CrossMark

The argument from convention revisited

Francesco Pupa[1]

Abstract The argument from convention contends that the regular use of definite descriptions as referential devices strongly implies that a referential semantic convention underlies such usage. On the presumption that definite descriptions also participate in a quantificational semantic convention, the argument from convention has served as an argument for the thesis that the English definite article is ambiguous. Here, I revisit this relatively new argument. First, I address two recurring criticisms of the argument from convention: (1) its alleged tendency to overgenerate and (2) its apparent evidential inadequacy. These criticisms are found wanting. Second, following Zacharska (Univ Coll Lond Work Pap Linguist 22:56–63, 2010), I argue that while the argument from convention does alter the landscape of logical possibilities insofar as it provides good grounds for treating Donnellan's (Philos Rev 75:281–304, 1966) referential–attributive distinction as having truth-conditional consequences, the argument from convention nonetheless fails to demonstrate that 'the' requires two lexical entries.

Keywords Definite descriptions · The referential–attributive distinction · The semantics–pragmatics divide · The argument from convention

1 Introduction

The argument from convention contends that the regular use of definite descriptions as referential devices strongly implies that a referential semantic convention underlies such usage. On the presumption that definite descriptions also participate in a

✉ Francesco Pupa
   Francesco.pupa@ncc.edu

[1] Philosophy Department, Nassau Community College, 1 Education Drive, Garden City, NY 11530, USA

quantificational semantic convention, the argument from convention has served as an argument for the thesis that the English definite article is ambiguous. Here, I revisit this relatively new argument. First, I address two recurring criticisms of the argument from convention: (1) its alleged tendency to overgenerate and (2) its apparent evidential inadequacy. These criticisms are found wanting. Second, following Zacharska (2010), I argue that while the argument from convention does alter the landscape of logical possibilities insofar as it provides good grounds for treating Donnellan (1966) referential-attributive distinction as having truth-conditional consequences, the argument from convention nonetheless fails to demonstrate that 'the' requires two lexical entries.

The paper proceeds as follows. In Sect. 2, I provide a historical overview of Donnellan's referential–attributive distinction. Next, in Sect. 3, I present the argument from convention. With the argument from convention in place, I turn to its two major criticisms in Sects. 4 and 5. In the former section, I explore the charge that the argument from convention overgenerates semantic ambiguities. Here, I argue that all parties to the dispute are susceptible to charges of overgeneration. That is, I argue that overgeneration is inherent to both semantic and pragmatic accounts of the referential–attributive distinction. As such, I conclude that the charge of overgeneration does not provide good grounds for rejecting the argument from convention. In the latter section, I investigate the claim that the argument from convention is evidentially inadequate. Here, I present a range of evidence for the claim that speakers regularly use definite descriptions both referentially and attributively. With Zacharska, I argue in Sect. 6 that the chief problem with the argument from convention is its failure to establish an ambiguity in the definite article.

2 Some background

Russell (1905) argued that the English definite article is a univocal quantifier, that definite descriptions (e.g. 'the cat') are univocal quantifier phrases, and that sentences exemplifying the form 'the F is G' express general propositions—propositions containing no individuals.[1] For Russell, the use of a sentence exemplifying (1) expresses a general proposition exemplifying (2):

1. The F is G
2. There is at least one **F**, there is at most one **F**, and every **F** is **G**

In contrast, Strawson (1950) argued that 'the' is a univocal referential determiner, that definite descriptions are univocal referential terms, and that speakers use sentences exemplifying (1) to express singular propositions—propositions that contain individuals and thus propositions whose existence depends upon the existence of individuals.[2] For Strawson, when a speaker uses 'the F' to refer to a particular **F**, say α, in an utterance of (1), she expresses the singular proposition (3):

---

[1] With the obvious *proviso* that 'F' and 'G' do not themselves contain any referential terms.

[2] Strawson (1950) couched his theory in terms of statements, not propositions. Later, however, Strawson (1971/2004) would recast his view in proposition-talk. I follow that recasting here.

3. α is **G**

If α is not an **F** (if the speaker *misdescribes* α), the speaker fails to refer. In such a case, the speaker also fails to express a proposition.[3] In this sense, sentences exemplifying (1) *presuppose* that there is at least one **F**—as do the speakers who use them.

Sixteen years later, Donnellan (1966) cast a plague on both houses. Donnellan observed that definite descriptions admit of two uses: attributive and referential. To illustrate, consider a slightly revised version of Donnellan's well-known case. Sera and Jennifer discover poor beloved Smith, brutally murdered. Judging from the horrid condition of his body and the high opinion the community held of Smith, Jennifer says (4):

4. The murderer is insane.

Donnellan claims that here Jennifer uses 'the murderer' attributively. In saying (4), Jennifer wishes to assert that *whoever* committed the murder is insane. General considerations furnish the grounds of Jennifer's speech act, namely the condition of Smith's body and Smith's positive reputation. Jennifer uses the definite description in question without intending to refer to any particular individual. Indeed, she can felicitously use 'the murderer' in (4) without being causally or perceptually acquainted with whoever murdered Smith.[4] Moreover, Sera can grasp what Jennifer wishes to *get across*[5] without Sera herself being acquainted with whoever murdered Smith.

Now imagine instead that Jennifer says (4) on the basis of observing the peculiar behavior of Jones, the defendant in Smith's murder trial. Donnellan claims that here Jennifer uses 'the murderer' referentially. She wishes to say that Jones himself is insane. This time, singular considerations ground Jennifer's speech act. That is, Jones and his observed behavior cause Jennifer to assert (4). Donnellan went so far as to claim that with referential usage, a speaker could misdescribe an individual and nonetheless refer to that individual. In the current case, Jennifer could successfully refer to Jones using 'the murderer' even if Jones were innocent, and even if Smith had no murderer. Here, Jennifer's use of 'the murderer' is felicitous only insofar as she has a causal–perceptual acquaintance with Jones. In this case, Sera cannot grasp what Jennifer wishes to *get across* by her speech act unless Sera is also acquainted with Jones.

Donnellan held that the referential–attributive distinction demonstrated the incompleteness of Russell and Strawson's competing accounts. While Russell's univocal quantificational semantics could account for attributive usage, Donnellan held that it ultimately failed to explain referential usage. While Strawson's univocal referential semantics could account for referential usage, Donnellan claimed that it failed to make sense of attributive usage.[6] A proper account of definite descriptions, Donnellan argued, must explain *both* attributive and referential usage. Neither Russell nor Strawson could. So, Donnellan thought.

---

[3] This, at least, was the view that Strawson (1964, 1971/2004) eventually settled upon.

[4] Donnellan, for his part, framed the referential–attributive distinction in terms of whether there's some individual the speaker 'has in mind' when she uses a definite description. Following Devitt (1974), I will instead couch the referential–attributive distinction in causal–perceptual terms.

[5] The term '*get across*' is neutral between minimal propositions, explicatures, and implicatures.

[6] Indeed, Strawson's view, Donnellan noted, could not account for the referential success speakers had in misdescription cases.

But here's where things get tricky. The most straightforward way to construe Donnellan's criticism of Russell and Strawson is to take Donnellan to be endorsing the ambiguity thesis ('AT', hereafter). AT holds that English contains (at least) two semantically distinct but phonetically identical definite articles, 'the$_Q$' and 'the$_R$' respectively.[7] Within AT, attributive usage reflects the quantificational semantics of 'the$_Q$', referential usage the referential semantics of 'the$_R$'. But Donnellan found AT unappealing. Instead Donnellan claimed that the two uses in question reflected a 'pragmatic' ambiguity in 'the', not a lexical-semantic one.

But, as Kripke (1979) noted, a pragmatic categorization of Donnellan's distinction seems *prima facie* unproblematic for Donnellan's targets. After all, Russell and Strawson proposed *semantic* theories about the *semantic* content of the definite article. Any evidence against such views must have semantic import. The mere existence of the uses Donnellan observed doesn't.

Russellians would seize upon this opening. Surely Donnellan's observation's correct: English speakers do use definite descriptions attributively and referentially. But such usage is perfectly compatible with a univocal quantificational semantics. Attributive usage reflects the quantificational semantics Russell posited. When a speaker uses a definite description referentially, she exploits the definite article's quantificational semantics along with some pertinent contextual information in order to generate a singular proposition as a *pragmatic implication*.[8] Referential usage, then, is a pragmatic matter. It thereby doesn't trouble Russell. Indeed, as Kripke (1979) argued, any version of English that conformed to Russell's theory of definite descriptions, any 'Russell English' as Kripke labelled it, would be a version of English that nonetheless admitted of referential usage. In fact, English speakers sometimes use expressions that are uncontroversial cases of univocal quantifiers referentially. Following Davies (1981), consider 'most'.[9] From afar, Jennifer and Sera see five of their colleagues eating lunch. Jennifer and Sera mutually know that two of their colleagues have tenure while two don't. Sera asks whether Jessica, the fifth member of the group, has tenure. Jennifer, a member of the Promotion and Tenure Committee, knows that Jessica just recently received tenure. She'd like to tell Sera the good news. Still, she feels uncomfortable *directly* relating it. Here, Jennifer may use (5) to generate (6) as a pragmatic implication, and thus *indirectly* provide Sera with the information she's requested.

5. Most of the group has tenure.
6. Jessica has tenure.

Now, if a pragmatic explanation is necessary for referential uses of 'most' and if that pragmatic explanation seamlessly extends to referential uses of 'the', then Grice's Modified Occam's Razor ('MOR', hereafter) dictates that it should be so extended.[10] Donnellan had missed his mark. So, Russellians claimed.

---

[7] The notation is Neale's (1990).

[8] The *locus classic* is Neale (1990). But see Grice (1969), Kripke (1979), Bach (1981, 1987), Davies (1981), Evans (1982), Salmon (1982), and Soames (1986) as well.

[9] Davies's case centered around five philosophers and their respective criminal records. While I have changed the details of the case, the basic moral remains.

[10] MOR requires that theorists 'don't multiply senses beyond necessity'. See Grice (1989) for further details.

