Universals in semantics[1]

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Abstract

This article surveys the state of the art in the field of semantic universals. We examine potential semantic universals in three areas: (i) the lexicon, (ii) semantic "glue" (functional morphemes and composition principles), and (iii) pragmatics. At the level of the lexicon, we find remarkably few convincing semantic universals. At the level of functional morphemes and composition principles, we discuss a number of promising constraints, most of which require further empirical testing and/or refinement. In the realm of pragmatics, we predict that Gricean mechanisms are universal, but suggest that the precise nature of presuppositions may be subject to cross-linguistic variation. Finally, we follow E.L. Keenan in concluding that the overarching universal of effability or translatability between languages cannot be upheld in its strongest form. A recurring theme throughout this survey is how much work still remains to be done in the relatively young field of cross-linguistic formal semantics.

1. Introduction

Semantics is concerned with the way natural languages express meanings. Meanings of complex phrases and sentences arise compositionally from the meanings of their parts (down to the smallest meaning-bearing elements: morphemes). The compositional derivation of meanings depends systematically on the syntactic structure of the complex expressions. Further, once an expression

is actually used in an actual context, pragmatic mechanisms lead to further enrichment and modification of the grammatically composed meanings. So, when we ask what in the realm of meaning is universal and what is language-particular, we need to look at three areas:

(i) the inventory of lexical/content morphemes;
(ii) the mechanisms that compose meanings:
     a. the inventory of functional "glue" morphemes,
     b. the inventory of composition principles;
(iii) the mechanisms of pragmatics.

This article is organized precisely along those lines: we will ask about each of these three components what some samples of proposed universals in that area might be.

One can easily find statements such as this one: "In contrast to phonological and syntactic universals, very little attention has been paid to the study of semantic universals" (Mairal and Gil 2006: ix) and, in the same volume, "Most of the work on universals of human languages has been concentrated on the phonological, morphological, and syntactic properties of languages, with much less attention being devoted to the semantic side of language" (van Valin 2006: 155). We believe that the reasons for this comparative dearth of work on semantic universals are mostly mundane: semantics in a theoretical and formal vein is a particularly young and understaffed discipline, which has only quite recently started to seriously look at cross-linguistic variation and uniformity.[2] We are unaware of any surveys on universals in semantics, a gap which we hope to start filling here.

Before we delve into the composition of meaning, we will address some overarching issues.

1.1.   Sapir/Whorf and linguistic relativity

Common culture (what one might call "folk linguistics" or "folk anthropology") frequently assumes that languages not only differ widely in their seman-

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2. Serious questions of semantics were considered pretty much intractable in early formal linguistics and their treatment was to be delayed until there was a suitable analytical framework in place, which was not expected to happen any time soon, or ever. It wasn't until semantic methods from formal logic began to be applied to natural language in the 1960s that the discipline of formal semantics coalesced. See Partee (2005) for a personal perspective on this history. Perhaps not unexpectedly, the new methods were first applied to well-studied languages such as English and German. Pioneering contributions to cross-linguistic semantics are the 1995 volume on cross-linguistic quantification (Bach et al. 1995b) arising out of an NSF-funded collaborative research project, Carlota Smith's work on aspect (Smith 1997), and Maria Bittner's work (see for example Bittner 1994). The developments since then are thankfully too numerous to list here.

tics but that these differences are correlated with deep differences in the "world view" of the speakers of different languages.[3]

Languages do look quite different from each other on the surface, which makes the leap from noticing that superficial variety to presupposing an *underlying* variety, even at the level of meanings, rather tempting. This mentality is nicely characterized by Bloom and Keil (2001: 364–365):

> [O]n a subjective level, languages are extremely different from one another. A monolingual speaker of English, for instance, will hear Chinese or Turkish as gibberish – as odd and unfamiliar noise. The phenomenally alien nature of other languages might lead to the impression that there must be profound differences at deeper cognitive levels as well. English and other languages seem so massively different; surely those differences must lead to commensurate differences in other areas of cognition. This impression is magnified by cultural differences that so often correlate with linguistic differences.

Reinforcing the leap from superficial variety to presupposing underlying incommensurability may be a psychobiological tendency to assume that *other* people and cultures, since they are not like *us*, must be fundamentally different, not just superficially so. The denial of human universals, unsurprisingly, has a long intellectual history (see Brown (1991) for crucial discussion).

Infamously, "Eskimo" speakers are supposed to have at their disposal many different words for snow, which is taken to *reflect* the fact the "solid phase of water" (Poser 2004) is of paramount importance for their culture.[4,5] Other times, the direction of causation might be said to go in the other direction; deep distinctions in the grammar of a language might *influence* the way speakers of that language look at and think about the world – an idea that is often called the Sapir/Whorf hypothesis, or, less tied to those particular scholars, the hypothesis of Linguistic Relativity (the term given to the idea by Whorf himself).

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3. A random example from the letters to the editor page of the *Boston Globe* (June 17, 2007), written by an American of Hmong descent, about the ravages of the Indo-China wars on the Hmong homeland:

    > My people are as nonpolitical as they come. We did not even have a word for "war" until the secret war came to our villages. The concept of two enemies so ravenous for land that they would destroy the earth to claim a space was so remote, so strange, that to this day, our term for "war" exists only as a metaphor. We call it the "time of tigers wrestling".

