins (1995c, pp. 200–201) objects to calling his view a "hidden indexical" theory.

22 Or what plays its role on their account: Crimmins thinks of T as a "thought map" describing a hypothetical attitude in terms of particular mental representations ("notions") the subject is assumed to have (1992b, chs 4–5).

23 In the hands of Stanley and Recanati, that phrase later acquired a more specific meaning than Perry understood by it (see Perry, 2001/2012, pp. 55–57; Korta and Perry, 2011, ch. 9; Crimmins, 1992b, pp. 16–17, 152).

24 The term "quasi-singular" comes from (Schiffer, 1978, p. 182). Recanati holds that in some cases complements don't express those but rather express content schemas (1993, ch. 18, n. 15 and §19.4), and in other cases express general propositions (§19.3).

Recanati's vocabulary and theoretical framework make his views somewhat hard to track. In (1993, ch. 3; 1995, §3) he distinguishes an utterance's "semantic" from its "truth-conditional" content. He understands the latter to be a Russellian proposition, and identifies it with "the proposition expressed" or "what is said" by a literal utterance (1993, pp. 54–55, 65). That is the notion that best fits our inquiry. (His "semantic" content belongs to more ambitious stories about how to explain successful understanding; see the discussion of Loar's stockbroker at Recanati, 1993, pp. 53ff.; 1995, pp. 184–185.) Recanati's view of unembedded sentences with "referential" terms is that their utterances have singular truth-conditional contents, as Russellians claim (though their semantic content will be richer). What I report in the text is that Recanati thinks *these terms* also have Russellian truth-conditional contents when embedded, but *the complement clauses that contain them*, as wholes, usually have richer, quasi-singular truth-conditional content (see 1993, pp. 395–397; 1995, pp. 188–190). In this regard, Recanati is closer to Schiffer's preferred (2003) view than to the HIT that Schiffer discusses more critically. Schiffer compares his version of HIT to Recanati's in Schiffer (2000).

25 Richard's theory is developed in (1990, chs 3–4), the central parts of which were also published as his (1989). The theory was then refined in his (1993; 1995; 1997, §§10–11; 2006; 2013b).

26 This quote is from Richard (2013b, p. 12). At this level of abstraction, Richard's view also resembles the neo-Fregean account of attitude reports offered in Forbes (1990). But I won't explore those connections.

27 So Richard's view has similarities and connections to the "ILF" theories advanced in Higginbotham (1986; 1991), Segal (1989), Larson and Ludlow (1993), Larson and Segal (1995, ch. 11), and Ludlow (2000).

28 Richard uses "accepts" in a technical sense: see his (1990, §1.5, and pp. 181–183, 211–213). He generalizes to attitudes other than belief in (ch. 3, n. 20).

29 Note that Richard never prohibited correlations from being many-one; he always allowed diverse expressions in a complement to correctly report beliefs that a subject in fact has by doxizing recurring Mentalese expressions. That is, if Anita accepts only (her Mentalese version of) (5), nonetheless (in some contexts) I can correctly say:

(1*) `Anita believes that Cicero admired Tully.`

That is, (in some contexts) (1*) can *allow* the reported thought to be cyclic. Richard doesn't think such a report would *attribute* a cyclic thought (1990, p. 217), but he left open whether it's *ever* possible for diverse terms in a complement to do so (pp. 214–217). In other words, he only endorsed the left-to-right direction of: recurring expressions in the complement → the report attributes a cyclic thought.

The cases that later caused Richard to partly withdraw that endorsement were given by Soames and Crimmins. We'll discuss them in §6 below.

30 Fine resists Putnam's specific proposal that sentences like (1) and (5) differ in "logical structure" (p. 41). But they are pursuing similar ideas.

31 My presentation isn't maximally explicit either: for conciseness, I help myself to some natural shorthands that I'll trust the reader to understand.

For further discussion of the "antinomy of the variable" and Fine's solution, see Kellenberg (2010) and Pickel and Rabern (2016). Compare the latter's solution to https://en.wikipedia.org/wiki/De_Bruijn_index (accessed 30 September, 2016), which is a more efficient technique commonly used in writing compilers.