In this way, Russellians raised a general skepticism regarding AT as a necessary or adequate explanation of the referential–attributive distinction. To alleviate such skepticism, Devitt (1997, 2004) proposed a new argument, the 'argument from convention' ('AFC', hereafter).[11,12]

3 The argument from convention

AFC begins with an observation. Speakers *regularly*[13] use definite descriptions referentially. Such usage is *statistically common*,[14] a *systematic*[15] feature of English. There is a *ubiquitous* practice among English speakers of using definite descriptions to *get across* singular propositions. For their part, English audiences *regularly interpret* definite descriptions referentially. Indeed, the *default interpretation* of definite descriptions appears to be the referential one.[16]

AFC proponents hold that regular usage shares an intimate connection with semantic convention. The idea's this. The more often speakers use a particular expression to convey a particular content and the more often hearers interpret the use of a particular expression to convey a particular content, the *more likely* it is that such usage *is* or *will become* a semantic convention. This intimate connection between usage and convention seems a natural consequence of the Lewis–Schiffer approach to conventions that AFC proponents endorse.[17] For example, Devitt (2006, 2013b) maintains that a semantic convention for an expression *e* obtains in a particular speech community whenever:

    (i) The community's members share a communicative disposition to associate *e* with a particular speaker meaning, and

---

[11] The label's Neale's (2004). Buchanan and Ostertag (2005), Pupa (2008), Amaral (2008, 2010), and Abbott (2010) also endorse AFC. While Neale (2004) remains skeptical that AFC provides definitive support for AT, he nonetheless accepts that AFC has truth-conditional import. And while Elbourne holds that AFC does not '[land] a knock-out blow on the traditional Russellian account' (2013: p. 109), he does acknowledge that AFC casts traditional Russellianism in a less compelling light.

[12] Reimer (1998) also formulates and advocates AFC. But, she does not put AFC in the service of AT but rather argues in favor of the pragmatic ambiguity thesis that Recanati (1989, 1993) and Bezuidenhout (1997) endorse (for details, see Sect. 6 and fn. 62). I will put her particular brand of AFC to the side in Sects. 3–5. The two major criticisms of AFC are directed at the use of AFC to vindicate AT. They are directed at Devitt's version of AFC. Dialectical coherence, then, obligates the initial sidelining of Reimer's AFC. Now, I think that Reimer's AFC has not been the subject of direct criticism in virtue of the fact that it seeks to substantiate the pragmatic ambiguity thesis, a thesis that most theorists, following Neale (1990: pp. 110–112), take to be a non-starter. For instance, both Devitt (2007: fn. 9) and Bach (2007: fn. 4) approvingly cite Neale's argument against Recanati's pragmatic ambiguity thesis. Taking Zacharska's (2010) lead, I argue in Sect. 6 that the real problem for AFC is its indeterminacy with respect to AT and the pragmatic ambiguity thesis. In fn. 62, I argue that Neale fails to demonstrate that Recanati's proposal is "highly artificial" (Neale 1990: p. 112).

[13] See Devitt (2004).

[14] See Reimer (1998).

[15] See Neale (2004).

[16] See Klein (1980).

[17] See Lewis (1969) and Schiffer (1972).

(ii) The community members share the disposition in question because each member takes the other members to also have the same communicative disposition.[18,19]

(Devitt 2013b: pp. 98–99).

On this gloss, it's not hard to imagine that when a particular use of a particular expression is widespread among a community, there is a shared communicative disposition to use the particular expression in the manner in question. Nor, I suppose, is it hard to imagine that frequent usage of an expression in a particular manner causes community members to have a particular communicative disposition to use the expression in the particular manner *because* each member notices that every other member has the disposition in question. Of course, semantic conventions may arise from irregular usage. Perhaps a semantic convention originates in an authoritative stipulation. In the same vein, a semantic convention may emerge because a particularly charismatic community member with a big enough platform engages in some novel speech behavior that her audience immediately internalizes. And, of course, regular usage needn't establish semantic conventions. Consider 'try'. We regularly use 'try' to *get across* 'not succeed'. All the same, 'try' just means *try*.[20] Still, over time, it seems reasonable to hold that the regular use of an expression to convey a particular message tends to crystalize into a semantic convention.[21]

---

[18] Devitt holds that his view of semantic conventions is less intellectualized and thus fuzzier than Lewis or Schiffer's. The view is less intellectualized since it's couched in terms of dispositions as opposed to mutual knowledge. The conception is fuzzier insofar as it leaves opaque the precise mechanisms that ground the community's shared dispositions. Nothing here hinges upon these features. For further discussion, see Devitt (2013b). Reimer (1995) subscribes to the traditional Lewis-Schiffer view that invokes mutual knowledge. Bach (1995), a Russellian and AFC critic, does too.

[19] Two points are in order. First, Lewis claims that conventions are, at bottom, *arbitrary*. Now, one might hold that the referential use of definite descriptions is not arbitrary. After all, the definite article must have the right semantic properties in order to lend itself to such usage. This, I think, is a misreading of Lewis's notion of *arbitrary*. For Lewis (1969: pp. 69–70), a convention is arbitrary in the sense that an alternate convention *could have* arisen in place of the actual convention. On this gloss, referential definite descriptions are *arbitrary*. For example, English speakers *could have* encoded the referential properties of the definite article in a different lexical item and thereby used a different lexical item to refer via those properties. Second, one might claim that a speaker that uses a definite description referentially need not conform to (ii). The speaker, one might allege, could hold an agnostic view regarding the communicative dispositions of her audience. I'm skeptical. An agnostic speaker, I think, would be a terribly uncooperative speaker. If a speaker wishes to refer to a particular individual and also wishes to be *understood* by her audience, then she will employ a lexical item that she takes her audience to be disposed to use for similar purposes. Relatedly, a cooperative hearer that wishes to *properly interpret* a speaker's referential speech act must take the speaker to be employing a lexical item that the hearer herself would be disposed to use for such purposes were she the speaker. I would like to thank an anonymous reviewer at *Synthese* for raising these issues.

[20] The example's Grice's (1989: p. 43). Since there is no entailment relation between 'try' and 'succeed', neither <'try', 'succeed'> nor <'succeed', 'try'> form Horn scales [see Horn (1972)]. Thus, the movement from 'try' to 'not succeed' is not a case of scalar implicature. For a discussion of scalar implicatures and overgeneration, see Sect. 4.1.

[21] Here, I follow Kripke, a prominent Gricean:

I find it plausible that a diachronic account of the evolution of language is likely to suggest that what was originally a mere speaker's reference may, if it becomes habitual in a community, evolve into a semantic reference. (1979: p. 249).

On this score, the regular use of definite descriptions referentially (along with the regular interpretation of definite descriptions as such) strongly suggests that there is a *semantic convention* that underwrites such usage. Sure, there are referential uses of uncontroversial quantifier phrases such as 'most'. And, sure, such usage demands a pragmatic explanation. But referentially used definite descriptions are *qualitatively distinct* from such usage. The referential use of 'most' is a relatively one-off event. To repeat, the referential usage of definite descriptions is a commonality. English speakers can only use 'most' referentially in contextual circumstances that do not invariably or even normally occur. They are heavily context-dependent. Moreover, their production and interpretation call for sophisticated inferences of the Gricean variety. In a word, the referential use of 'most' counts as *indirect* usage. In contrast, English speakers regularly use definite descriptions referentially across a diverse set of contextual circumstances. Their usage is relatively independent of the contextual circumstances in which they are so used. Their production and interpretation requires no sophisticated Gricean-style inferences. Such usage is *direct*. For these reasons, AFC proponents claim that the *best* explanation for the ubiquity of referentially used definite descriptions is that such usage reflects the *semantics* of the definite article. Referential usage, then, appears to have *semantic import*. English, it seems, does contain 'the$_R$'. AFC, then, serves to substantiate AT.[22]

4 Overgeneration

Several theorists charge that AFC overgenerates.[23] If regular usage is a sufficient basis for granting semantic import to referential usage, so the argument goes, one will have to label uncontroversial cases of regularized pragmatic phenomena semantic. This, it

---

Footnote 21 continued

As Grice (1989: p. 43) himself notes, what's implied may later become conventionalized. Elsewhere, Grice expresses the point this way:

> …It may not be *impossible* for what starts life, so to speak, as a conversational implicature to become conventionalized. (39: my emphasis).

It's hard to imagine what else besides regular usage or authoritative stipulation would be able to mold a pragmatic effect into a semantic convention. For some evidence of the tight connection between regular usage and convention, see Sect. 4.2.

[22] Strictly speaking, AFC only substantiates the existence of 'the$_R$', not 'the$_Q$'. So, AFC doesn't fully ground AT. In place of an argument for the existence of 'the$_Q$', AFC proponents *simply grant* that the definite article has at least a quantificational semantics (Reimer 1998: p. 92; Devitt 2004: p. 280; Amaral 2008: p. 288). That is, AFC proponents exhibit an 'attributive bias'. Since AFC critics also tend to exhibit the same bias, I will proceed on the assumption that AFC is an argument for AT. This, in the end, *is* a harmless assumption. After all, AT proponents could easily extend AFC to cover 'the$_Q$' just so long they demonstrate what seems independently uncontentious, namely that attributive usage is a regular phenomenon.

[23] The overgeneration argument against AFC originates with Bach (2004), a Russellian. See Bach (2007) as well. An earlier version, used for different purposes, appeared in Bach (1995). Bontly (2005), Schoubye (2012), who defends Heim's (1988) theory of definite descriptions, and Zacharska (2010), who defends Recanati's (1989) theory of definite descriptions, have all put forward overgeneration arguments against AFC.

is claimed, is a terrible consequence, one we should take pains to avoid. Thus, some theorists hold that there's a good reason to cast a skeptical eye on AFC.

This worry seems unmoving. Sure, AFC overgenerates. But when coupled with MOR, Russellian accounts that frame referential usage as a pragmatic phenomenon also overgenerate. That is, they either lend themselves to the miscategorization of obviously semantic phenomena as pragmatic or they lend themselves to the miscategorization of obviously pragmatic phenomena as semantic. In this area, overgeneration is an inherent vice.

4.1 Take one: generalized conversational implicatures and standardization

Grice (1989) distinguished between two classes of conversational implicature. There are particularized conversational implicatures ('PCIs', hereafter) and there are generalized conversational implicatures ('GCIs', hereafter). With Levinson (2000), we may distinguish PCIs and GCIs as follows:

> **PCI** An implicature *i* from an utterance *u* is particularized *iff u* implicates *i* **only** by virtue of specific contextual circumstances that would not invariably or even normally obtain.
>
> **GCI** An implicature *i* from an utterance *u* is generalized *iff u* implicates *i* **unless** there are unusual specific contextual circumstances that defeat it.

<div align="right">(Levinson 2000: p. 16)</div>

To illustrate, consider two cases. While baking cookies, Sera had left her windows closed. As a result, her apartment had become hot and stuffy. Outside, a cool breeze swirled. Jennifer, visibly sweating, remarked that:

1. It's HOT in here.

In this context, Jennifer generates (2) as a PCI:

2. Please open the windows!

Without a mutual recognition of the apartment's temperature, the cool breeze outside, Jennifer's uncomfortableness, the ability to alleviate Jennifer's uncomfortableness by opening the windows, and Sera's eagerness to please, Jennifer would not be able to generate (2) via (1). This we may contrast with (3) and (4).