4. Of course, the empirical facts are not as clear-cut as the myth has it and the fact that the myth lives on without any significant grounding in empirical facts is puzzling and disturbing. See Martin (1986) and Pullum (1989) for discussion.

5. In work we discuss below, Everett (2005) argues that certain properties of Pirahã culture lead to a lack of certain linguistic features in the Pirahã language. As a reviewer pointed out to us, this is a twist on the Eskimo story in that here culture isn't used to explain a particular kind of abundance in the language, but a surprising sparseness.

If Linguistic Relativity is correct, linguists in search of semantic universals may be doomed to failure, tilting against windmills. We do not however believe that the thesis is correct to a degree that would make cross-linguistic semantics impossible. We concur with Bloom and Keil when they say

> We think the intuition here is wrong in two ways: Languages do not really differ as much as people think they do. Our "folk linguistics" is wrong in this regard. And correlation is not causation; the fact that people who speak different languages tend to belong to different cultures does not entail that language has a profound influence on thought. So although there is a strong impression that the language one speaks must influence how one thinks, we think that this impression is more seductive than it is instructive. (Bloom and Keil 2001: 365)

As we will outline below, the truth as usual is probably somewhere in the middle and only extensive research will establish how much of Linguistic Relativity is correct.[6]

1.2. *Effability and translatability*

> *There are many ways in which the thing I am*
> *trying in vain to say may be tried in vain to*
> *be said.*   Samuel Beckett

At the other end of the spectrum is the strong effability thesis proposed by Katz (1976: 37):

(1)   Strong Effability Hypothesis
          Every proposition is the sense of some sentence in each natural language.

Effability, if true, would be the most basic semantic universal. It would assert that all natural languages have the same expressive power and that furthermore, that expressive power is complete in the sense of being able to express any proposition whatsoever. Note that Katz (1976: 36) further suggests that effability – rather than anything syntactic such as recursion or constituent structure – is the unique feature which distinguishes human languages from animal communication systems.

In principle, we would like to concentrate on a weaker thesis which leaves it open whether there are propositions that cannot be expressed in any language, either because they can't even be *thought* or *grasped* by humans or because

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6. For some of the recent research into Linguistic Relativity, see Gentner and Goldin-Meadow (2003); Gumperz and Levinson (1996); Li and Gleitman (2002); Masharov and Fischer (2006), and many others.

they cannot be captured in natural language. What we would like to consider is therefore the Translatability Thesis:

(2)   For any pair of natural languages and for any sentence *S* in one and any sense σ of *S*, there is at least one sentence *S′* in the other language such that σ is a sense of *S′*. (Katz 1976: 39)

In other words, any meaning that can be expressed in any given language can also be expressed in any other language. In practice, we have found that the distinction between the full effability thesis and the weaker translatibility thesis is often not made.

The effability idea has been around since at least Sapir (1949: 153–155):

> The outstanding fact about any language is its formal completeness ... No matter what any speaker of it may desire to communicate, the language is prepared to do his work ... Formal completeness has nothing to do with the richness or the poverty of the vocabulary ... The unsophisticated natives, having no occasion to speculate on the nature of causation, have probably no word that adequately translates our philosophic term "causation," but this shortcoming is purely and simply a matter of vocabulary and of no interest whatever from the standpoint of linguistic form ... As a matter of fact, the causative relation ... is expressed only fragmentarily in our modern European languages ... [but] in Nootka ... there is no verb or verb form which has not its precise causative counterpart.

Effability is also widely assumed by modern semanticists; for example, van Benthem (1991: 25) suggests that all languages are "expressive", meaning that "every useful meaning can be verbalized."[7]

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7. There are weaker versions of effability that would run the risk of allowing non-universality, such as when Li and Gleitman (2002: 291) write:

> All languages have the formal and expressive power to communicate the ideas, beliefs, and desires of their users. From this vast range of possibilities, human communities select what they want to say and how they want to say it. This stance is at its core the same one that explains why the Elizabethans habitually used terms for falconry and we do not, and why English speaking vacationers at Aspen and Vail find it natural to develop terms like sugar, powder, and granule to amplify their heretofore impoverished means for discussing the state of the snow on the slopes. In the end, it's the thought that counts.

In other words, effability – while a universal property of natural languages – might be formulated in a language-relative way: each language provides the expressive power needed by its speakers, which allows the possibility that different language communities have different needs and thus different languages have different sets of meanings that they can express. We may detect the same weak thesis in the quote from Sapir in the text above. NB: Again, the solid phase of water is mentioned, albeit not referring to Eskimos but to Rocky Mountain skiers.

(2) is empirically testable: if there is any proposition that can be expressed in one language but not in another, translatability – and therefore also effability – is false (Katz 1976: 39). One potential challenge to (2) discussed by Katz is Quine's example of a putative "jungle language" into which one would not be able to translate the English sentence *neutrinos lack mass* (Quine 1960: 76). (Note that Quine does not provide any evidence that there is a language into which one cannot translate *neutrinos lack mass*; he merely claims that if a speaker of the putative jungle language were asked for the translation, "we would expect him to coin words or distort the usage of old ones" (Quine 1960: 76, cited in Katz 1976: 41).) Katz's response to this example is that the failure of translation would result merely from a temporary vocabulary gap, rather than a fundamental deficiency of the language. Indeed, Quine's imagined result of the translation attempt supports this idea, since effability merely requires translatability – it makes no claims about the naturalness of the translation or the number of coinages which might be required to achieve it.