32 One may have the concern that the natural extension of the given semantics to $\supset$ will conflate $[\![\forall x(Fx \supset \bot)]\!]$ with $[\![(\forall xFx) \supset \bot]\!]$. But this does not happen. Although both of those will depend on $[\![x, F, x, \bot]\!]$, they'll depend on it in different ways, and end up being non-equivalent as they should.

33 Soames (2010, p. 465) suggests that Fine should handle examples like `I asked Martha to call me` along the same lines as (5). At (pp. 41, 122–123), Fine considers handling anaphoric examples like (3d) in this way too. See Soames (2012, pp. 259ff.) for complaints about how this interacts with Fine's desire to have multiple readings of attitude reports.

34 King (1996; 1995) is paradigmatic of these. This tradition goes back to Lewis (1970) and Cresswell and von Stechow (1982), and has roots in Carnap's notion of "intensional isomorphism," discussed in §2 above. See also the later developments of King's view, and comparisons to other accounts of structured propositions, in King (2007; 2011).

As Fine remarks on his p. 54, he only needs to rely on enough structure to talk about the different appearances of some object in a proposition.

35 Fine also briefly addresses the possibility of handling empty names and "confused" names (see ch.2, nn. 4 and 14, pp. 126–127; and his 2010c).

36 See (pp. 54–57; also pp. 77–78). For other articulations of this idea, see Soames (1987b, p. 112, attributing it to unpublished work by Kaplan), Salmon (1986a, ch. 4, n. 4; 1992, pp. 59–60), and Pinillos (2011, pp. 319ff.). Higginbotham (1985, pp. 564ff.) uses a similar device, but his explanatory purposes are not the same.

An alternative to wires would be to tag all the leaves of a structured proposition with indices, understanding leaves to be linked when they have the same value and index.

Fine in fact defines his "coordination schemes" so as to allow wires spanning *multiple* structured propositions in a sequence.

37 On (p. 59), Fine might instead be read as saying that (1) expresses a coarser, Russellian proposition; but I will ignore that possibility because it ill fits the overall shape of his account. Further, this passage appears only a page after Fine introduces the idea of different "levels" of semantic value. (Recall we saw a similar idea in Richard, 1986–1987.) Our discussion here is focused on only the most specific level.

38 See King (2007, pp. 41–42, 218–222); he is following a suggestion made by Quine (1940/1981), and then repeated in Kaplan (1986, p. 244), Salmon (1986a, p. 156), and Soames (1989–1990, p. 204). See also Crimmins (1992b, ch. 4) and Evans (1977, pp. 88–96). (On p. 102, Evans permits the wires to cross sentence boundaries, and from pp. 104ff. they're also used to join "donkey pronouns" to their antecedents. In these cases the apparatus has to be interpreted differently.)

This appeal to wires shouldn't be *conflated with* the way they're appealed to in the work cited in note 36 above. It is a substantive question whether the present use can be a special case of the wires Fine is working with. Nor should either of these uses be conflated with the use of arrows in linguistic work on anaphora (for example, in Higginbotham, 1983; 1985; Moltmann, 2006, pp. 236ff.; Heim, 1998; McKay, 1991, pp. 724ff.; and the usage in Evans, 1977, from 102ff. mentioned above; see also Fiengo and May 1994's notion of "coindexing without c-command"). But as we'll see in §6 below, there are plausible connections between these.

39 That does seem to be Fine's thinking (see his p. 30 and n. 11, and pp. 115–117). See also Soames (2012, pp. 238–241). Recall also Richard's notion of "co-relativized" terms, mentioned in §3 above.

40 For the failure of necessity, see note 33 above. See also Fine's discussion of `Carl/Peter Hempel` at pp. 46–47. `Paderewski` cases might be counter-examples to sufficiency, though this turns on controversial issues. Compare Fine's example where one has stipulated the same definition for `glub` and `flox` but isn't explicitly aware of having done so (pp. 129–130). For a range of similar cases, see the works cited at Soames (2012, n. 5).

