BOOK REVIEW

Roads to anti-descriptivism (about reference fixing): replies to Soames, Raatikainen, and Devitt

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Abstract I reply to comments and criticism of my book *Roads to Reference* by Scott Soames (on the referents of ordinary substance terms and the conventions governing reference fixing for demonstratives, proper names, and color adjectives), Panu Raatikainen (on the exact scope of my critique of descriptivism and on the relation between referential indeterminacy and "partial reference"), and Michael Devitt (on the role of referential intentions and anti-descriptivism in the metasemantics of demonstratives).

Keywords Names · Demonstratives · Natural kind terms · Reference fixing · Anti-descriptivism

1 Reply to Soames: ordinary substance terms, color adjectives, and the status of conventions about reference fixing

Scott Soames's perceptive commentary deals with many issues arising from *Roads to Reference*, which makes it impossible to reply adequately to his points in the allotted space, but I'll comment on as many of those points as I can. Most of Soames's remarks concern the book's proposal that ordinary substance terms, such as "water", refer to what I call ordinary substances, instead of to scientific kinds such as H_2O. The idea of an ordinary substance, in a bit more detail and applied to the case of water, is that the criterion for some thing being an instance of water is the vague condition of being a thing sufficiently similar to the appropriate paradigms of water as regards the general compositional properties of these paradigms (see Gómez-Torrente 2019, 179); whether something is an instance of

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water is thus a vague matter in ways in which being an instance of H_2O is not, though many things will determinately be instances of water and many other things will determinately be instances of non-water. However, although I am thus committed to the idea that the kind water is different from the kind H_2O, Soames interprets me as embracing the idea that water is *vaguely identical* with H_2O, and that the ordinary term "water" refers (vaguely, I suppose) to H_2O. Before asking why Soames might want to interpret my claims in this way, let me emphasize that what I say is different from, and incompatible (against what Soames conjectures) with what he interprets me as saying.

To see that it's different doesn't require much explanation: for example, my claim of non-identity using the traditional notion of identity is just not the same thing (in the ordinary sense!) as Soames's claim of vague identity. To see that what I say is incompatible with what Soames interprets me as saying, note first that to say that water is vaguely identical with H_2O is to say that water is neither determinately identical (in the ordinary sense) with H_2O, nor determinately non-identical with H_2O; but I am committed to saying that water is determinately non-identical (in the ordinary sense) with H_2O, as they are different kinds with different determinacy profiles. And second, as for the claim about reference, to say that "water" refers vaguely to H_2O is presumably to say that "water" neither determinately refers (in the ordinary sense) to H_2O nor determinately fails to refer to H_2O; but I am committed to saying that "water" determinately fails to refer to H_2O, as "water" refers to water and water is different (in the ordinary sense) from H_2O.

Now, why does Soames want to save me from my claims? Is there some undeniable truth behind the claim that water is (in some sense) identical with H_2O? Or behind the claim that "water" refers (in some sense) to H_2O? Or behind the (stronger) claim that it is a necessary a posteriori truth that water is H_2O? Might we perhaps think that denying any of these things casts doubts on the basis of the Kripke–Putnam view of natural kind terms, and thus that, given that we have no theory with comparable virtues, we are left with no good theory at all? I don't think there is much reason to worry, and in fact this is perhaps the main message of chapter 5 of *Roads to Reference*. As argued at length there, it's no essential part of the Kripke–Putnam theory that water must be identical with H_2O, and (as made especially clear when one considers my refined version of the theory) the theory makes it basically inevitable for "water" to come to refer to the vague ordinary kind water. As for Soames's manifest worry that we might be left without necessary a posteriori truths in this crucial area, it should be appeased by my view's postulation of some necessary a posteriori truths in the vicinity of the (false) claim *Water is H_2O*, such as (presumably) *Instances of water are instances of H_2O* (see Gómez-Torrente 2019, 180–181). (In his commentary, Raatikainen agrees with me on this point.)