A real-life jungle experiment is that of Everett (2005), who argues that Pirahã (Muran) lacks (among other things) numerals or a concept of counting, quantifiers, and all color terms. However, even if Pirahã lacks all these elements – and see Nevins et al. (2007) for a reply to Everett–, it does not necessarily follow that the same concepts cannot be expressed in Pirahã as in English. For example, to illustrate that Pirahã lacks a word corresponding to *most*, Everett offers a Pirahã sentence which he translates as "We ate most of the fish" and glosses literally as "My bigness ate [at] a bigness of fish, nevertheless there was a smallness we did not eat" (Everett 2005: 624). It is not obvious that the same proposition is not being expressed here, and it is at least possible that translatability obtains in spite of all the proposed gaps in the Pirahã lexicon and syntax.

It should be clear at this point that cross-linguistic uniformity of meaning cannot be found at any kind of structural level (logical form), since what corresponds to a quantificational determiner like *most* in one language might be a rather complex expression in another language. So, it's crucial that by "proposition" we mean not any kind of representation that reflects the syntactic structure of the expression used to convey the proposition; instead, we are assuming a purely denotational view of proposition as in possible worlds semantics. The theme of "what language X expresses simply is also expressible in language Y but at the price of some complexity" will recur throughout this article.

So far we have taken the view that necessary coinage of new vocabulary items and possibly complex rephrasing are not principled problems for the thesis of translatability. But there are other problems that do raise the possibility that languages sometimes cannot quite convey the same meanings, at least when one considers subtle aspects of meaning.

Challenges to translatability are of course legion in the theory and practice of actual translators; Bar-On (1993) has some illuminating discussion and examples. Let us grab one such example from the cabinet of semantic curiosities.[8] Burushaski, a language spoken in Pakistan, has two relational nouns to denote siblings, much like English *sister* and *brother*, except that the morpheme *cho* means "sibling of the same gender" (as the internal argument of the nominal) and *yas* means "sibling of the opposite gender" (from that of the internal argument). So, a male speaker would call his brother *a-cho* "my same sex sibling" and his sister *a-yas* "my opposite sex sibling", while a female speaker would use *a-cho* to refer to her sister and *a-yas* to her brother. Now, whether John calls Peter *my brother* or *my same-sex sibling* doesn't seem to make a difference at the level of denotational semantics. But as soon as we consider situations where the sex of the speaker is uncertain, the two phrases give rise to different propositions. As Yancey (2000: 10) puts it: "a Burushaski text in which the gender of the speaker has purposefully not been mentioned until the end, at which point the reader discovers that the speaker and her *a-cho* are both female, would not be readily translatable into languages which would force a gender specification. In English one could say *sibling*, but this would most likely tip off the reader to the surprise at the end".

A different type of challenge to effability is that of E.L. Keenan (1974), who explicitly argues that not all languages are equivalent in expressive power. Keenan argues that if two sentences have the same meaning – if they are translations of each other – then they must make the same assertions and the same presuppositions (E.L. Keenan 1974: 193). He then argues that languages "differ systematically with respect to presupposition structures" (194). Keenan's examples concern differences in relativization possibilities between Hebrew and English. He compares (E.L. Keenan 1974: 195) the Hebrew sentence in (3) with its ungrammatical English equivalent in (4a), and an alternative sentence which is not an exact translation of it in (4b); (4b) does not share the same presuppositions as (3).[9]

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8. See Bar-On (1993), Catford (1965), and Yancey (2000).
9. In a reply to Keenan's argument, Katz (1976) offers the following translations of (3) which are meant to preserve its presuppositions:

   (i)   a.   This is the woman such that she was given a book by the man I know.
         b.   This is the woman who received a book from the man I know.
         c.   This is the woman who was given a book by the man I know.

   We actually don't think Katz's translations circumvent Keenan's point: Keenan's example presupposes that there is a (unique) man that gave the book to the woman. Katz's examples have a different presupposition: that I know a (unique) man. Further, as a reviewer pointed out, Keenan's example presupposes a unique book, while Katz's don't; note, however, that this would be easy to remedy by just using *the book* instead of *a book*.

(3)   *Zot   ha– isha   she– ani makir   et ha– ish   she–*
      That is the woman   that I   know the   man   that
      natan la   et   ha- sefer
      gave   to her the book

(4)   a. *#This is the woman that I know the man that gave a book.*
      b. *I know a man that gave some woman a book and this is that
          woman.*

Keenan has raised a serious challenge to translatability. If translations are required to preserve presupposition/assertion structure, it is not obvious that translatability can be upheld. For example, if, as is often assumed, pronominal features involve presuppositions (Heim and Kratzer 1998, though see Kratzer 2006 for a different view), languages which encode different distinctions in their pronoun systems may not be able to express the same propositions while keeping all presuppositions constant. We will even discuss below a challenge to the idea that English-style presuppositions are present in every language (see Section 4.1).[10]