3. I ate SOME cookies.
4. I did not eat EVERY cookie.

When Jessica tells Stephanie (3), she generates (4) unless there is some glaring contextual reason for rejecting (4). So, (4) counts as a GCI. Indeed, whenever a speaker uses a sentence exemplifying (5), she generates (6) unless context intervenes:

5. Subject Verb Some F
6. Subject NOT Verb Every F

The very *form* of the sentence seems to secure the conversational implicature in question.

For Grice, a GCI counts as a regular conveyance of the sentence that speakers use to generate it. This makes GCI theoretically interesting. With GCIs, speakers *regularly* use expressions of a certain form to *get across* particular messages without those messages serving as the semantic content of the expressions used. GCIs are a wedge between semantic content and regularity.

Bach (1995) holds that standardization is another kind of regularized pragmatic implication. When a speaker uses an expression in a standardized manner, she exploits a *streamlined* Gricean-style inference on the part of her audience in order to convey something distinct from the literal content of the expression used.[24] The robust pragmatic inference becomes *short-circuited* through *precedent*. The length of the inference diminishes on the basis of previous such uses of the expression. Whereas an expression possesses its literal content on the basis of a mutual recognition that it possesses such content, there's no requirement that what the expression is standardly used to *get across* needs to be the subject of mutual recognition. This distinguishes standardization from conventionalization.

Consider (7) and (8):

7. Can you pass the salt?
8. Please pass the salt!

With regard to its literal semantic content, (7) is a request for information about the audience's *capacity* to pass the salt. We can easily imagine the question being posed to someone undergoing physical therapy for a broken arm. More clearly, when one *embeds*[25] (7) in the antecedent of a conditional, it retains its 'capacity' reading:

9. If you can pass the salt, we can set an appointment to remove your cast.

Now, speakers regularly use (7) as an indirect (and therefore polite) way to make a request. Indeed, it's a standard way to request that the salt be passed. We can imagine that the first use of (7) as a request required a lot on the hearer's side of the ledger. The hearer would have had to retrieve the literal content of (7) (i.e. an interrogation about a physical capacity), note that such a speech act would be odd given the speaker's presumed cooperative disposition along with the speaker and hearer's shared beliefs regarding the hearer's physical capacities, note that a request to pass the salt requires assent from the hearer, *infer* that the speaker couldn't have meant the literal content of (7), and thereby *infer* that the speaker meant (8). Eventually, hearers would move right from the 'form' of (7) to (8), without retrieving (7)'s literal content or checking the context for a mismatch between literal content and presumed speaker cooperation in the process. Speakers can then exploit such streamlining and regularly *get across* something distinct from the expression's literal semantic content. All the same, the literal content of (7) concerns capacities not requests.

---

[24] Bach distinguishes between standard indirections and standard non-literality. According to Bach, the former are cases such as 'I order you to leave the room' where a speaker uses an expression to convey two things: (a) the expression's literal semantic content (or performative force) and (b) something more than the expression's literal semantic content (or performative force). For Bach, the latter are cases like (7) where speakers use an expression to *convey* something *other* than the expression's literal content. Bach (2004) argues that the referential use of definite descriptions falls into this camp.

[25] For an overview of the interaction between standardization and sentential embedding, see Bach (1995).

Like a GCI, the standard conveyance of an expression will stand as its regular interpretation *unless* context stifles the interpretation. Like GCIs, standardizations move from the form of an expression to some non-semantic content. And like GCIs, standardizations fit snuggly between semantic content and regular usage.

Now, if GCIs and standardizations are regularized, then the logic of AFC seems to dictate that they are semantic in nature, not pragmatic. But, that hardly seems to be the case. 'Some' means *some* not *some but not all*, 'can' quantifies over capacities not permissibility. So, Russellians may, with plausibility, maintain that AFC overgenerates: it's liable to treat *pragmatic regularities* as *semantic conventions*.

What's more, Russellians may charge that AFC casts doubt only on those univocal accounts that assimilate referential usage to PCIs. That, it seems, is all AFC demonstrates. Russellians, for their part, are not wedded to a PCI account of referential usage. After all, the referential use of definite descriptions does diverge from the referential use of 'most'. Russellians may contend that while the former is a kind of pragmatic regularity, the latter is a PCI. Surely, Russellian–Griceans[26] need not lose sleep over AFC's meagre conclusion.

4.2 Take two: dead metaphors

Through an appeal to pragmatic regularities, AFC critics remind us that there is a border separating mere regular usage and conventional meaning. The point is well taken. We cannot hastily jump from the regularity of referential usage to the semantic import of such usage. Still, the border separating regular usage and semantic convention is quite porous. As we saw in Sect. 3, there is no denying that the regular usage of a particular linguistic item to convey a particular message will *often* result in the establishment of a linguistic convention. To repeat, there is a very tight connection between regularity and convention. To deny such a connection is to risk overgenerating in a different direction. It is to risk treating *semantic conventions* as *pragmatic regularities*.

Take dead metaphors.[27] A metaphor 'dies' when the phrase speakers use to convey the metaphor 'absorbs' the metaphor. One pronounces a metaphor dead when the metaphorical message becomes semantically encoded. Consider 'hardwired'. Originally, the phrase was univocal. In particular, 'hardwired' univocally denoted a characteristic that 'contain[ed] or involve[ed] permanently connected circuitry for implementing a specific, unchangeable function'.[28] Within some circles, primarily ones composed of cognitive scientists, speakers would riff on this univocal adjective, using 'hardwired' as a metaphor for an organism's genetically-determined features.[29]

---

[26] The label is Neale's (1990). Neale (1990) and Bach (1987), the two main targets of AFC, fall into the Russellian–Gricean camp. Neale (2004) does claim that AFC extends to GCI accounts of referential usage (see Sect. 6). In this way, Neale (2004) maintains that his previous analysis of referential usage needs replacing. For details, see Pupa (2008).

[27] The term appears in Davidson (1978). See Saddock (1978), Levinson (1984), Devitt (1997, 2004), Reimer (1998), and Pupa (2008) for overgeneration arguments that employ dead metaphors.

[28] *Oxford English Dictionary Online* (Accessed February 19, 2016).

[29] *Oxford English Dictionary Online* (Accessed February 19, 2016) credits a 1971 *New Scientist* article with the first publication of such a use.

Over time, the metaphorical usage spread. The metaphorical usage became regularized. Eventually it stuck.[30] And so here we are. 'Hardwired' is now equivocal. The phrase encodes matters pertaining to electrical engineering as well as matters pertaining to nativism. That is, English contains both $hardwired_{<+\text{nativism}>}$ and $hardwired_{<+\text{engineering}>}$.[31] Indeed, in most contexts, the default interpretation of 'hardwired' is $hardwired_{<+\text{nativism}>}$.

Now suppose that metaphor is a wholly pragmatic affair.[32] If so, a 'fundamentalist Gricean'[33] might argue that metaphors never die. Rather so-called 'dead' metaphors are just instances of regularized pragmatic phenomena. After all, if we can assimilate dead metaphors to pragmatic regularities, we can deny the deceased metaphors semantic import. We can adhere to MOR. Here, the fundamentalist Gricean might maintain that although speakers regularly use 'hardwired' to *get across* $hardwired_{<+\text{nativism}>}$, there's nonetheless a gap between regular usage and semantic convention. 'Hardwired', the fundamentalist might claim, semantically encodes $hardwired_{<+\text{engineering}>}$. English speakers *regularly* play upon the word's meaning to *implicate* that a particular characteristic of an organism is determined genetically. The fundamentalist thereby rejects the claim that 'hardwired' means (among other things) $hardwired_{<+\text{nativism}>}$.

Surely this position is untenable. The semantic conventions governing 'hardwired' do not pertain *simply* to electrical engineering. More generally, natural languages abound in dead metaphors. Indeed, the absorption of metaphorical content plays a powerful role in the evolution of natural language semantics. Moreover, speakers who employ dead metaphors are often unaware that the phrases in question ever had a meaning other than their 'metaphorical' one.[34] Such conversational participants could not then exploit the principles of cooperative conversation to *implicate* the 'metaphorical' meaning from the phrase's presumed original and univocal meaning.[35]

Now, the Russellian must demonstrate that her pessimism regarding the semantic import of referential usage doesn't similarly bleed into a pessimism regarding dead metaphors. If the AT opponent denies the semantic import of regularized referential usage, she seems committed to denying the semantic import of dead metaphors. The pragmatic reasoning that applies to referential usage seems to seamlessly carry over to the fundamentalist's reasoning concerning dead metaphors. A troubling result, to be sure.

---

[30] For example, popular news agencies regularly use 'hardwiring'-talk in framing issues surrounding brain differences between various populations. A case in point: NPR correspondent Singh (2015) recently wondered 'Why some teen brains may be hardwired to make risky choices'.

[31] For an alternative take on the meaning of 'hardwired', see Sect. 6.

[32] For a critical overview of some competing theories of metaphor, including the Gricean theory, see Camp and Reimer (2006).

[33] The term's Devitt's (2004).

[34] See Reimer (1998) and Devitt (2004).

[35] As Devitt (2004: p. 285) notes, the metaphor remains dead even in cases where a speaker knows the historical evolution of the lexical item and therefore *can* provide a pragmatic derivation of the previously metaphorical meaning from the original literal meaning.

4.3 Inherent vice

So where are we?

AFC proponents claim that AT provides the *best* explanation for the regularized referential use of definite descriptions. AT opponents disagree. They claim that AFC overgenerates semantic conventions. For this camp, the best explanation remains the simplest: definite descriptions are univocal; one category of usage has semantic import, the other category lacks such import. AFC proponents counter that pragmatic explanations of regular usage overgenerate pragmatic regularities. This group views AT as explanatorily superior. From the AFC proponent's perspective, the universe seems unconcerned with ontological simplicity.

So what are we to do?

4.3.1 From pragmatic regularities to semantic conventions? Devitt's response

In response to the charge of overgeneration, Devitt (2007) argues that referential usage diverges from paradigm instances of regularized pragmatic implicatures. All pragmatic implicatures, Devitt claims, involve a speaker *expressing* some proposition p in order to *implicate* some other proposition q. For Devitt, if a speaker uses a sentence to implicate q, she inevitably expresses p through her use of the sentence. This 'implication generalization', Devitt maintains, fails to subsume the referential use of definite descriptions. As such, Devitt concludes that referential usage is not a matter of pragmatic implication, regularized or otherwise. The AFC proponent may then partition referential usage from textbook cases of regularized pragmatic implicatures. Here's how Devitt proceeds.