Soames's next group of remarks concern my view of reference fixing for color adjectives. On the view, particular uses of, say, the adjective "green", don't refer to a subjective property such as *looking green to Jones (in normal conditions)*, but to an objective property such as *irradiating light between rough levels h_1 and h_2 in the objective hue dimension*, where h_1 and h_2 are hue levels of green somehow taken as standards by Jones' (typically implicit) contextual intentions. Soames's worry here is that the view postulates "deeply private" contextual parameters, in the sense that h_1 and h_2, for example, need not be detectable at once by Jones' conversational

interlocutors, and may remain hidden from them for a long time if conversational circumstances are unfavorable and prevent Jones' contextual intentions from becoming transparent. My reply is that there is nothing to worry about, for contextual parameters or the referential intentions that fix them are by their very nature potentially opaque, and this is manifested very often when one uses context-sensitive expressions in general and color adjectives in particular. Misunderstandings occasioned by mistakes as to the intended referent of a demonstrative are commonplace, and disagreements caused by applications of different standards for "green" and "blue" are not infrequent in ordinary life (in agreement with my theory's predictions). That conversational circumstances may conspire to make contextual parameters *unusually* opaque is also clear [see my discussion in Sect. 4 of Gómez-Torrente (2016)], so I will not expand on this point here.

Ironically, Soames's diagnosis of the perceptual variation examples that provide one of the motivations for my theory appeals to a contextualist account of the vagueness of color predicates, which turns on the postulation of suitable contextual cut-off points fixed again by the speaker's intentions. He admits that these cut-off points may be unknown to other conversational participants in some cases, though they typically become (approximately) commonly known if the conversational circumstances are appropriate. What I say is in fact very similar, though I deny that precise cut-off points are fixed, and speak instead of rough standards. These rough standards need not be initially known by other conversational participants, and may occasionally be hidden for a long time, but, as in Soames, they will often become (approximate) common knowledge after a conversational while.

Soames criticizes in several ways my claim that semantic referents for demonstratives and proper names are determined by conventions that take the form of disjunctively sufficient conditions. One thing he suggests is that this is in tension with the idea that "what is asserted depends on facts about all parties to the communication". Perhaps the worry arises from the fact that my conditions for semantic reference are based on what we might call speaker reference, or speaker referential intentions, and surely facts of speaker reference are not "facts about all parties to the communication". There is no tension or incompatibility, however, as it is not the content of the relevant sufficient condition, but the fact that it codifies a convention, that guarantees that a fact partly involving "all parties to the communication" is involved. It must also be noted that my postulated conventions also often involve conditions about the intentions and beliefs of members of the linguistic community other than the speaker, as can be seen from two of my basic conventions for proper names, which I cite here for the reader's convenience:

> *Successful explicit name introduction via perceptual intention.* If a speaker S forms the explicit intention of using a name N that he or she introduces to refer to an object o that he or she is clearly perceiving, then N as used by S will refer to o, if S forms no intention conflicting with that intention, and if S doesn't form intentions about how to use N that on the whole conflict with the intentions of the community of users of N at large. (Gómez-Torrente 2019, 93)
> *Successful name transmission.* If a name N as used by a community of speakers refers to an object o and a speaker of the community, S, clearly

perceives some use of N by another speaker of the community and understands that N is used as a name, then N as used by S will refer to o, if S forms the (explicit or implicit) intention of using N as is used by the community of users of N and forms no intention conflicting with this intention, and if S doesn't form at the transmission stage a set of beliefs involving N that on the whole conflict with the beliefs of the community of users of N at large. (Gómez-Torrente 2019, 96)

When discussing my postulated convention for Arabic numerals, namely

Number rule. The Arabic numeral "1" refers to the number one; and whenever an Arabic numeral refers to a certain number, the Arabic numeral that follows it in the [roughly lexicographic] generating order refers to the number greater by one than that number. (Gómez-Torrente 2019, 128),