One could imagine a weakened version of translatability in which one allows differences in presuppositions (and implicatures, expressive content, etc.), but maintains that at the level of core truth-conditional content, what one language can express any other can as well. We suspect that this is a position that is quite widespread among linguists, and it seems like a reasonable stance to us as well. But we don't know whether this position can be given a precise formulation that would make it more than a warm and fuzzy feeling. In any case, even if we assume such a universal effability/translatability claim to the effect that at the level of core truth-conditions, any meaning expressible in any language is also expressible in all other languages, we need to be aware of the limits to translatability: (i) aspects of meaning like presupposition and expressive meanings, where languages may in fact differ in effability, and (ii) a suspicion that the grammars of particular languages highlight different aspects of reality in ways that might influence certain aspects of the world view of speakers. Bach (2005) cites a pertinent remark by Roman Jakobson: "... the true difference between languages is not in what may or may not be expressed but in what must or must not be conveyed by the speakers" (Jakobson 1959). We will see that this is indeed pretty much what one finds.

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10. Another likely source of counter-examples to full translatability comes from *expressive meaning*, as discussed by Potts (see most recently Potts 2007). Potts argues that the meaning of expressive items like *bastard* is "descriptively ineffable": "Speakers are never fully satisfied when they paraphrase expressive content using descriptive, i.e., nonexpressive, terms" (Potts 2007: 166). We might then conjecture that across languages, expressive content will be particularly hard to match up. Thanks to Eric McCready (p.c.) for discussion of this point.

Even if we anticipate finding a checkered result on universality and variation in semantics, we think that sound methodology in semantic work on any given feature of grammar has to start from a null hypothesis of universality and proceed to rigorous testing of that hypothesis by looking at that feature in as many diverse languages as possible. This method is explained further by Matthewson (2007a). Note that this methodology is not the same as assuming that all languages work like English. Matthewson (2001), for example, has tried to show that the way quantification works in St'át'imcets (Lillooet Salish) may well be the guide to the proper treatment of quantification in English (arguing against long-held anglocentric assumptions about the structure of quantified noun phrases). Similarly, Bittner proposes a universal system of temporal anaphora which "instead of attempting to extend an English-based theory to a typologically distant language [. . .] proceeds in the opposite direction – extending a Kalaallisut-based theory to English" (Bittner 2007a: 36).

1.3.   Sources of universality and variation

In what follows, we will rarely comment on possible sources of a claimed universal. We believe that the state of the art in semantic universals is largely too immature to allow explorations of their sources. But perhaps, a few words on this topic are in order. We assume that the part of the human genetic endowment that has any relevance to semantics is constant throughout the species.[11] Any differences in the semantics of different languages would therefore have to be traced back to accidents of history, environment, and culture. How much in the way of semantic universals we expect to find then correlates with our expectations about how strongly the genetic component, the shared physical environment, the shared biology, shared cultural traits constrain the structure of individual languages. There can be widely varying positions on this question. As we said, methodologically we recommend that universality be the null hypothesis, only rejected case by case after extensive cross-linguistic checking.

Once a universal has been discovered and has held up to cross-linguistic scrutiny, the question arises as to its source. Is the feature universal because it is genetically hardwired or because languages couldn't fulfill their function

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11. While this has been the working assumption of work in all subfields of generative linguistics, it is not entirely implausible that it is wrong in its strongest sense. See for example the recent work by Dediu and Ladd (2007) raising the possibility that there is a genetic correlation with whether a language has tones or not.

otherwise?[12] Giving an answer to this question for a particular universal is not easy, and we will refrain from speculations on this matter in this article. We agree that, in principle, the following typical course of argumentation is reasonable: if for a particular universally attested feature under investigation there is no plausible functional explanation, the feature can reasonably be assumed to be part of the genetically hardwired Universal Grammar (UG).[13] But we do not think that at this point, we have sufficient material to even consider possible functional explanations for given semantic universals.

2. Lexical universals

As advertised in the beginning, we will organize our survey of universals in semantics around the major components of complex meanings. First up is the lexicon of content morphemes (in distinction to functional/grammatical morphemes, which will be discussed in Section 3.1). Content morphemes are the predicates (nouns, verbs, adjectives, at least) that help language talk about the world. What universals do we find in the content lexicon?

The lay person, of course, expects major variation. We already mentioned the Great Eskimo Vocabulary Hoax. The idea that different languages have differential access to different parts of reality truly is a widespread meme. The schema "language *X* has no word for *Y*" holds endless fascination for many people.

In the same vein, perhaps everyone remembers from their first linguistics course the claim that languages put arbitrary labels on reality and that they can differ quite a bit on how they do that. One textbook example is from the Danish structural linguist Hjelmslev (1943: 50, English added), as depicted in Figure 1.

So, is there any hope for universals in the content lexicon? There are two questions we'll address: (i) are there meanings which are universally expressed as lexical items? (ii) for the remaining meanings (the ones that are not uni-

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12. We assume that in the absence of genetic uniformity and uniform functional demands, languages would have innovated variations on the feature, so that simple shared ancestry is not usually a plausible explanation.