41 This quote is from (p. 40); see also (pp. 60ff.). As we'll see in the next section, Pinillos uses similar language when defining his notion of "*de jure* coreference." So too does Recanati: see his (2012, pp. 92, 106, 110). Schroeter uses similar language when defining the notion of expressions "*striking one* as being *de jure* coreferential": in her (2003, pp. 18ff.; 2007, pp. 600, 611; and 2008, pp. 110ff.), she talks of its being "obvious and rationally incontrovertible" to the subject that the expressions codesignate. In (2012) she adds the condition that this appearance be "epistemically basic." Unlike some of the others, Schroeter thinks the appearance of *de jure* coreference can be mistaken: see her (2007; 2008).

42 I say "in some way" because Fine rejects the common view that `believe` expresses a dyadic relation between a subject and the proposition expressed by its complement (see his discussion of (SB) at 2010b, pp. 476–478; also the move labeled F4 below).

43 See (pp. 93, 120–121, and his 2010b). Notably, what Fine says about the truth-conditions for the "strong *de dicto*" reading involves reference to the report itself, but he doesn't want to say that the *semantic content* of these reports is self-reflexive. The issue raised next in the text (and afterwards labeled F4) also poses difficulties.

Fine also posits a "pure *de re*" reading of attitude reports that ignores any coordination in its complement. See Soames (2012, appendix) for complaints about these ambiguities, and how they interact with the motivation and explanation Fine offers in the earlier parts of his book.

44 Though with multiple subjects, we confront special issues because Fine denies that interpersonal coordination will be transitive: see Fine's (pp. 98, 105ff.), also Taschek (1998, §5) and Richard (1990, pp. 210ff.; 1993, 129ff.). Crimmins (1992a, pp. 193–194; 1995a, pp. 387–390) criticizes this part of Richard's account. Taylor (2000, pp. 175ff.; 2003b, pp. 12ff.) also discusses such cases, but instead of abandoning transitivity, he denies that subjects are authoritative about whether they've introduced a new name.

45 As we saw in §3 above, Richard committed to the left-to-right direction of this. See also Pinillos's Principle of Attitude Closure, discussed below, also his (2011, p. 316); Taschek's "Logic Requirement" in his (1995, pp. 77, 81, and 86; 1998); and Soames's "Belief Coordination Principle" in his (2012). See also Taylor (2003b, §8) and Forbes (1987, pp. 24–25).

46 Compare the discussion of "transparency" in Recanati (2012, chs 10–11; and 2015, §I.iii, which replies to Ball, 2015); Schroeter (2007; 2008); and the discussion of "slow-switching" in Boghossian (1989; 1992; 1994; 2011; 2015) and many other sources surveyed in Parent (2013, §3.3). See also Salmon's discussion of recognizing the reappearance of an object in several propositions, in his (2012, pp. 423–424, 425–426, and 427ff.; 2015, pp. 455–456). Salmon argues that failures of such recognition aren't manifestations of linguistic incompetence or semantic ignorance.

We'll return to this issue with Pinillos, in the next section.

47 For discussion of Fine's work, and his replies, see Soames (2010), Lawlor (2010), Hovda (2010), and Fine (2010b; 2010c; 2010a). See also Soames (2012) and (1987b, whose relevance was mentioned at the end of §3 above); and Salmon (2012), Fine (2013), Salmon (2015). Further commentary includes Ostertag (2009), Rattan (2010), Sosa (2010), Bonardi (2013), Weiss (2014), and Pickel and Rabern (forthcoming).

48 See Salmon (1986a, ch. 4, n. 4), Ostertag (2009, p. 348), Sosa (2010, pp. 353, 356), Soames (1987b, pp. 112–113, 123; 2010, p. 467; 2012, pp. 237, 244–245), Salmon and Soames (1988, p. 13, n. 10), and Pinillos (2015, p. 326). Putnam (1954, p. 156) seems to embrace the semantic equivalence of (23a) and (23b).