Soames more explicitly asserts that my postulated conventions for demonstratives and proper names, by contrast with the *Number rule* convention, cannot be real semantic conventions. The first reason he gives is that my alleged conventions for demonstratives and proper names are not language-specific, unlike the real convention(s) for Arabic numerals, which can't be found in ancient Latin, for example. But it's clear that one cannot argue that my postulated reference-fixing convention for Arabic numerals is not language-specific by noting that it didn't exist in ancient Latin—Arabic numerals simply didn't exist in ancient Latin! Surely all languages that have Arabic numerals incorporate the same reference-fixing convention(s) for them, as these are part of what characterizes the system of Arabic numerals. Similarly, all languages that have proper names (and I think that means all currently existing languages) incorporate roughly the same reference-fixing conventions for them, as these are part of what characterizes names. Yet historical linguists conjecture that proper names didn't exist in some very ancient languages that contained demonstratives or proto-demonstratives (and surely names don't exist in some imaginary languages), but certainly this doesn't make whatever conventions for proper names there are non-language-specific—they are specific, that is, to the languages in which proper names exist.

The second reason Soames gives is that my conventions for demonstratives and proper names do not reflect "contingent decisions that spread and become widely shared", again unlike my postulated convention for Arabic numerals. He doesn't explain why he thinks they differ in this way, and it's hard to see how they could. *Any* decision as to what is to be the semantic referent of an expression or expression use is perforce contingent. There is no more necessity in the stipulation that a use of a name N by a speaker S is to semantically refer to an object o when the conditions specified in *Successful explicit name introduction* via *perceptual intention* are met than there is in the stipulation that "1" is to refer to the number one, codified by the *Number rule*. But though not necessary, these conventions become widely shared in virtue of their usefulness in coordinating speakers' expectations concerning the beliefs and consequently the actions of other speakers.

Soames's third reason is that my postulated conventions for proper names don't really have much to do with understanding these expressions, and "the most

interesting … take the name already to have a referent in the language of the speaker's community". The claim about my postulated conventions not having much to do with understanding is not developed, but I suspect that Soames may want to reserve the term "understanding" in this context for knowledge of truth-conditional content of particular names or name uses, which certainly is not provided by knowledge of general conventions such as *Successful explicit name introduction* via *perceptual intention* and *Successful name transmission* above. Since I prefer to use the term "understanding" in a broader way which includes other aspects knowledge of which I take to be required for full competence with an expression, and since Soames after all concedes that the conditions I postulate as conventions "do have something to do with understanding names", I think that for present purposes I can rest content with repeating the observation, already made in *Roads to Reference* (see p. 71), that a fully competent speaker must know that names can get introduced and transmitted in something like the way described in *Successful explicit name introduction* via *perceptual intention* and *Successful name transmission*. (Note that Soames is right to think that the *part* of the content of a name that can be fully known is its referent, as follows from my considerations on the unknowability of the completeness of the list of reference-fixing conventions; see my reply to Raatikainen.) But, finally, do my postulated conventions really fix semantic referents for proper names, or do they rather "take the name already to have a referent in the language of the speaker's community" and are thus somehow circular, as Soames implies? Presumably Soames has in mind *Successful name transmission* above, where it is presupposed that a given arbitrary name N has a referent in the idiolects of the speakers of a given community up to a certain point, and conditions are given for when N preserves that referent in the idiolect of a new user of the name in the community. What we must observe is that even when a name has a semantic referent in the idiolects of a certain community, the question arises of when it keeps that semantic referent in the idiolect of a newcomer; this question is not in any way circular nor does it find a presupposed answer in facts about previous speakers. Answering that question is what Kripke's theory of the transmission of reference was all about, and it's also what my postulation of the convention of *Successful name transmission* is all about.