13. Note that the converse situation is a bit more complicated: the existence of a plausible functional explanation would not be a knock-down argument that the feature is not hardwired: hardwiring it may have functional advantages (faster learning, for example) on top of the functional need to have the feature in the first place. (As Harry van der Hulst (p.c.) points out to us, this connects to the so-called "Baldwin Effect" in evolutionary theory.) On the other hand, we currently lack any kind of experimental methodology to find out whether there are such features of UG. As a methodological principle, in the absence of independent experimental evidence, it is reasonable to not assume an innateness source if a plausible functional explanation has been found.

English	German	Danish	French
tree	Baum	træ	arbre
wood	Holz		bois
		skov	
woods	Wald		
forest			forêt

Figure 1: Hjelmslev's depiction of the tree/wood/woods semantic field

versally attested), are there constraints on what can be expressed as a lexical item?

2.1.   Universal lexical meanings?

There are several lists of proposed universally attested lexical items, for example:

○ Swadesh lists, prepared not as claims for universal lexical status, but as reliable tools for wide-scale lexico-statistical and glotto-chronological investigations (see for example Swadesh 1952);

○ from a textbook (Immler 1974: 41, quoted from Immler 1991: 39): rustle, soil, [many animals], [many plants], [parts of the body], sleep, big, small, heavy, light, fast, slow, sick, talk, call, ask, believe, decide, birth, wave, up, down, hunger, life, death, danger, fear, want/will, power/authority, be allowed, be obliged, mother, man, woman, caress, high, deep, warm, cold, air, water, rain/snow, wind, sun, pain, pleasure, we, they, group, drink, shelter, make love;

○ the list of "semantic primes" proposed by Wierzbicka (1996) and other researchers working in the Natural Semantic Meta-Language (NSM) approach.

Immler claims about such lists "we are immediately convinced of the validity of these universals, not only so: we are sure of them – and this without having verified them by empirically looking at all the languages of the world" (Immler 1991: 39).

We cannot share Immler's confidence. On the contrary, many of the words in these lists are probably not universal. First, as argued by Goddard (2001), a claim about a universal lexical item is interesting only insofar as the correspondences in meaning of that lexical item across languages are reasonably precise. Goddard notes, for instance, that the claim that all languages have words for "black" and "white" (Berlin and Kay 1969) is only approximately true, since

in languages with only those two color terms, the terms do not mean the same thing as they do in English.[14]

If we adopt the criterion of reasonably strict meaning correspondence, we can falsify several of the proposed universal items on Immler's list using St'á-t'imcets. There is no single word for "cold" in St'át'imcets, as illustrated in (5).[15] (The two forms in (5a) are dialectal variants.)

(5)   a.   *lháxil / ts'úlhum*   "cold (a person's feeling)"
       b.   *xelh*   "cold (weather, the air)"
       c.   *ts'ip'*   "cold (an object, to the touch)"

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14. For those of us who are still obsessed with water, note the following passage from Goddard (2001: 20):

> Surprising as it may seem to English speakers, 'water' is probably not a universal lexical unit. Japanese has two words (*mizu* and *yu*) for 'water', with *yu* (often with an honorific prefix *o–*) being reserved for hot water (Suzuki 1978: 51–52). *Mizu* cannot be used about hot water. Furthermore, combining the adjective *atsui* 'hot' with *mizu* sounds unnatural – Suzuki calls it "self-contradictory" – though there is no such restriction in relation to other liquids, e.g., *atsui miruku* 'hot milk' (cf. Wierzbicka 1996: 229). These facts imply that *mizu* and *yu* both have a reference to temperature built into their meanings.

We actually suspect that there is a possible pragmatic explanation in which *yu* means 'hot water' while *mizu* means just 'water' but because of the available option of *yu* implicates 'cold water'. Eric McCready (p.c.) tested the following dialogue for us on several native informants:

(i)   A: *Koko   mizu   deru?*
             here   water   come.out
             'Can you get water out of here (e.g. faucet)?'
       B: *Un,   oyu   dake   dakedo   ne*
             yeah   hot.water   only   though   PT
             'Yeah, but only hot water.'

This dialogue is perfectly natural, which indicates that *mizu* can in fact be used as a neutral term covering liquid water of all temperatures and that it conveys coldness only through an implicature in many occurrences.

A reviewer urged us to comment on the relevance of cases like these for the translatability thesis: on the one hand, we are suggesting that Japanese *mizu* means 'water', on the other hand, we are saying that because of the availability of the item *yu* ('hot water'), *mizu* implicates coldness, something that English *water* doesn't do. So, is there inter-translatability between *mizu* and *water*? Clearly not in a strict sense, and this percolates up to the propositional level, which is where we argued earlier any reasonable translatability thesis would have to be located. The English sentence *There was water in the cup* has no precise Japanese counterpart as soon as we take implicatures into consideration: more likely than not, the Japanese sentence would either assert with *yu* that there was hot water in the cup or implicate with *mizu* that there was cold water in the cup. We consider the issue of cross-linguistic differences in implicature further in Section 4.2.

15. Throughout this article, St'át'imcets data are presented in the official orthography of the language, developed by Jan van Eijk; see van Eijk and Williams (1981).

Similarly, in St'át'imcets, there is no word for "group", nor for "decide", nor for "pleasure".

Perhaps the most careful studies of possible lexical universals have been conducted by proponents of NSM; for an overview see Goddard's article (2001). Here's the (short) list of items that survived Goddard's scrutiny:[16]

(6)   man, woman, child, mother, head, eye, ear, nose, hand, day, kill, make, people, good, bad, big, small, think, know, want, see, hear, say, do, happen, live, die, here, above, below, inside, a long time

There are some others on his list but those are functional items and we will deal with universals in that domain in Section 3.1.