Taschek acknowledges this problem for his own account, which anticipates Fine's in several ways: see his (1995, pp. 78–80; 1998, p. 327). He proposes a solution much like the "ambitious" one I go on to describe Fine giving.

49 See also Taschek (1995, pp. 81ff.; 1998, esp. §II).

50 Pinillos (2015, pp. 330–334) argues that if we attend to embeddings of (1)/(22) and (23a/b) in attitude reports, they will be "always accompanied, often implicitly," by other reports that are also asserted or (more likely) *presupposed* in the discourse. This is similar to the proposal I mentioned Richard offering. The view of reports that Pinillos ends up with is similar to the Crimmins and Perry view described in §3 above, albeit with presuppositions taking over some of the work of their "unarticulated constituents."

51 This term is from his (1992). In (1995), he defines a somewhat different notion of "Content," in terms of which (1) and (5) do count as different because they aren't inter-substitutable in attitude reports. This notion is more closely connected to Taschek's notion of "logical potential" than to his notion of "information content," which he stops using.

52 Pryor (2016) discusses connections between this work and the "multi-centered" views of content in Ninan (2012; 2013). See also Recanati (2012, §18.3; 2015, §I.iv).

53 See his (2011, p. 315; 2015, p. 325). Sometimes Pinillos gives examples using "slash names" like `Hesperus/Phosphorus`. Other times he uses anaphoric expressions like `there` or `that planet`. Pinillos's interpretation of all these cases is challenged by Goodsell (2014) and Contim (2016).

54 Compare Fine's quote about "sensibly raising the question" whether the things they designate are the same. Contim (2016) argues that this criterion of Pinillos's may be too liberal.

55 See Soames (1989–1990, example 15a; 1994, examples 8ab; 2010, example 26; 2012, example 34a). See also Higginbotham (1991, examples 38 and 45), Sider (1995, pp. 503–505), Crimmins (1992a, pp. 192, 195–196; 1995a, pp. 383–387, 391–392), and Cumming (2008, example KR).

56 See Soames (1994, example 34d; 2012, example 34b). See also Higginbotham (1991, examples 42 and 46).

57 A special twist on Soames's examples is when they're embedded inside other reports, for example `The children thought that Juan told Maria that he wasn't him`. See Soames (1989–1990, example 18; 1994, example 9). For the purpose of reporting the children's thinking about Juan, we'd want `he` and `him` to be *de jure* coreferential; but for the purpose of reporting the content of what Maria was allegedly told, we wouldn't.

Using the machinery from §7 below, the children's thought might be represented as: `((λ (he) (alias ([him he]) (disclaim-identity he Maria he (value him)))) Juan)`, where `disclaim-identity` represents its second operand as having been told something from which she could infer `Someone is not himself` iff the third and fourth operands are `aliased?`, as here they are not. Nonetheless, from the perspective of the embedding thought, `he` and `him` are `aliased?`.

58 In an earlier paper Higginbotham (1983, pp. 404–406) endorsed the idea that "referential linking" is transitive. But Higginbotham (1985, pp. 570–574) argues against this.

59 This is one of three main families of contemporary language that descend from Lisp. Scheme is a *family* of languages because it has many different "implementations" that extend or further specify the language in somewhat different ways. Details on how to run the example code supplied here inside some implementations of Scheme can be found at http://www.jimpryor.net/research/code/dejure.html (accessed September 30, 2016). The linebreaks, indentation, and variation between `()`s and `[]`s are all just stylistic choices.

60 I've assumed that in the evaluation of `(list (f 3) (f 3) (f 3))`, we evaluate the operands from left to right. If we proceeded in a different order, we'd get a different result. Different implementations of Scheme handle this differently.

61 What we will make use of are ideas that in other programming languages go by the keywords "same lvalue," and "call-" or "pass-by-reference." Everything exhibited below is expressible in (many implementations of) Scheme, using that language's ability to define *macros* that operate on program syntax before it gets evaluated. See the URL in note 59 above for details.

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