2 Reply to Raatikainen: the status of descriptivism and the idea of "partial reference"

Panu Raatikainen's most important worries concern the question of what status name descriptivism is left in after the critique of *Roads to Reference*. Raatikainen forcefully explains his own view that any descriptivist theory that accepts that the relevant description that a speaker associates with a name she understands is not *analytically* equivalent with the name gives up the essence of descriptivism: according to Raatikainen, a descriptivist theory of this sort, a *weak* descriptivism, as Raatikainen would call it, cannot solve the Frege puzzles, but offering descriptions-based solutions to these puzzles is the defining theoretical job of descriptivism. (Raatikainen also has psychological implausibility, circularity, and parasitism

arguments against weak descriptivism, but I cannot go into these here.) On Raatikainen's view, regardless of the merits of the anti-descriptivist arguments in *Roads to Reference*, these arguments are to a large extent superfluous, given that they are directed at weak descriptivism.

In *Roads to Reference* I take what Raatikainen would call proper or strong descriptivism to have been refuted by Kripke and others, and grant for the sake of argument that what he calls weak descriptivism is psychologically not impossible (even if it *is* implausible) and that, if true, it might be theoretically fruitful. For example, if the relevant description associated by a speaker with a name turned out to be knowably (either a priori or a posteriori) but not analytically equivalent with the name for the speaker, we would still have an explanation of Frege's puzzle about the informativeness of true name identities of the form "$a = b$": if DH is the description associated with "Hesperus" by a speaker and DH is different from the description DP associated by her with "Phosphorus", the informativeness of *Hesperus is Phosphorus* for her would receive a certain kind of explanation from the fact that when told that Hesperus is Phosphorus she is learning whatever content the identity between DH and DP has in her idiolect.

The critique of (weak) descriptivism in *Roads to Reference* concludes that a speaker can at most associate with a name she understands a list of (particular applications of) general conventions stating sufficient conditions for reference and reference failure. Now, while Raatikainen sees force in the book's arguments for this conclusion, he wonders why a descriptivist might not concede it and still claim that that's a sufficiently strong descriptivist claim for his purposes: according to *Roads to Reference*, doesn't a speaker associate with a name she understands a certain kind of "descriptive" content (a list of (particular applications of) general conventions stating sufficient conditions for reference and reference failure)?

I think, however, that the book's conclusion is anti-descriptivist in a strong, essential way. First of all, *Roads to Reference* crucially argues that the list of (particular applications of) conventions stating sufficient conditions for reference and reference failure that a speaker associates with a name is a list that the speaker doesn't know or believe to be complete; i.e., the speaker doesn't know or even believe that the list incorporates all the conventions relevant to the fixing of the reference of the name as it comes out of her mouth. Second, even if the list, unknown to the speaker, incorporates all the relevant conventions, typically it will not by itself fix the reference of the name as it comes out of the speaker's mouth; typically, it could do so only together with a host of mostly unknown (to the speaker) facts about the history of the name. (By contrast, the descriptions proposed by weak forms of descriptivism do by themselves either describe a certain object or fail to describe any.) A minimal, definitory descriptivist thesis must be that the information a speaker associates with a name both determines the referent of the name in the speaker's mouth when the name has a referent (or determines reference failure otherwise) and is information (implicitly) believed or even known by the speaker to fix the thing that she is referring to if there is any (the information cannot just "hang around" the name in the speaker's mind if it is to be related to the name in a reference-fixing way). If a theory accepts that the referent of a name use is not fixed by information in the speaker's mind, or that the speaker does not even believe

(in however implicit a way) that such information fixes what she is talking about, there is no substantive sense in which the theory can be called descriptivist. The essence of descriptivism has all to do with its internalist approach to the fixing of reference and content in general.[1]

Raatikainen regrets that in the book I don't relate my ideas about referential indeterminacy to Field's (1973) idea of partial reference (for historical uses of some scientific terms, prominently "mass"), applied by Devitt to the case of ordinary proper names and demonstratives. I regret this too—as I regret having had to omit discussion of many other important works of the vast literature on reference in the book. Given an opportunity to compare the ideas, I can say that, regardless of the merits of Field's analysis of the case of "mass", I'm unsympathetic to the application of his apparatus to the case of ordinary singular terms. The key idea is that, in a case where the reference of a singular term is indeterminate between two or more things, e.g., as with "Madagascar" in Marco Polo's mouth (see Gómez-Torrente 2019, 74), the term "refers partially" to each of the things. The main justification for introducing this concept is to use it in a definition of truth conditions for sentences containing such partially referring terms: this essentially says that one such sentence is true in the defined sense just in case it is true in the ordinary sense for all relevant ways of assigning ordinary reference to the partially referring term(s) in it. This may give somewhat appealing results in some cases, but to me it sounds like a technical trick that, first, misses important facts about how names and demonstratives work, and that, second, even appears incoherent.