While it appears that some small list of lexical items might survive close scrutiny, we do not think that there is much of interest here: languages do differ almost without limit as to which meanings they choose to lexicalize.[17] As Levinson (2003: 32) puts it, "[t]here are vanishingly few universal notions, if any, that every language denotes with a simple expression".[18]

2.2.   *Constraints on the lexicon?*

If languages differ so wildly in how they lexicalize even arguably universal domains of meaning (weather, personal relationships, etc.), are there *any* constraints on what lexical items there can be in a natural language? The most radical claim would be that languages are indeed completely constrained: they can only choose to lexicalize concepts that are part of an innate repository of possible concepts, even concepts like *doorknob*, *carburetor*, *bureaucrat*, etc. This claim is primarily associated with Jerry Fodor (see Laurence and Margolis 2002 for a critical discussion; see also Chomsky 1997: 29). Most claims for universal constraints on the lexicon are, however, considerably more tame. We will discuss first some universals about the make-up of the lexicon. Apart from universals about the inventory of lexical items, there are other possible universals about the lexicon. We will ask the following questions: Are there universal semantic determinants of the category membership of predicates (2.2.2)? We

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16. We do not actually agree with some of Goddard's discussion but this is not the place to lay out our worries about his methodology.
17. We should note that we do not at all endorse the NSM claim that there are primitive semantic elements from which all others are created by combination. See the replies by Barker (2003), Geurts (2003), and Matthewson (2003) to a target by Durst (2003).
18. Note that Levinson (2003: 35) claims (without referring to specific evidence) that "not all languages have a word (or other expression) for 'red' or 'father' or 'in' or 'come' or even 'if'." The claim that *if* is not universal is contrary to what Goddard (2001) concluded, so there clearly is work to be done to figure out whether *any* lexical items are universal.

will also discuss the potential universality of the Vendler classes (2.2.3). And are there constraints on the semantic type of predicates (2.2.4)?

2.2.1. Constraints on possible lexical items.   Of the 142 semantic universals listed in the Universals Archive at the Universität Konstanz (http://typo. uni-konstanz.de/archive/intro/), most are some kind of constraint on the lexicon.[19] Here is a small sampling of proposed lexical universals.

(7)   The color term hierarchy (Berlin and Kay 1969)

All languages contain terms for white and black, and there is an implicational hierarchy such that if a language possesses a term in the hierarchy, it also possesses all terms to the left of it:

$$\text{white \& black} < \text{red} < \left\{ \begin{array}{l} \text{green} \\ \text{yellow} \end{array} \right\} < \text{blue} < \text{brown} < \left\{ \begin{array}{l} \text{purple} \\ \text{pink} \\ \text{orange} \\ \text{grey} \end{array} \right\}$$

(8)   If a language has adjectives for shape, it has adjectives for color and size (Dixon 1977).

See Levinson (2001) for arguments against the color term hierarchy and against all conclusions about universality which can be drawn from it. Levinson claims that it is not even true that "all languages have terms which exhaustively partition and describe the perceptible color space" (2001: 8). See Kay (2005) for a relevant reply.

2.2.2. Semantic determinants of category membership?   Our question in this sub-section is whether semantics can predict which predicates will end up in which lexical category (N, V or A) cross-linguistically. The answer to the question appears to be "no". Although there have been many attempts to define the core semantics of the lexical categories N, V, and A, the proposed criteria are

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19. Out of the 142 semantic universals in the archive, 46 are statistical; a randomly chosen example is given in (i):

(i)   If a causative morpheme in any language can express permissiveness, it can *usually* also express factitivity (Nedjalkov and Sil'nickij 1969; Vardul' 1969).

We will not pay much attention to statistical universals here, since they are by definition not really universals in themselves, even though they may of course be epiphenomena arising out of deeper universal features.

not exceptionless.[20] For example, it is often claimed that gradability is a characteristic of adjectives, yet not all adjectives are gradable (Baker 2003: 213). Examples also abound of predicates with similar or identical semantics which belong to different categories in different languages. Baker (2003: 291) offers, among others, the examples of English *intelligent*, an adjective, vs. Chichewa *nzeru*, a noun, and of English *know*, a verb, vs. Warlpiri *pina*, a noun (taken from Bittner and Hale 1995: 83). St'át'imcets differs systematically from English in lacking any nouns which correspond to English agentive *–er* forms; these must be rendered by relative clauses containing verbal predicates (Davis 1999).

Even Baker (2003), who offers a universal semantic definition of nouns (as the only elements which have a condition on identity), proposes that the reason all languages have the categories N, V and A has to do with general cognition and with language acquisition. He thus agrees (as do we) with Grimshaw (1981) that the language acquisition device allows for "semantic bootstrapping". This means that children utilize semantico-cognitive categories to help them begin assigning syntactic categories to words.[21] After that, however, the process is completed by means of purely syntactic and morphological evidence.