Referentially used definite descriptions are usually *incomplete*.[36] That is, definite descriptions usually house noun phrases that denote sets containing at least two members. Devitt claims that a speaker who uses an incomplete definite description referentially may on occasion be unable to restrict the definite description so that it uniquely denotes her intended referent. Sometimes, the speaker will be *ignorant* of the information that would uniquely denote her intended referent. Other times, she will simply be *wrong* about the information in question.[37] As a result, some incomplete definite descriptions will also be *incompleteable*. All the same, speakers can use incompleteable definite descriptions referentially and thereby *get across* singular propositions.[38] Consider a case from Wilson:

> Suppose that Jean Valjean, as a very old man, has only the spottiest memory of his life up to a year before the events now to be described. One day while walking through Paris he catches sight of a face that fills him with fear and foreboding. When he sees the face he cannot say what it is that he so dreads, and he continues through the day brooding over his troubled state of mind. By later that evening two things have developed. He has totally forgotten that it was the sighting of

---

[36] See Devitt (2007) and Bach (2007).

[37] See Wilson (1991) and Devitt (2007).

[38] See Wilson (1991) and Devitt (2007).

the face that gave rise to his continued emotional disturbance and, at the same time, he comes to be seized with the conviction that, as he expresses it, 'My old enemy is back in Paris'. In fact, it is this thought that he now takes to be the object of his fear.

…despite the fact that Valjean is unable to summon to mind any helpful representation of Javert, he would be able to recognize the Javert of his past as the man he has been thinking of all along. As facts of this kind are assembled, it becomes hard to deny that Valjean's use of 'my old enemy' does refer to Javert even though nothing like a uniquely identifying description has been provided. (1991: p. 373).

When a referentially used definite description is incompleteable, a speaker will not be able to supplement the definite description so that it uniquely identifies her intended referent. Since the referentially used definite description is also incomplete, it will not uniquely identify the speaker's intended referent. So, when a speaker uses the incompleteable definite description referentially in an utterance of 'the F is G' and thereby *gets across* the singular proposition that α is **G**, Russellians cannot claim that the speaker *expressed* (10):

10. [The x : **F**x & ®](**G**x)[39]

This leaves Russellians with exactly one candidate for the general proposition speakers *express* in such cases, namely (11):

11. [The x: **F**x](**G**x)

But, (11)'s a nonstarter. In the case at hand, (11) is as an obvious falsehood that, in its absolute generality, fails to uniquely identify any particular **F**. As such, there is no way for a speaker to conversationally exploit (11) in order to *implicate* a singular proposition about a *particular individual*. Here, the speaker has no reason to believe that her audience will be able to grasp that she means to *convey* that α_F is **G** rather than β_F is **G**. If the speaker *expresses* anything, it is the singular proposition she *gets across*, namely α is **G**. So, referential usage has semantic import.

Referential usage, then, seems more akin to dead metaphors than to regularized pragmatic implicatures. When Jennifer says (12) she *expresses* (13) alone. More to the point, she does not express (14) and thereby implicate (13):

12. Grammatical principles are hardwired.
13. Grammatical principles are hardwired$_{<+\text{nativism}>}$
14. Grammatical principles are hardwired$_{<+\text{engineering}>}$

Now consider some paradigm cases of regularized implicature. When Jennifer says (15)–(16), she *expresses* (a) and thereby *implicates* (b):

15. SOME abilities are innate.
    a. Some abilities are innate.
    b. Not every ability is innate.

---

[39] Here, '®' denotes a formula that, when conjoined to **F**x, produces a formula that α uniquely satisfies.

16. Can you stop tapping your pen?
    a. Do you have the capacity to stop tapping your pen?
    b. Please stop tapping your pen!

Regularized implicatures are different. Unlike referential usage and dead metaphors, regularized implicatures obey the implication generalization. This difference, Devitt believes, circumvents the overgeneration charges facing AFC. Regularized implicatures carry two *identifiable* messages, semantic conventions only one. Overgeneration averted.

4.3.2 Some problems with Devitt's response

Or is it? Devitt's overgeneration defense seems problematic. The response relies crucially on the presumption that some referential uses of definite descriptions are indeed *incompleteable*. But this presumption seems unfounded. When a speaker successfully uses a definite description to refer to a particular individual, the speaker must, at minimum, have some intimate causal connection to the individual in question. As Donnellan would have it, the speaker must have the individual 'in mind'. But if that's the case, the speaker must be able to provide a *minimal* completion of the definite description that accurately and uniquely denotes her intended referent.[40] In particular, the speaker should be able to use a 'Gödelian'[41] completion to restrict the reach of her definite description:

17. '$x = \alpha$'

Thus, the view that some referential uses of definite descriptions are *incompleteable* seems meritless. Looking back to a revolutionary France, if Jean Valjean successfully uses 'my old enemy' to refer to Javert, then Jean Valjean must be able exploit a Gödelian restriction containing Javert as a proper part.

18. My old enemy is back in Paris.
19. My old enemy **who's identical to Javert** is back in Paris
20. [The x: **Enemy** (x, *Jean Valjean*) & $x = Javert$](**In** (x, *Paris*))

Otherwise, there's no reason to believe that Jean Valjean referred to Javert and not Little Gervais.[42]

Russellians may utilize Gödelian completions in service of two distinct pragmatic accounts of referential usage. First, Russellians may claim that when a speaker uses a definite description referentially to refer to α in an utterance of 'the F is G', she *gets across* two *general* propositions. The speaker *expresses* (21) thereby *implicating* (22):

---

[40] See Neale (2004).

[41] The label is Neale's (2004). See Kripke (1973/2013), Lepore and Ludwig (2000), and Neale (2004, 2005) for Russellian proposals of this nature. Elbourne (2005) employs Gödelian completions in the service of a univocal referential semantics; Wilson (1978) does so while defending a univocal predicational semantics.

[42] As an anonymous reviewer for *Synthese* notes, the Jean Valjean case is rather odd. Given his state of confusion, it is unclear whether Jean Valjean does successfully refer to Javert. So, Russellians might simply reject this particular case out of hand. Still, Gödelian completions provide Russellians with a fail-safe: *if* Jean Valjean successfully refers to Javert, he does so through the employment of a Gödelian completion.

21. [The x: **F**x](**G**x)
22. [The x: **F**x & $x = \alpha$](**G**x)

'Implication Gödelianism' respects the implication generalization. Moreover, this form of Gödelianism posits a form of implicature that perhaps one may justly label 'regularized'.[43]

Of course, there seem to be pragmatic processes that affect what's expressed.[44] For these processes, the implicature generalization is inoperative.[45] Second, then, Russellians may claim that when a speaker supplements an incomplete definite description with a Gödelian completion, she engages in a *regularized* pragmatic process that impinges upon what's *expressed*, not what's *implicated*. Within 'enrichment Gödelianism', a Russellian may hold that when a speaker makes a referential use of the definite description in 'the F is G', she *expresses* (22) and nothing else.[46] The implication generalization is thereby rendered irrelevant.

Whether Russellians gravitate towards implication or enrichment Gödelianism, the result will remain unaltered. Devitt's response will have failed to rule out the possibility that referential usage is a species of some regularized pragmatic phenomenon. Thus, when a theorist claims that a referential semantics underwrites referential usage she may give license to others to label non-semantic phenomena semantic.

Devitt's response leaves us just as we were. There's a non-negligible possibility that the logic propelling AFC will lead to pragmatic regularities, both *implicative* and *non-implicative*, being unjustly branded semantic conventions. The overgeneration problem remains.

4.3.3 Russellianism and overgeneration

While defending a pragmatic account of referential usage, Bach claims that the overgeneration charges against Russellians are unfounded. The pragmatic strategy the Russellian implements, we are told, diverges from the strategy the fundamentalist Gricean uses to handle dead metaphors. The crucial difference between the two strategies, Bach claims, is that a speaker's use of, say, 'hardwired' to convey *hardwired*$_{<+\text{nativism}>}$ does not involve the manipulation of *hardwired*$_{<+engineering>}$. However, the 'quantificational character' of definite descriptions plays a role in referential usage. Bach explains:

---

[43] Lepore and Ludwig (2000) maintain that many referential uses of definite descriptions may be subject to such a strategy.

[44] See Sperber and Wilson (1986/1995) and Recanati (1993).

[45] See Zacharska (2010: p. 61) as well.

[46] See Neale (2004, 2005). While enrichment Gödelianism is a form of Russellianism as it provides a univocal Russellian analysis of the definite article, it is not a form of Gricean Russellianism as it frames referential usage not as a kind of conversational implicature but rather as a kind of free enrichment. Now, Devitt (2007) argues that enrichment Gödelianism is Russellian in name only, that the enrichment Gödelian redundantly builds into incomplete definite descriptions the causal–perceptual link that helps to determine the reference of a referentially-used definite description. For further discussion of enrichment Gödelianism, see Sect. 6.

In sum, the quantificational character of a definite description plays a role in its referential use. The speaker thinks of a certain object, takes that object to be the **F**, and uses 'the F' to refer to it. The speaker, on hearing 'the F', thinks of a certain object that he takes to be the **F**, and takes that to be what the speaker is referring him to. His thinking of that object may depend in part on the fact that it is the only plausible candidate for what the speaker has in mind in using 'the F'. (2004: p. 203).

When a phrase is ambiguous, one of its encoded meanings is inoperative. When a speaker uses an unambiguous phrase to regularly *get across* some particular message, what the phrase encodes will involve itself in the communicative process. The referential use of definite descriptions invokes quantification, uniqueness particularly. Thus, such usage does not reflect a referential semantics. The use of dead metaphors not does invoke the previous literal content; the metaphorical message imprints onto the semantic architecture of the phrase. In this way, referential usage is similar to regularized pragmatic implicatures, not dead metaphors.

Essentially, Bach holds that Russellians can distinguish dead metaphors from referential usage at the level of processing. In the case of dead metaphors, the hearer does not go through a Gricean inference, no matter how streamlined. Instead, the hearer leans upon a convention that is grounded in mutual knowledge. In the case of referential descriptions, the hearer must be able to invoke a Gricean inference, albeit one that's been compressed through regular usage.

There are three problems with Bach's position.

First, as Devitt (2013a) demonstrates, Bach's position is *evidentially inadequate*. When an inference is compressed, it is shortened in length. Presumably, the more a language community uses a standardization, the more compressed its accompanying inference becomes. Now, the more compressed an inference becomes the shorter it is rendered. At some point, then, the compression of an inference will eliminate the inference entirely. That is, popular standardizations, in the long run, produce conventions. This, Devitt notes, is exactly what happens when metaphors die. Here, then, a question emerges: what evidence can Bach adduce to demonstrate that while dead metaphors have compressed into conventions referential usage hasn't (yet)? Popular standardizations involve increasingly shorter inferences. These inferences largely occur at an unconscious level. If referential usage is a kind of standardization, then it's rather well-worn. Thus, its accompanying inference is exceedingly short. Presumably, the inference is an unconscious one. Now, as Devitt points out, there's no clear indication that, in cases of referential usage, hearers do in fact process a quantificational reading of the definite article before turning their attention to a referential reading. As such, there simply isn't any evidence to support the claim that referential usage is a mere standardization. At the evidential level, Bach fails to distinguish dead metaphors from referential usage.[47]

Second, Bach's defense rests on the presumption that if some quantificational feature plays a role in the use of some phrase, the phrase has a quantificational semantic type. Strictly speaking, this claim is false. Semanticists often couch the reference con-

---

ditions of referential terms in the language of quantification. Elbourne (2005: p. 97), for instance, provides (23) as a lexical entry for the third-person feminine singular pronoun:

23. $\llbracket \text{ She } \rrbracket = \lambda f : \exists! x f x \,\&\, \forall x (f x \rightarrow \textbf{Female } x). \, \iota x f x$

On this treatment, 'she' is a referential article whose reference conditions are specified through quantification, particularly uniqueness. So, the *assumption* that uniqueness plays a role in referential usage does not provide particularly strong reason to treat such usage as a purely pragmatic affair. After all, the quantificational element could be embedded within the semantics of a phrase whose semantic type is referential.