As for the first point, I take it as evident that proper names are conventionally singular terms; that this is manifested in how a competent speaker takes care not to make a use of a proper name if she does not believe that a single thing has somehow become the referent of that use; and that it is also manifested in intuitions about the truth conditions of sentences containing names. Even Marco Polo's utterance of *Madagascar is a piece of land* is not intuitively true (if it is true, which I doubt) *because* of the fact that two different things are pieces of land; if it were true, it should be true because some one thing is a piece of land, and Marco Polo would wholeheartedly agree! As for the second point, on the Fieldian theory *"Madagascar" determinately refers to Madagascar* (as a sentence in Marco Polo's idiolect) comes out true (and thus also *"Madagascar" determinately refers to some thing* comes out true), but it should be false according to the theory itself, and according to the intuitions elicited in *Roads to Reference*. (This is reminiscent of how

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[1] Uses of demonstratives, on the other hand, can be said to satisfy an internalist conception of reference fixing more often than uses of names, according to the picture of the book. In many, perhaps most cases of successful uses of a demonstrative, the referent will be determined by the (particular applications of) conventions stating sufficient conditions for reference and reference failure that a speaker associates with the demonstrative, coupled with other information known by the speaker, e.g. about what she is perceiving. However, even for demonstratives the internalist descriptivist thesis will not be true in general. For example, when the convention of *Successful demonstrative reference* via *non-conflictive nominal referential intentions* (see Gómez-Torrente 2019, 52, n. 24) is in operation, the reference of the relevant use of a demonstrative is a function of the reference of a certain name (in the idiolect of the speaker), and, as we have just noted, the reference of a name will most often not be determined in a way that satisfies the internalist conception of reference fixing.

traditional supervaluationism about vagueness implies its own falsity, as it implies that it is (super-)true that there are precise cut-off points for vague predicates.)[2] (See my reply to Devitt for further criticism of the idea of "partial reference" as applied to demonstratives.)

Raatikainen also complains that I don't discuss Devitt's ideas about multiple grounding. This is not strictly true, however, as in the book (on p. 84) I discuss and offer a counterexample to Devitt's (2015) proposal that if a token of a name designates a certain object, then all the designating chains underlying the token must be grounded in the object. On the other hand, the ideas of Devitt and several others, that reference may be consolidated via multiple groundings and that the elimination of indeterminacies often results from new groundings, are incorporated in the book: see the discussion of my postulated conventions of *Successful name adoption* via *perceptual intention* and of *Successful name adoption* via *description* on pp. 98ff.

On the surface, Raatikainen's remarks on natural kind terms don't suggest the existence of substantive disagreements between us. However, I'm intrigued by his implicit idea that ordinary kind terms don't fit especially well the Kripke–Putnam picture—implicit, that is, in his suggestion that "proto-scientific" terms like "oxygen" "best fit" the picture. His emphasis on the idea that ordinary natural kind terms have many associations of a non-(proto-)scientific nature doesn't seem to me to imply by itself any difficulty for the claim (of my refinement of the picture) that their referents are certain vague kinds with a core identifiable by science. This will be true provided we are dealing with bona fide natural kind terms, i.e. terms associated with an overriding convention that they should refer, if at all, to appropriate natural kinds. (On the other hand, there may be terms which are originally intended as natural kind terms but which evolve so as to adopt a different, descriptive meaning; or terms of which it may be a vague matter whether they are natural kind terms or descriptive terms. But I would not count these as bona fide natural kind terms.) However, perhaps Raatikainen has some thoughts in mind that do call into question the idea that some or all apparently bona fide ordinary natural kind terms are really such, in which case I hope that those thoughts can be made explicit by him in a future occasion. Certainly, if the picture worked well only for things like "oxygen", we would have a big gap in our understanding of ordinary linguistic reference.