We conclude that semantic characterizations of the categories N, V and A are able to predict only general cross-linguistic tendencies, and we therefore do not see that there are clear semantic universals to be found in this area. Rather, the evidence seems to support Grimshaw's (1981) claim that although certain cognitive categories have a canonical structural realization, "[s]yntactic categorization is autonomous, since syntactic category membership is not reducible to meaning."

2.2.3. Vendler classes.   Languages often or always sub-divide the class of verbs into lexical aspectual classes or Aktionsarten. The question of whether Aktionsarten are universal is answered affirmatively by van Valin (2006: 177), who claims that

> the basic insight of Vendler (1967), which was formulated solely on the basis
> of data from English, is in fact a linguistic universal: the Aktionsart distinctions

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20. For relevant discussion, see Geach (1962), Kamp (1975), Lyons (1977), Dixon (1977), Wierzbicka (1988), Croft (1991), Hamann (1991), Bhat (1994), Hopper and Thompson (1994), Larson and Segal (1995), Baker (2003), Bittner (2003, 2007b), Wunderlich (2004), Bittner and Trondhjem (to appear), among many others. Note that the issue of whether the categorial labels are part of the lexical entry or are imposed at a different level (cf. Marantz 1997, Borer 2005, etc.) is not relevant here; the arguments apply in either case.

21. As a reviewer pointed out to us, it would be interesting to explore connections between this kind of semantic bootstrapping and the claim that there is a small set of lexical universals, discussed in Section 2.1. If there are such lexical universals, are they preferentially used in semantic bootstrapping?

> derived from his basic four categories are among the most important organizing
> principles of verbal systems in human languages.

Even though van Valin's proposals allow for a certain amount of cross-linguistic variation – for example, he allows predicates which are translation equivalents to belong to different aspectual classes in different languages – we are skeptical of the idea that there is some universal set of Aktionsarten. Cross-linguistic research has actually revealed a lack of uniformity in the semantics of aspectual classes. For example, at least some Salish languages possess a class of verbs which correspond in their lexical content to English accomplishments, but which do not entail culmination of the event in the perfective aspect (see Davis 1978 and Watanabe 2003 for Sliammon, Bar-el 2005 for Squamish, Bar-el et al. 2005 for Squamish and St'át'imcets, and Kiyota 2007 for Saanich). Travis (2005) reports the same fact for Malagasy and cites other researchers for Chinese, Japanese and Tagalog. So, this may in fact be a much more widely distributed phenomenon than an English perspective would lead one to expect. In a similar vein, van Valin himself discusses a difference between English and Mparntwe Arrernte with respect to the semantics of change-of-state verbs. In English, *to cool something* means that the relevant object does get cool; in Mparntwe Arrernte, "cold + PROC" merely expresses a process, with no necessary implication of an endpoint and a result state (van Valin 2006: 165).

The fact that the semantics of 'accomplishments' and other aspectual classes can vary from language to language suggests that what is universal may not be the classes themselves, but perhaps the smaller building blocks from which event structures are composed. Languages could then choose whether or not – to take another example from the Salish family – their class of states is aspectually homogeneous, as in English, or involves an initial change-of-state, as in Squamish (Bar-el 2005) and Saanich (Kiyota 2007). Such an idea is actually supported by one example cited by van Valin. He argues (2006: 174) that Basque has an overt version of Dowty's DO predicate (Dowty 1979). DO is often assumed to be present in the denotations of activities and accomplishments, but importantly, does not itself pick out one single aspectual class. DO, then, could be one of the smaller building blocks which all languages use in constructing their Aktionsarten.[22]

2.2.4. Semantic types of predicates. Montague's article "The Proper Treatment of Quantification in Ordinary English" (1973, usually referred to as

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22. A reviewer asked why we seem to be more sympathetic to Dowty's DO as a universal building block than we were to the lexical universals claimed by NSM supporters. The reason lies in the fact that DO, if it exists universally, would be part of the inventory of functional/grammatical meanings, rather than the "content" lexicon, and as we will discuss in Section 3.1, we think there is (some) universality to be found in that domain.

"PTQ") assumes a strict uniform correspondence between syntactic and semantic types. For example, noun phrases (DPs in current terminology) always correspond to generalized quantifiers (GQs, type $\langle\langle e,t\rangle,t\rangle$) in PTQ. However, subsequent researchers have weakened these syntax-semantic correspondences, and/or proposed cross-linguistic variation in the available semantic types for various syntactic categories. One of the most debated areas is that of nominals: Ns, NPs and DPs.

Partee (2000) claims that every language has Ns and NPs of type $\langle e,t\rangle$, and that common noun phrases are always of type $\langle e,t\rangle$. Similarly, Longobardi (1994) claims that NP is uniformly of type $\langle e,t\rangle$. On the other hand, Krifka (1995) proposes that all simple nouns across languages, mass or count, are of type e, denoting a kind. Following Krifka, Kratzer (2007) assumes that all simple predicates are cumulative (Krifka 1998); this has the consequence that nouns cannot be simple predicates. Instead, they begin with referential denotations and combine with an incorporated, multiply ambiguous, non-overt classifier. Finally, Chierchia (1998) argues that the denotations of Ns and NPs vary cross-linguistically: a language may allow these to be mapped into predicates, into arguments (i.e., kinds), or into both – constituting a "semantic parameter".