Third if definite descriptions became ambiguous in a manner similar to the way metaphors die, then, as Amaral (2008) notes, one would predict that referential and attributive usage would share some intimate conceptual link and that referential and quantificational descriptions would thereby share some semantic ties. For instance, $hardwired_{<+\text{nativism}>}$ and $hardwired_{<+\text{engineering}>}$ center upon the architecture of an item. Both signal that an item has some structure that is unalterable. Now the dual use of definite descriptions occurs cross-linguistically.[48] Like dead metaphors, cross-linguistic phenomena are bound to share some intimate conceptual links. They are therefore prone to sharing some semantic overlap. So if definite descriptions are ambiguous and if the process that rendered them ambiguous is akin to the process that disposes of metaphors, then we would expect that referential and attributive definite descriptions would contain similar semantic features. So, the claim that a uniqueness condition underwrites referential usage is entirely consistent with AT. Indeed, it's predictable!

The picture's not any rosier for Gödelianism. The implication Gödelian must distinguish herself from the fundamentalist Gricean. Like Bach, the only serious resource is to claim that quantification underwrites referential usage and therefore such usage diverges from dead metaphors. Of course, this won't do. As we just saw, quantificational elements may help to determine the reference conditions of a term.

Interestingly, the enrichment Gödelian does not seem to overgenerate pragmatic regularities. Within enrichment Gödelianism, referential usage reaches the level of a convention, one with truth-conditional import. Since the enrichment Gödelian accepts that referential usage reflects a convention, her analysis of such usage may increase the likelihood that pragmatic phenomena are mislabeled. The enrichment Gödelian will face the same overgeneration problem the AFC proponent faces.

For the enrichment Gödelian, the definite article is a univocal quantifier. Sentences that contain definite descriptions, the enrichment Gödelian maintains, provide 'blueprints'[49] that house gaps. These gaps may dissipate. In such cases, the sentence's content matches its compositional semantics. Alternatively, speakers may plug the

---

[48] See Larson and Segal (1995), Neale (2004), and Amaral (2008).

[49] The term's Neale's (2004).

gaps by leaning on two distinct *conventions*. The first convention substitutes the gap for a set; the second places a Gödelian completion there instead.

24. [The $x : \mathbf{F}x\,\mathbf{_{GAP}}](\mathbf{G}x)$ BLUEPRINT
25. [The $x : \mathbf{F}x](\mathbf{G}x)$ DISSAPATION   : NO SUPPLEMENTATION
26. [The $x : \mathbf{F}x\,\&\,\mathbf{H}x](\mathbf{G}x)$ CONVENTION$_1$: SET SUPPLEMENTATION
27. [The $x : \mathbf{F}x\,\&\,x = \alpha](\mathbf{G}x)$ CONVENTION$_2$: GÖDELIAN SUPPLEMENTATION

If, as Neale suggests, referential usage is a conventional way of completing a definite description, then what's to stop a semantically-inclined theorist from treating regularized pragmatic phenomena as matters of convention? Consider 'can' once more:

28. Can I leave?

As we've seen, speakers regularly use (28) to *get across* (29):

29. Do I have permission to leave?

Such usage is amenable to treatment paralleling enrichment Gödelianism:

30. **Can**$_{<+\text{possible}\,\mathbf{GAP}>}$ I leave?   BLUEPRINT
31. **Can**$_{<+\text{possible}>}$ I leave?   DISSAPATION : NO SUPPLEMENTATION
32. **Can**$_{<+\text{possible},+\text{capacity}>}$ I leave? CONVENTION$_1$: CAPACITY SUPPLEMENTATION
33. **Can**$_{<+\text{possible},+\text{permission}>}$ I leave? CONVENTION$_2$: PERMISSIBILITY SUPPLEMENTATION

I imagine, however, that most theorists would not want to conclude that a convention governs the 'permissibility' use of 'can'. As such, enrichment Gödelianism seems suspect in precisely the same way that AFC does. In both cases, there's a possibility that pragmatic regularities get swept up in the rush to label the referential usage of definite descriptions conventional.[50]

4.4 A summary

To recap: AFC proponents often meet with charges of overgeneration. These charges seem fair enough. The internal logic of AFC lends itself to the misrepresentation of pragmatic regularities as semantic conventions. However, pragmatic accounts of the referential usage of definite descriptions, when paired with MOR, face overgeneration problems too. The internal logic of such accounts also lend themselves to the misrepresentation of linguistic phenomena. This, I think, shows that neither side gains much from trading allegations of overgeneration.

To a large degree, the possibility of overgeneration should be expected. The gap between regular usage and semantic convention certainly exists. But sometimes you have to squint to see it. Moreover, all parties provide accounts of Donnellan's distinction largely in analogical terms. Analogies breed counterexamples. It's not surprising that each opposing camp can point to some region on one side of the divide that will

---

[50] Interestingly, the overgeneration charge against enrichment Gödelianism is of a piece with Neale's (1990: p. 112) overgeneration charge against Recanati's (1989) pragmatic ambiguity account of Donnellan's distinction. For details, see fn. 62.

receive mistreatment if we label referential usage semantic (pragmatic). Counterexamples are to be expected. That's just the cost of business.

Let's now turn to a second alleged problem with AFC, namely its evidential basis.

5 Evidence

As we saw, AFC centers upon empirical claims pertaining to the use of definite descriptions, uncontroversial quantifiers, and uncontroversial referential devices. First, AFC proponents claim that the referential use of definite descriptions diverges from the referential use of uncontroversial quantifier phrases. AFC, remember, holds that the former is ubiquitous among English speakers, the latter an infrequent occurrence. Second, AFC proponents insist that, absent elaborate stage-setting, speakers regularly and successfully use definite descriptions referentially and that hearers regularly and successfully interpret such usage *immediately* and with an *ease* unexpected for paradigmatic pragmatic phenomena. The same cannot be said for uncontroversial quantifier phrases. So, AFC proponents maintain. These claims might seem uncontentious, obvious even.

Still the above claims are empirical. AFC proponents therefore seem obliged to provide *empirical evidence* that supports their *empirical* claims. Schoubye (2012) claims that AFC proponents fail to fulfill this obligation. In place of actual empirical evidence, Schoubye contends that AFC proponents offer 'intuitions': gut feelings concerning frequency, subjective reflections about production, introspective reports regarding processing.[51] Furthermore, Schoubye argues that the empirical presumption that underwrites processing intuitions runs afoul of actual empirical investigations. In particular, Schoubye alleges that AFC proponents presume there to be a strong link between the immediacy and ease in processing some aspect of a linguistic element and the conventionality of the element in question. But, as Schoubye notes, psycholinguists sometimes observe similar processing in the presence of scalar implicatures and standardizations.[52]

Here, two interrelated challenges emerge. The first requires that AFC proponents go beyond mere intuitions when substantiating the empirical claims central to AFC. Let's call this the 'substantiation challenge'. The second requires that AFC proponents respect what empirical studies on frequency, production, and processing have discovered. Let's call this the 'respect challenge'. Fortunately, AFC proponents can meet each challenge. I begin with the respect challenge.

5.1 The respect challenge

Schoubye reads claims about the *immediacy* and *ease* by which hearers interpret referentially used definite descriptions as claims about the *speed* at which hearers process such usage. On this construal, AFC proponents commit themselves to the

---

[51] See Schoubye (2012: pp. 525, 522, fn. 8).

[52] See Schoubye (2012: pp. 525–527).

view that, all else equal, hearers process semantic information quicker than pragmatic information. And, as Schoubye notes, current processing studies do not seem to support such a view. This, of course, is unwelcomed news for AFC proponents *only if* their claims about immediacy and ease truly are claims about processing speed.

But they're not. Rather, AFC proponents use 'immediacy' and 'ease' quite narrowly. In particular, they use such language to characterize interpretations that do not involve pragmatic inferences of the Gricean variety. Here's Reimer:

> But in a linguistic community (such as our own) where such a use *was* standard, it is plausible that the intended meaning of an utterance of (1) [i.e. 'I was incensed'] would be grasped *immediately*: that is, **without the mediation of any Gricean-style inference**. …One could thus infer the intended 'singular' (non-literal) interpretation of an utterance of the form *The F is G* from its literal (quantificational) interpretation. But in a linguistic community (such as our own) where such use *was* standard, it is plausible to suppose that the intended meaning would be grasped *immediately*: that is, **without the mediation of any Gricean-style inference**. (Reimer 1998: pp. 98–99: *her emphasis*: **my emphasis**).

In a similar vein, Devitt notes that:

> The basis for RD [i.e. the thesis that English contains 'the$_R$'] is not simply that we can use a definite referentially, it is that we regularly do so. When a person has a thought with a particular **F** object in mind, there is a regularity of her using 'the F' to express that thought. And there need be *no special stage setting* enabling her to conversationally imply what she has not literally said, nor any sign that her audience needs to *use a Gricean derivation* to understand what she means. … 'Every' and other quantifiers are different. There is no convention of using them to convey a thought about a particular object in mind. *With special stage setting* they certainly can be used for that purpose, as Neale illustrates. …Thus it is plausible that Neale grasped 'Only Smith turned up' by a Gricean derivation, and that audiences grasp metaphors likewise. But people do not now grasp what speakers commonly mean by the verb 'incense' in that *Gricean way*. That is why the metaphor is really dead. And people do not grasp what a speaker means by a referentially used definite *in that way either*. Rather, they grasp the meaning *immediately* and *directly* because that is the meaning it conventionally has. (Devitt 2004: pp. 283–285: my emphasis).