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[2] A third point might be added that, in any case, the apparatus doesn't appear to have any use in cases where there are not two or more potential referents, but the indeterminacy is between reference to a certain thing and reference failure, as in the case of "George Smith" and the students (from Kripke 1972, 95; see Gómez-Torrente 2019, 73). Would Field or Devitt say that "George Smith" partially refers to the neighbor and partially fails to refer? Then presumably no sentence containing "George Smith" comes out true in the students' idiolects anyway.

3 Reply to Devitt: the causal-perceptual theory of demonstratives and the role of intentions in reference fixing

Michael Devitt's incisive piece develops a critique of my use of referential intentions in the theory of reference fixing, and defends his alternative, intentions-free account of how demonstratives refer. I will successively explain why his account doesn't work, how the reasons why it doesn't work provide one motivation for theories like the one I propose, and why the criticisms he directs at these theories, including mine, are incorrect.

Devitt's account says that "the reference of a person's deictic referential demonstrative is fixed in the object in mind by a causal link between the person and the object when it is, or was, the focus of that person's perception … what I call a 'grounding'"; for example, "a person's use of 'he' refers to a *male* that is, or was, the focus of her perception". One reason why this doesn't work is that, even if the existence of a causal perceptual link between object in the focus of perception and speaker were a necessary condition for a demonstrative utterance of the speaker to refer to the object (it isn't; see below), it would not be a sufficient condition. Suppose that right now the object "in mind", fully in the focus of my perception is a certain token of the letter "a" in the computer screen in front of me, as I make a pause in my writing. And suppose that, without giving it *any* thought, an utterance of my mantra sentence *This is difficult stuff* mechanically comes out of my mouth. According to Devitt's account, my "this" *ipso facto* refers to the token of the letter "a" (note that at some point he says that "past groundings are trumped by the present one", so even if I somehow sometime *perceived* the "stuff" I am talking about, the reference-fixing grounding is my perception of the letter token). But this is not right. The reference of my "this" is intuitively the stuff I'm writing about. Part of what makes this clear is that, if a friend next to me asks me *What are you talking about?*, I'll say something like *The demonstratives stuff is difficult*, not something like *This letter token is difficult*.

I have set up this example so that it also illustrates why Devitt's condition is not necessary for demonstrative reference, either. Is "the demonstratives stuff" something I can really have "perceived" at some point, or with which I am in causal contact? I submit that the most natural view, and at any rate a view that should not be ruled out by a (correct) theory of the semantics of demonstratives, is that the topic of demonstratives is not an object I have perceived, but one whose existence I presumably infer or postulate in some implicit way from the existence of things I perceive, including utterances of demonstratives and papers and books about them. Much the same applies to the "that" in my utterance of *So that's* π, right after being introduced to the Leibniz formula for π [as noted in *Roads to Reference* (Gómez-Torrente 2019, 21)].[3]

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[3] Devitt places himself among a series of "perceptual" theorists, including Gareth Evans. But most of these theorists, including Evans, do not really propose theories like Devitt's. They do uniformly claim that a (quasi)-necessary condition on *singular thought* involving *perceivable* objects is that the relevant object is or has been perceived by the thinker. This is compatible with claiming, as in fact several of those theorists do claim, that a *singular term*, including an indexical or a demonstrative, may conventionally