Now, perhaps hearers can work through some inferential obstacle course as quickly as they implement a disambiguation procedure. But no AFC proponent needs to hold any position on such matters, though Devitt (2006) *does recognize* that, in certain cases, nonliteral language gets processed just as quickly as literal language. More to the point, processing studies will prove unhelpful in establishing whether hearers implement Gricean mechanisms or disambiguation procedures if those focus solely on speed *unless* researchers establish that hearers implement disambiguation procedures more quickly than they implement Gricean mechanisms. But, that seems unlikely in short term. So, here, as elsewhere, we must still rely largely on the observation of speech behavior. On these grounds, Devitt and Reimer's claims

about Gricean derivations seem plausible enough. After all, their Russellian opponents shy away from such long-winded derivations. Instead, Russellians gravitate towards pragmatic explanations that invoke 'streamlined' inferences or conventions of supplementation. In the former case, the inference involves *one* meagre step: hearers move from the form of 'the F' to its referential interpretation. In the latter case, hearers supplement 'the F' through the application of *one convention*, namely *Gödelian supplementation*.

5.2 The substantiation challenge

The substantiation challenge raises an important methodological question: what counts as *suitable* empirical evidence for the claim that a particular phrase regularly admits of two distinct uses? The *best* evidence, I think, would be a representative sample of phrasal usage that contains statistically frequent occurrences of two distinct uses. Let's call such evidence 'direct'. Unfortunately, the acquisition of direct evidence, at this moment, is a remote possibility. First, it's unclear what would constitute a representative sample of phrasal usage given the enormous heterogeneity of conversational contexts across both space and time. Second, many of the corpuses currently available involve specialized contexts: printed media, legal-political transcripts, the texts of controlled language acquisition studies, transcribed conversations that focus upon children in the midst of acquiring a first language, and the like. We must, then, set our sights somewhat lower.

Now, Schoubye discusses two possible data collection methods. First, if speakers regularly use a particular phrase in two different ways, then we should regularly find a divergence in truth conditions when speakers use a sentence that houses the phrase in question. Thus, frequent truth-conditional divergence might provide suitable empirical evidence for claims regarding regularized referential usage. Take a familiar case:

1. Banks often collapse.

On many occasions, a speaker's use of (1) will be true just in case a particular kind of financial institution is susceptible to collapse. On many other occasions, a speaker's use of (1) will be true just in case a particular support structure for rivers is susceptible to collapse. This divergence provides good empirical evidence that 'bank' is ambiguous in English. Second, if a phrase is regularly employed in two distinct ways, then we should regularly observe a divergence in the communicative intentions of speakers who employ the phrase. Thus, the observation of divergent communicative intentions might also serve as suitable empirical evidence for regularized referential usage. Consider (1) again. On many occasions, a speaker will use (1) with the intention to communicate to her audience that a particular kind of financial institution often goes broke. On many other occasions, a speaker will instead use (1) with the intention to communicate to her audience that a certain type of geological structure tends to wither away. This divergence also provides good evidence that 'bank' is ambiguous in English.

Notice, however, that these two methods seem to require that researchers can cobble together a representative sample of phrasal usage and then investigate the frequency of truth-conditional or intention-based divergences within the sample. That is, these

methods require direct evidence of regular usage. But, as we saw above, that seems too burdensome, especially since currently available corpuses do not include an inventory of contextual cues that would help to definitively discern the truth conditions of sentences or the communicative intentions of speakers. So, while Schoubye might be right to insist that AFC proponents cannot make evidential use of truth-conditional divergences[53] or intention-based divergences, the same will hold for theorists who wish to claim that 'bank' is regularly used in two distinct manners. But, surely, there's little objection to the claim that English speakers frequently use 'bank' in two distinct ways.

Still, if it's too demanding to require *direct evidence* for regularity claims, it seems too lax to allow such claims to rest upon mere intuitions. As it happens, AFC proponents can meet the substantiation challenge without fruitlessly searching for representative samples or unhappily settling for intuition mongering. Evidence drawn from careful observation, expert consensus, and lexicography may substantiate the AFC proponent's claims of regularized usage, indirect as each evidential form may be. After all, we native English speakers take 'bank' to regularly admit of two distinct uses because (a) we regularly *observe* such usage, because (b) linguists, philosophers, and logicians agree upon the regularity of such usage, and because (c) lexicographers provide both a financial and a geological entry for the phrase in question. The same, I think, largely holds for the referential use of the definite article.[54]

---

[53] Schoubye argues that AFC proponents cannot avail themselves to truth-conditional divergences since 'The$_R$ F is G' entails that there is one and only one **F** and every **F** is **G**, which, of course, makes 'The$_R$ F is G' truth-conditionally equivalent to 'The$_Q$ F is G'. Strictly speaking, however, the truth-conditional equivalence holds only if AFC proponents posit a uniqueness condition for both attributive and referential definite descriptions. Devitt (2004) rejects the uniqueness condition for referential definite descriptions. In place of a uniqueness condition, Devitt adopts a familiarity analysis of referential definite descriptions:

$$[\![\text{The}_R]\!] = \lambda \mathbf{f}.\lambda c.\exists x[\mathbf{f}x \,\&\, \mathbf{Familiar} - \mathbf{In}(x, c)].\iota x[\mathbf{f}x \,\&\, \mathbf{Familiar} - \mathbf{In}(x, c)]$$

Thus, on Devitt's view, 'the$_R$ F is G' does not entail that there is one and only one **F**. Rather, the sentence entails that there is at least one **F**. So, on Devitt's view, referential usage and attributive usage should give rise to observable truth-conditional divergences. Devitt believes such a divergence most clearly arises in the case of referential and attributive uses of *incomplete* definite descriptions.

[54] Some may wish to turn to corpus-based studies as an additional indirect source of evidence. Unfortunately, such studies are currently unhelpful. First, the studies presume that definite descriptions are primarily devices of (discourse) reference and thereby exhibit a 'referential' bias. Fraurud (1990), for example, treats definite descriptions in her corpus as either the 'first mention', the 'subsequent mention', or the 'isolated mention' of a *referent*. In a similar vein, Poesio and Vieira (1998) direct their annotators to classify definite descriptions in their corpus using a familiarity-based taxonomy derived from Hawkins (1978) and Prince (1981). Second, the familiarity-based taxonomies researchers impose upon their corpuses cross-cut Donnellan's referential–attributive distinction. Thus, while some researchers simply set the distinction aside, others reject the utility of such a distinction for natural language processing. Poesio and Vieira (1998: p. 191) fall into the former camp, Fraurud (1990: pp. 427–428) the latter. Third (and obviously), the studies tend to be unrepresentative, drawing upon the written usage of definite descriptions in various publications. However, since the researchers proceed as if reference-based accounts are the only game in town, the investigations might provide a hint of indirect empirical evidence for the claim that definites are frequently used referentially. After all, the annotators in Poesio and Vieira's study do not have a hard time classifying definite descriptions in purely referential terms.

5.2.1 Careful observation

Let's begin with careful observation. The AFC proponent gains support for her claim that referential usage is a regular occurrence from the diversity of the examples found in the immense literature concerning Donnellan's distinction. Here's Devitt:

> Critics of RD [i.e. the thesis that English contains 'the$_R$'] have not replied to [AFC]. How might they do so? (1) They might deny that definites are regularly used referentially. But *the range of examples* produced by Donnellan and others make this denial implausible. (2004: p. 283: my emphasis).

The literature and the data it houses are the results of decades of careful observations by English speakers about the operations of their own language. That should count as a form of *empirical* evidence for the commonality of referentially used definite descriptions.

In contrast, the literature on the referential use of uncontroversial quantifier phrases is a *proper* subset of the literature on Donnellan's distinction. The examples that emerge from it are primarily cases of indirect speech that rely on contrived circumstances. The examples, then, *indirectly* indicate that the referential use of uncontroversial quantifier phrases is an irregular phenomenon that lacks theoretical import. So, careful observation does help to empirically ground the AFC proponent's regularity claims.

In its own way, the history of the referential–attributive distinction lends the AFC proponent's regularity claims indirect credence. A noteworthy aspect of the history of the referential–attributive distinction is the distinction's *rediscovery* by contemporary theorists. Recanati (1989: fn. 1), for example, was able to locate a version of the referential–attributive distinction in the work of Antoine Arnauld, a seventeenth-century French philosopher.[55] Another notable feature of the distinction's history is its *parallel discovery* by contemporary theorists. As Neale (1990: p. xii) notes, prior to Donnellan (1966), Hampshire (1959), Marcus (1961), Geach (1962), Mitchell (1962) and Rundle (1965) had distinguished between referential and attributive uses of definite descriptions. This, I think, indicates that both uses occur with a frequency strong enough to encourage the distinction's resurfacing across time and language.

5.2.2 Expert consensus

A stronger case takes shape when we remember that while there is a venerable cross-linguistic tradition composed of experts—philosophers of language, linguists, and logicians—that treats definite descriptions as univocal devices of reference[56], there is no tradition, venerable or otherwise, that treats quantifiers such as 'every' and 'most' in a similar manner. This tradition suggests that the referential use of definites occurs *frequently enough* to encourage univocal referential accounts.[57] The lack of a similar tradition for 'every' suggests that the referential use of 'every' seems to escape the

---

[55] See Neale (1990) and Ostertag (1998: fn. 29) for further details.

[56] For instance, see Frege (1892/1997), Strawson (1950, 1964, 1971/2004), Stalnaker (1972), Barwise and Perry (1983), Heim and Kratzer (1998), and Elbourne (2005, 2013).

[57] As Heim and Kratzer note:

gaze of otherwise expert observers. Here, we obtain further substantiation that definite descriptions are regularly used referentially.

Notice too that prominent univocal Russellians concede that referential usage is not marginal. This concession is implicit in the Russellian–Gricean strategy of assimilating referential usage to GCIs or standardizations and not PCIs. Sometimes, however, the concessions are quite explicit. Consider Neale's remark that:

> Referential uses of descriptions are common, standard, regular, systematic, and cross-linguistic; indeed so much so that it would be a bit rich to deny that such uses are conventional, a direct function of linguistic meaning in a way that referential uses of other quantified DPs are not. I think Devitt and Reimer are right about this. (2004: p. 173).

Or Bach's remark that:

> In any case, argument I [AFC] does indeed challenge the analogy with particularized implicature. It exploits the fact that not only *can* we use definite descriptions referentially, we *regularly* do so (hence no need for special stage setting). This is not true of other sorts of quantifier phrases that can also be used to refer, but generally only in special circumstances. I agree with Devitt that this is an important difference. (2004: pp. 225–256: his emphasis)

> Devitt and I agree on a number of points: …Most definite descriptions that we use are incomplete (not uniquely satisfied). Most uses of incomplete definite descriptions are referential. (2007: p. 37)

These are *expert* observations that concede a point that AT theorists use to undermine the experts own univocal position! Surely, that must count as a form of empirical evidence.

5.2.3 Lexicographic support

Lexicography supports the AFC proponent's contention that speakers regularly use definite descriptions referentially. Dictionaries routinely provide a referential entry for the definite article. Lexicographers tend to cast the referential entry for 'the' in terms of the familiarity theory of definiteness.[58] Now, if there were no difference in the frequency between the referential use of definite descriptions and the referential use of 'every', then presumably lexicographers, who track usage, would accord each usage equal weight. However, referential entries for 'every' are conspicuously absent from the lexicographic record. The lack of referential entries for uncontroversial quantifiers

---

Footnote 57 continued

The basic *intuition* about phrases of the form 'the NP' is that they denote individuals, just like proper names. Had it not been for Bertrand Russell's famous claim to the contrary, few people would think otherwise. Frege, for one, thought it *obvious*: 'let us start, e.g., with the expression 'the capital of the German Empire'. This *obviously* takes the place of a proper name, and has as its reference an object.' (1998: p. 73: my emphasis).

[58] See Szabó (2000) for details.

is all the more conspicuous in light of the fact that lexicographers provide financial and geological entries for 'bank' as well as capacity and permissibility entries for 'can'.[59]

In short, the evidence for the regularity of referential usage largely mirrors the evidence for regularity claims quite generally: careful observation, expert consensus, and lexicographic support. The same cannot be said for the referential use of uncontroversial quantifier phrases. So, a case can be made for AFC's regularity claims. A rather good one, I think. AFC seems to survive this challenge too.

6 Indeterminacy

In its brief existence, AFC has faced two distinct threads of criticism. The first thread concerns overgeneration, the second empirical substantiation. I've shown that AFC proponents can provide reasonable replies to both criticisms. Assuming the absence of any further challenges, AFC does indeed seem to provide good reason to believe that the referential–attributive distinction has *truth-conditional import*.

Imagine that AFC faces no further objections. Therefore, imagine further that AFC *establishes* the truth-conditional significance of the referential–attributive distinction. Does AFC thereby *prove* that English contains 'the$_R$' and 'the$_Q$'? Does AFC entail AT?

Unfortunately not.

To see why, let's reconsider dead metaphors. As Devitt notes, dead metaphors may produce ambiguities. The process is rather straightforward. The phrase in question retains its previous literal meaning while also encoding the regularized metaphorical message as a second meaning. Thus, we move from univocal 'hardwired' to equivocal 'hardwired'. However, dead metaphors may also generate what Zacharska (2010) calls 'pragmatically polysemous' phrases. That is, a phrase might *accommodate* its regularized metaphorical message through a loosening of its previous literal meaning. The loosening serves to encompass *both* the metaphorical message and the previous literal meaning. Utilizing various contextual resources, speakers can then *pragmatically* narrow the phrase's expanded meaning in order to *express* the previous literal meaning or the previous metaphorical message.[60] Consider 'hardwired' once more. Recall again that 'hardwired' began life as a univocal term that encoded esoteric matters within electrical engineering. And again recount that English speakers began regularly using 'hardwired' metaphorically to discuss genetic endowment. Now, let's entertain the idea that 'hardwired' doesn't acquire a second meaning but rather loosens into *hardwired$_{<+\text{unalterable}>}$*. This loosening, in turn, allows speakers to *express* different propositions in different contexts through a pragmatic process that affects what's expressed. On this model, a speaker may use (1) to *express* either (2) or (3):

1. That's a hardwired$_{<+\text{unalterable}>}$ feature.

---

[59] The 'permissibility' entry is sometimes accompanied by a note claiming that such usage is improper. Speakers, we are told, should use 'may' in place of 'can' in such cases (*Oxford Pocket American Dictionary of Current English* 2002: p. 106). I think we *can* safely ignore such prescriptive remarks.

[60] Despite the different outcomes, the underlying result remains the same. The non-literal usage of a phrase, when regularized, often affects the semantic content of the phrase.

2. That's a hardwired$_{<+\mathrm{unalterable},\,+\mathrm{engineering}>}$ feature.
3. That's a hardwired$_{<+\mathrm{unalterable},\,+\mathrm{nativism}>}$ feature.

Here, we stumble upon a *semantically univocal phrase* that generates distinct truth-conditional contents in distinct contexts.

AFC, then, establishes AT only if we require separate articles to explain the truth-conditional import of the referential–attributive distinction. But, we need not invoke 'the$_R$' and 'the$_Q$' to account for Donnellan's distinction. There is now an alternate explanation. If we treat 'the' as pragmatically polysemous, as having a univocal meaning flexible enough to encompass attributive and referential usage, then we may account for the regularity of such usage as well as its truth-conditional import *without* positing a semantic ambiguity.[61] As it happens, Reimer (1998) does exactly that. Following Recanati (1989, 1993), Reimer provides a univocal account of the definite article. Within Reimer's framework, 'the' signals that the set its accompanying noun phrase denotes is a singleton. At the semantic level, the definite article is neither a device of reference nor a device of quantification. Instead, Reimer holds that sometimes context molds the definite article into a referential device while other times the definite article transforms into a quantifier.[62] On its own, then, AFC produces an

---

[61] See Zacharska (2010).

[62] As previously mentioned, a general skepticism surrounds pragmatic polysemy accounts of the referential–attributive distinction. The skepticism, I think, traces back to Neale's critique of Recanati's (1989) pragmatic ambiguity thesis. Neale (1990) claimed to be "baffled" (110) by Recanati's account, arguing that the pragmatic ambiguity thesis is "puzzling" (111) and "highly artificial" (112) as it holds speakers can use univocal phrases to express *distinct kinds* of propositions. With Recanati (1989: p. 233) and Bezuidenhout (1997: pp. 393–394), I think that this criticism begs the question against pragmatic ambiguity; it simply presumes that univocal phrases cannot contribute distinct *types* of content to what's expressed. I also find Recanati's proposal rather natural. To fix ideas, consider the following example. Recanati (1989: pp. 227–228) holds that sentences such as 'You will go home' are *unmarked* for or *neutral* with respect to illocutionary force. Following Recanati, we can imagine that the sentence contains a 'gap' where an element marking illocutionary force should reside. In a context, the 'gap' is 'plugged' with either an assertoric marker or imperative marker. The result: on some occasion of use, the sentence has assertoric force, on other occasions, the sentence has imperative force. Whatever the plausibility of this proposal, it is hardly puzzling.

For his part, Recanati (1989) claims that we should treat 'the' as univocally denoting $the_{<+unique,\pm REF>}$, where '*REF*' denotes the property of being type *referential*. For Recanati, referential terms such as proper names are *+REF* and quantificational terms such as 'every' are *–REF*. Since the definite article is neutrally marked with respect to *REF*, it will be contextually toggled to a fixed value. If context toggles $\pm REF$ to *+REF*, 'the' operates as a device of reference. If, instead, context toggles $\pm REF$ to *–REF*, 'the' functions as a quantifier. The result: definite descriptions will participate in different types of propositions depending on how context fixes the value of $\pm REF$. Again, whatever the plausibility of such an analysis, surely it is readily intelligible. (Neale (2004) is a bit more conciliatory on this point: "I took issue with [the pragmatic ambiguity thesis] in ch 3 of *Descriptions* (n. 36), and I am still inclined to think that, as stated, it is unacceptable; however, the desires that motivate it are highly instructive and I am inclined to think there is something valuable in it. Perhaps it just needs to be stated differently." (2004: p. 69, fn. 1).)

Now, Neale (1990) does claim that Recanati's account is implausible insofar as it seemingly reduces *genuine semantic ambiguity* to mere pragmatic polysemy. Recanati's account seems to allow one to hold that 'bank' contains a 'gap' that, in a context, may be 'plugged' with one of two markers: *+financial* or *+geological*. But surely if anything's semantically ambiguous, 'bank' is! Or take a familiar example. Just as AFC proponents and the enrichment Gödelian provide analyses that lend themselves to treating the permissibility reading of 'can' as a semantic phenomenon, so too do pragmatic polysemy analyses. For example, Recanati's framework suggests treating 'can' as neutral with respect to a permission marker (i.e.

indeterminate result. Perhaps the definite article is semantically ambiguous, perhaps it is pragmatically polysemous.

In fact, AFC's indeterminacy stretches beyond Devitt's ambiguity account and Reimer's polysemy account, incorporating Neale's (2004) unitary Russellian account as well. The enrichment Gödelian, remember, holds that a definite description is a univocal quantifier phrase that is regularly supplemented by a pragmatic convention that affects what's expressed, not what's implicated. As such, the enrichment Gödelian, like the polysemy theorist, can welcome the truth-conditional import of AFC without positing a lexical ambiguity in the definite article. As Neale (2004) puts it:

> …Rather than undermining the unitary Russellian analysis *per se*, [AFC] under-mines only the standard, wooden, Gricean explanation of referential usage (like the one I sketched in *Descriptions*), which amounts to no more than a generalized conversational implicature story. But the proposal on the table here is unruffled by [AFC]. (173, fn. 146 omitted).

This is the chief weakness of AFC. Many theorists, my former self (2008: p. 118) included, have heralded AFC as an *end* to the debate between AT theorists and Russellians. If AFC demonstrates that Donnellan's distinction has semantic import, then surely, so the thinking goes, English must contain two definite articles. But such a sentiment presumes that pragmatic mechanisms cannot impinge on what's expressed. This presumption is clearly controversial, as Sperber and Wilson (1986/1995) and Recanati (1993) have demonstrated.[63] More jarringly, the leap from truth-conditional import to ambiguity ignores the very conclusions that Reimer herself drew from AFC and the dead metaphor analogy that lends it support. In short, AFC *merely* shifts the debate. As Zacharska (2010) nicely summarizes:

> The question now is not whether the referential use is just as 'literal' as the attributive use, but, rather, whether the two uses amount to two distinct linguistic encodings of conventions (*semantic* ambiguity) or are, rather, two manifestations of a context-sensitive element of 'what is said' (*pragmatic* ambiguity). (2010: p. 62: her emphasis).[64]

7 Conclusions

For fifty years, philosophers and linguists have argued about just what to make of Donnellan's distinction. AFC refines the debate. We can now safely conclude that Donnellan's distinction affects truth conditions. As Devitt notes (2007), and as the discussions in Sects. 3 and 5 suggest, AFC provides *prima facie* evidence that a

---

Footnote 62 continued

'can' means $can_{<+possible,\pm permission>}$). Context toggles the neutral marker to either +*permission* or –*permission*. I think these overgeneration charges are correct. But, I don't think that these charges render Recanati's account implausible. Again, overgeneration is an occupational hazard of all accounts of the referential–attributive distinction. On this point, see Sect. 4.

[63] For details, see Sect. 4.3.2.

[64] For her part, Zacharska argues that the pragmatic polysemy account of the definite article trumps AT.

convention underwrites referential usage.[65] Moreover, as we saw in Sects. 4 and 5, neither overgeneration concerns nor evidential worries weaken the force of AFC. AFC, then, does alter the theoretical landscape: it's *very unlikely* that we can assimilate referential usage to what's *implicated*. Ultimately, however, this appears to be a meagre result. Given its indeterminate nature, AFC cannot settle the most basic question that Donnellan's distinction raises, namely whether the definite article is semantically ambiguous.