The existence of a perceptual relation between an object and an utterer as the demonstrative utterance is produced is most certainly not sufficient for the utterance to refer conventionally to the object. What is intuitively sufficient is that the utterance be appropriately related to the utterer's intentions. For example, my intention in uttering *This is difficult stuff* in the circumstance I described is to refer to the demonstratives stuff, as revealed by my dispositions to linguistic behavior subsequent to my utterance, and this is the basic fact responsible for the reference of my demonstrative. (See below for more on the nature of referential intentions.) Furthermore, within a referential intentions framework it is also easy to accommodate utterances of demonstratives used to refer to things which are not perceived or even perceivable: my dispositions to linguistic behavior may reveal, for example, that my intention is naturally described as relying on my having in mind a description such as "the topic of demonstrative reference fixing" as fixing the reference of my "this" in *This is difficult stuff*. This is not to say that finding out the conditions under which referential intentions fix the reference of a demonstrative utterance is easy—a whole chapter of the book and a mind-boggling amount of books and papers are devoted to the topic. This is difficult stuff!

Let's turn now to Devitt's three criticisms of referential intentions theories. (I won't consider a fourth criticism of these theories that according to Devitt doesn't apply to mine.) The first criticism is that these theories are "implausible" because, according to them, a speaker "*can't refer without thinking about reference!*", in Devitt's words. His idea is that these theories postulate that the referential intentions that secure demonstrative reference are intellectual thoughts previous to or simultaneous with the relevant utterances, mentally occurrent sentences somehow of the form of *Let me refer with "that" to the man I see in front of me*. I agree with Devitt that to postulate the existence of such thoughts in all or even most cases of successful demonstrative reference is implausible. But I'm puzzled as to why he attributes this idea to me—and I doubt that it has been proposed by any referential intention theorist. In order for an intention, including a referential intention, to exist, it is often enough that an agent is in a mental state that disposes her to act in ways revelatory of what we would call that intention. Thus, in the example above, I can be said to have the intention to refer to the topic of demonstratives with my utterance of "this" in *This is difficult stuff* because I'm clearly in a mental state that disposes me to act in certain ways, including certain linguistic ways; but this is not to say that a mentally occurrent sentence somehow of the form *Let me refer with "this" to the Φ* is or ever was in my mind. That Devitt has an intellectualized idea of intentions does not require referential intentions theorists to have one also.

Devitt's second criticism, related to the first, is that intention theories are "redundant". According to him, since they postulate the existence of a thought of the form *Let me refer with "this" to the Φ* as necessary for the reference of "this", they postulate a thought *additional* to a thought which must then also exist and

which already is reference-fixing, of the form *This is the Φ*. However, since (as just noted) on my view neither thought needs to exist in order for a use of "this" to refer, it is necessary (if a use of "this" is to refer) that the utterer be in an appropriate reference-fixing mental state, her referential intention, which need not consist in the existence of an occurrent mental sentence. Again the problem is evaded by the natural postulation of an intention to refer to the object, which need not be represented descriptively in the speaker's mind. In such cases, the dispositions to behavior revelatory of the relevant intention may include simple dispositions to demonstrate the object when asked *What are you talking about?*

Devitt's third criticism, related to the second, is that intention theories are "misleading". Since he thinks that intention theories, including mine, invariably postulate the existence of a thought of the form *Let me refer with "this" to the Φ* as necessary for the reference of "this", he thinks that these theories are invariably descriptivist theories. This is "misleading", apparently, simply because it "distracts" from the right theory, which Devitt has reasons to think must be anti-descriptivist and based exclusively on causal groundings, perceptions, etc. Now, as I said, I doubt that there are any intention theories that postulate the existence of an occurrent thought of the form *Let me refer with "this" to the Φ* in the utterer as necessary for the reference of a use of "this". But it is true that the intention theories other than mine that I'm familiar with do postulate that some more or less complicated description, that the utterer somehow knows to be co-referential with an utterance she makes of "this", fixes the reference of the utterance. (Mine certainly doesn't!) Nevertheless, I doubt that an effective criticism of these theories can consist in simply pointing out that they distract us from an anti-descriptivist conception, much as we may like such a conception for other reasons. As noted in my reply to Raatikainen for the case of names, I don't think one can reject all kinds of descriptivism merely on the grounds of psychological implausibility, for example; and definitely Devitt's defense of his causal-perceptual theory just cannot lead to such a rejection, since the theory often gives wrong results, as noted above. In *Roads to Reference* I develop a different argument directed specifically at the descriptivist commitment of previous intention (and other) theories of demonstratives, based on the existence of cases of uses of demonstratives that exhibit apparent referential indeterminacy.