For a definitive answer to that question, we will have to look elsewhere. One hopeful place for AT theorists to search is within languages such as Malagasy and Mönchengladbach. As Amaral (2008) observes, these languages do contain two distinct definite articles, each of which appears to perform a particular semantic function that corresponds to one side of the referential–attributive distinction. And, as Amaral also notes, the definite articles of most natural language have yet to be subjected to any substantial investigation. It might be *here* that AT theorists find a definitive answer to the question at hand.[66]

References

Abate, F. (2002). *Oxford pocket American dictionary of current English*. Oxford: Oxford University Press.

Abbott, B. (2010). *Reference*. Oxford: Oxford University Press.

Amaral, F. (2008). Definite descriptions are ambiguous. *Analysis*, *68*, 288–297.

Amaral, F. (2010). *Referring and describing: Three essays on the meaning and use of definite descriptions and complex demonstratives*. Dissertation, CUNY Graduate Center.

Bach, K. (1981). Referential/attributive. *Synthese*, *49*, 219–244.

Bach, K. (1987). *Thought and reference*. Oxford: Clarendon Press.

Bach, K. (1995). Standardization vs. conventionalization. *Linguistics and Philosophy*, *18*, 677–686.

Bach, K. (2004). Descriptions: points of reference. In M. Reimer & A. Bezuidenhout (Eds.), *Descriptions and beyond* (pp. 189–229). Oxford: Oxford University Press.

Bach, K. (2007). Referentially used descriptions: A reply to Devitt. *European Journal of Analytic Philosophy*, *3*, 33–48.

Barwise, J., & Perry, J. (1983). *Situations and attitudes*. Cambridge, MA: MIT Press.

Bezuidenhout, A. (1997). Pragmatics, semantic underdetermination, and the referential/attributive distinction. *Mind*, *106*, 374–410.

Bontly, T. (2005). Conversational implicature and the referential use of descriptions. *Philosophical Studies*, *125*, 1–25.

Buchanan, R., & Ostertag, G. (2005). Has the problem of incompleteness rested on a mistake? *Mind*, *114*, 851–875.

Camp, E., & Reimer, M. (2006). Metaphor. In E. Lepore & B. Smith (Eds.), *The Oxford handbook of philosophy of language* (pp. 845–863). Oxford: Oxford University Press.

---

[65] Since AFC provides *prima facie* evidence for the conventionality of referential usage, Devitt (2007) concludes that AFC also places an *explanatory burden* on those who reject the conventional nature of such usage. AFC opponents are obliged to explain why referential usage has not migrated from implication to convention. As we saw in Sect. 4.3.3, Bach, a main opponent of AFC, fails to meet this burden. I take this failure to be symptomatic of accounts that deny the conventionality of referential usage. I would like to thank an anonymous reviewer at *Synthese* for bringing this issue to my attention.

[66] Devitt (2013b) himself takes Amaral's discussion of the definite articles of Malagasy and Monchengladbach to have "driven the final nail into the coffin of the received Russellian view" (108).

Davidson, D. (1978). What metaphors mean. In: *Inquires into truth and interpretation* (pp. 245–264). Oxford: Oxford UP.

Davies, M. (1981). *Meaning, quantification, necessity*. London: Routledge and Kegan Paul.

Devitt, M. (1974). Singular terms. *The Journal of Philosophy*, *71*, 183–205.

Devitt, M. (1997). Meanings and psychology: A response to Mark Richard. *Noûs*, *31*, 115–131.

Devitt, M. (2004). The case for referential descriptions. In M. Reimer & A. Bezuidenhout (Eds.), *Descriptions and beyond* (pp. 280–305). Oxford: Oxford University Press.

Devitt, M. (2006). *Ignorance of language*. Oxford: Oxford University Press.

Devitt, M. (2007). Referential descriptions and conversational implicatures. *European Journal of Analytic Philosophy*, *3*, 87–112.

Devitt, M. (2013a). Good and bad Bach. *Croatian Journal of Philosophy*, *13*, 169–200.

Devitt, M. (2013). What makes a property 'semantic'? In A. Capone, F. Lo Piparo, & M. Carapezza (Eds.), *Perspectives on pragmatics and philosophy* (pp. 97–112). Cham: Springer.

Donnellan, K. (1966). Reference and definite descriptions. *The Philosophical Review*, *75*, 281–304.

Elbourne, P. (2005). *Situations and individuals*. Cambridge, MA: MIT Press.

Elbourne, P. (2013). *Definite descriptions*. Oxford: Oxford University Press.

Evans, G. (1982). *The varieties of reference*. Oxford: Oxford University Press.

Fraurud, K. (1990). Definiteness and the processing of noun phrases in natural discourse. *Journal of Semantics*, *7*, 395–433.

Frege, G. (1892/1997). On sinn and bedeutung. In M. Beaney (Ed.), *The Frege reader* (pp. 151–171). Oxford: Blackwell.

Geach, P. (1962). *Reference and generality*. Ithaca: Cornell University Press.

Grice, P. (1969). Vacuous names. In G. Ostertag (Ed.), *Definite descriptions: A reader* (pp. 195–200). Cambridge, MA: MIT Press.

Grice, P. (1989). *Studies in the ways of words*. Cambridge, MA: Harvard University Press.

Hampshire, S. (1959). *Thought and action*. New York: Viking Press.

Hawkins, J. (1978). *Definiteness and indefiniteness: A study in reference and grammaticality prediction*. Atlantic Heights: Humanities Press.

Heim, I. (1988). *The semantics of definite and indefinite noun phrases*. New York: Garland Publishing.

Heim, I., & Kratzer, A. (1998). *Semantics in generative grammar*. Cambridge: Blackwell.

Horn, L. R. (1972). *On the semantic properties of logical operators in English*. Dissertation, UCLA.

Klein, E. (1980). Defensible descriptions. In F. Henry (Ed.), *Ambiguities in intensional contexts* (pp. 83–102). Dordrecht: D. Reidel.

Kripke, S. (1973/2013). *Reference and existence: The John Locke lectures*. Oxford: Oxford University Press.

Kripke, S. (1979). Speaker's reference and semantic reference. In G. Ostertag (Ed.), *Definite descriptions: A reader* (pp. 225–256). Cambridge, MA: MIT Press.

Larson, R., & Segal, G. (1995). *Knowledge of meaning: An introduction to semantic theory*. Cambridge, MA: MIT Press.

Lepore, E., & Ludwig, K. (2000). The semantics and pragmatics of complex demonstratives. *Mind*, *109*, 199–240.

Levinson, S. (1984). *Pragmatics*. Cambridge: Cambridge University Press.

Levinson, S. (2000). *Presumptive meaning*. Cambridge, MA: MIT Press.

Lewis, D. (1969). *Convention: A philosophical study*. Cambridge, MA: Harvard University Press.

Singh, M. (June 17, 2015). Why some teen brains may be hardwired to make risky choices. *NPR*. Available at: http://www.npr.org/sections/health-shots/2015/06/17/415165246/why-some-teen-brains-may-be-hardwired-to-make-risky-choices. Accessed 18 February 2016.

Marcus, R. (1961). Modalities and intensional languages. *Synthese*, *27*, 303–322.

Mitchell, D. (1962). *Introduction to logic*. London: Hutchison.

Neale, S. (1990). *Descriptions*. Cambridge, MA: MIT Press.

Neale, S. (2004). This, that, and the other. In M. Reimer & A. Bezuidenhout (Eds.), *Descriptions and beyond* (pp. 68–182). Oxford: Oxford University Press.

Neale, S. (2005). A century later. *Mind*, *114*, 809–871.

Ostertag, G. (1998). Introduction. In G. Ostertag (Ed.), *Definite descriptions: A reader* (pp. 1–34). Cambridge, MA: MIT Press.

Poesio, M., & Vieira, R. (1998). A corpus-based investigation of definite description use. *Computational Linguistics*, *24*, 183–216.

Prince, E. (1981). Towards a taxonomy of given-new information. In P. Cole (Ed.), *Radical pragmatics* (pp. 223–256). New York: Academic Press.

Pupa, F. (2008). *Ambiguous articles: An essay on the theory of descriptions*. Dissertation, CUNY Graduate Center.

Recanati, F. (1989). Referential/attributive: A contextualist proposal. *Philosophical Studies*, *56*, 217–249.

Recanati, F. (1993). *Direct reference: From language to thought*. Cambridge: Blackwell Publishers.

Reimer, M. (1995). Performative utterances: A reply to Bach and Harnish. *Linguistics and Philosophy*, *18*, 655–675.

Reimer, M. (1998). Donnellan's distinction, Kripke's test. *Analysis*, *52*, 89–100.

Rundle, B. (1965). Modality and quantification. In R. J. Butler (Ed.), *Analytical philosophy, second series* (pp. 27–39). Oxford: Blackwell Publishers.

Russell, B. (1905). On denoting. *Mind*, *14*, 479–493.

Saddock, J. (1978). On testing for conversational implicature. In S. Davis (Ed.), *Pragmatics: A reader* (pp. 365–376). Oxford: Oxford University Press.

Salmon, N. (1982). Assertion and incomplete definite descriptions. *Philosophical Studies*, *42*, 37–45.

Schiffer, S. (1972). *Meaning*. Oxford: Clarendon Press.

Schoubye, A. (2012). Against the argument from convention. *Linguistics and Philosophy*, *35*, 515–532.

Soames, S. (1986). Incomplete definite descriptions. In G. Ostertag (Ed.), *Definite descriptions: A reader* (pp. 275–308). Cambridge, MA: MIT Press.

Sperber, D., & Wilson, D. (1986/1995). *Relevance, communication, and cognition* (2nd ed.). Oxford: Blackwell Publishers.

Stalnaker, R. (1972). Pragmatics. In D. Davidson & G. Harman (Eds.), *Semantics of natural language* (pp. 380–397). Dordrecht: D. Reidel.

Strawson, P. (1950). On referring. *Mind*, *59*, 320–344.

Strawson, P. (1964). Identifying reference and truth-values. *Theoria*, *30*, 96–118.

Strawson, P. (1971/2004). Introduction. In *Logico-linguistic papers* (2nd ed., pp. xi–xvi). Aldershot: Ashgate Publishing Limited.

Szabó, Z. (2000). Descriptions and uniqueness. *Philosophical Studies*, *101*, 29–57.

Wilson, G. (1978). On definite and indefinite descriptions. *The Philosophical Review*, *87*, 48–76.

Wilson, G. (1991). Reference and pronominal descriptions. *The Journal of Philosophy*, *88*, 359–387.

Zacharska, B. (2010). Definite descriptions: Semantic or pragmatic polysemy? *University College London Working Papers in Linguistics*, *22*, 56–63.