My final remarks concern precisely Devitt's way of dealing with apparent indeterminacy. My explanation for the referential indeterminacy of some uses of demonstratives appeals to the existence of multiple conflicting referential intentions on the part of the utterer, in such a way that none of the intentions conventionally overrides the others. Devitt's way with this is short: "Since referential intentions have no place in the theory of reference, conflicting intentions pose no indeterminacy problem". What I would say is that since referential intentions are the most likely candidate to help overcome the deficiencies of a merely causal-perceptual theory, conflicting intentions pose a problem that must be dealt with. And consideration of Devitt's specific treatment of particular examples of conflicting intentions and apparent indeterminacy further tips the balance in favor of my theory.

About the Carnap-Agnew case (see Kaplan 1978, 239, adapted e.g. in Gómez-Torrente 2019, 39), Devitt says that the use of "that" there "straightforwardly

semantically refers to Carnap's picture: it is the expression of a thought that was causally grounded in that picture via many earlier perceptions". But I would note, first, that theoreticians (including Kaplan in his (1978)), typically have the intuition that Kaplan's use of "that" conventionally refers to the picture of Agnew, and in some cases (including the later Kaplan) lean toward uncertainty or failure of reference views (see some references in Gómez-Torrente 2019, 40); to my knowledge, no one aside from Devitt says that reference is uniquely and "straightforwardly" to Carnap's picture, and this should give us, and Devitt, some food for thought; in fact, if Devitt thinks that his theory's prediction agrees with intuition, I would take this as additional evidence of the mixture of intuitions typical of indeterminacy cases. And second, that Kaplan's example might well have appeared underdescribed to Devitt: Kaplan says nothing about whether, as he makes his utterance, he has yet a third object "in the focus of his perception"; suppose Kaplan is in fact attentively looking at a picture of Tarski as he makes his utterance; since, as we saw, according to Devitt "past groundings are trumped by the present one", in this case his theory predicts that conventional reference is to the Tarski picture, but I trust that the reader will agree that this is plain wrong.

About cases like my tree example (see Gómez-Torrente 2019, 42), where there appears to be indeterminacy due to the existence of several conflicting perceptual groundings, in Devitt's description (or several conflicting perceptually based referential intentions, in my description), Devitt says that there is indeterminacy only in the sense that there is "partial reference" to all the grounding objects.[4] As noted in my reply to Raatikainen, I have doubts about the descriptive adequacy and the coherence of the idea of partial reference, and here I would add the following consideration specifically about its descriptive adequacy as applied to demonstratives. Presumably the possibility of partial reference is just as conventionally regulated as the possibility of simple reference. But if so, then presumably people would be comfortable with the consequences of the relevant conventions, that they should have some grasp of. Presumably people would sense no oddness in my saying *He is a friend I told you about* when I introduce my friends John and Jim to someone else as I surround both men with my arms at a party—how could it sound odd if I am touching the two men and both are in the focus of my perception, so that my "he" just "partially refers" to each of them and what I say is true of both? But I doubt people would sense no oddness in my utterance, precisely because (as noted for names in my reply to Raatikainen) demonstratives *are conventionally singular terms*, and speakers just don't have any notion that the truth conditions of a sentence involving "he" might involve two people as (partial) referents. There are other reasons why appeal to "partial reference" seems inadequate to me, but I must stop here.

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[4] About my Homer/Alexander example (Gómez-Torrente 2019, 31–32), Devitt also says that the demonstrative there "'partially refers' to both Homer and Alexander but does not determinately refer to either. The 'conflict' is in groundings not intentions". I find this perplexing, since the utterer (I) has not perceived either Homer or Alexander and need not even recall these names, having in mind only certain associated descriptions.

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