This list of contradictory analyses reveals that there is no consensus about a universal mapping of the categories N or NP to a single semantic type. On the other hand, the range of meanings proposed for these categories is still highly restricted – they denote either predicates or kinds. This reflects the distinction between basic expressions and functional expressions to be introduced in Section 3.1.1, with the former having low types and the latter high.

Closely tied to the question of possible semantic types is the issue of the availability of operations which shift types. Partee (1987) argues that potential DP meanings are of type $\langle\langle e,t\rangle,t\rangle$, e or $\langle e,t\rangle$, and proposes a set of type-shifting operations for getting from one DP/NP-meaning to another:

(9) a. lift: $j \rightarrow \lambda P[P(j)]$
     b. nom: $P \rightarrow \cap P$
     c. iota: $P \rightarrow \iota x[P(x)]$
     d. A: $\lambda Q\lambda P\exists x[Q(x)\wedge P(x)]$

Partee (1987) does not explicitly say that her proposals are intended to be universals, although subsequent authors have often assumed that that is how they were meant. For example, Chierchia (1998: 358) writes that "Partee (1987) has shown that there is a restricted number of type shifting devices that appear to be used in the languages of the world", yet according to Partee (1998: 369), "the principal motivation of Partee (1986, 1987) was to identify the types of *English* NPs and the principles governing the type-shifting possibilities within the family of NP interpretations" [emphasis added].

There are two ways in which we could make Partee's (1987) proposals into a universal claim: first, we could assume (with Chierchia) that *lift*, *nom*, etc. are the only available type-shifting operations in the nominal domain. This is a standard restrictiveness assumption: if these type shifts are what is required for one language, we suppose no others are necessary. Second, we could separately claim that all languages *must* possess these operations. This latter assumption is adopted for example by Giannakidou (2004), and it leads her to argue that St'át'imcets (contra the analysis of Matthewson 2001) must possess predicative DPs.

As with Ns and NPs, the conclusion is that the field does not currently agree on any universal uniform semantic type for DPs. As we will see in Section 3.1.3, the field does not even agree that all languages possess GQs. The best candidate for a universal in this area may be Partee's (2000) claim that "[i]f a language has DPs at all, it has DPs of type *e*."

3.   Putting meanings together

We have seen that in the first component of meaning, the lexicon of content morphemes, there is some universality at the level of constraints on possible lexical items, while virtually no individual lexical items are universally attested. It appears to be true that languages make idiosyncratic decisions on which predicates to elevate to the status of being denoted by a designated simple lexical item. Now, let's look at how we go from lexical predicates to full-fledged propositional meanings. If all we could do was string together content morphemes, we would not be able to express propositions, let alone propositions of any kind of interesting complexity. What is needed is "semantic glue", which comes in two forms: (i) functional/grammatical morphemes, (ii) principles of semantic composition. What we explore in this section is whether there are universals about semantic glue.

3.1.   Functional morphemes

3.1.1.   Introduction.   Consider a hypothetical example:[23]

(10)   cat purr

The common noun *cat* denotes a predicate of type $\langle e,t \rangle$, in other words a set of entities, the set of cats. The intransitive verb *purr* also denotes a predicate of

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23. Some of the introductory material in this section is recycled from von Fintel (1995).

type $\langle e,t \rangle$, the set of purring objects. Or perhaps it denotes a two-place predicate relating situations and entities. In any case, the types of the two words will not combine by the simplest way of combining meanings, namely function application. Something needs to mediate between the two basic types. What is needed is a functional meaning, a higher type meaning that takes the two low-level types and results in a normal sentence meaning. The simplest meaning we can imagine would be one that says that there is a situation in which there is something which is both a cat and a purring object. But there are other possibilities: we could add both a determiner meaning to the common noun and an inflectional meaning to the verb. So, semantically we need functional meanings as a kind of glue holding together the low-type meanings of content morphemes. From the point of view of generative syntax also, a sentence like (10) without functional categories is not a well-formed sentence. Functional categories are needed to complete the small clause structure in (10) and yield a fully formed sentence.

The distinction between functional morphemes and content morphemes is of course well-established in many linguistic frameworks. One of the crucial properties of functional morphemes is that, in any given language, their inventory is limited, as opposed to the open-ended lexicon of content items. A list of some important kinds of functional morphemes may give an idea of what we are dealing with:

(11)   Noun Class – Gender – Number – Determiner – Quantifier – Case – Verb Class – Voice – Aspect – Tense – Modality – Negation – Complementizer – Conjunction – *Wh*-Elements – Topic/Focus – Degree Words – Comparative – Superlative

From looking at the list in (11), it seems that one could essentially write a whole grammar of a language by describing just the functional morphemes in that language. This intuition is often framed as a principle of natural language: cross-linguistic variation in the grammar is confined to the inventory and properties of functional morphemes (Borer 1983; Chomsky 1991; Fukui 1986).

One might ask why a semantics article (on universals or any other topic) has anything to say about functional morphemes. After all, aren't functional morphemes just that: "functional", without content? Indeed, in the literature on the diachronic development of functional morphemes out of content morphemes, it is felt that a morpheme that is being grammaticalized gradually loses its meaning. There are numerous suggestive terms for this aspect of grammaticalization: Heine et al. (1991: 40) mention "semantic bleaching", "semantic weakening", "semantic fading", "desemanticization", "generalization or weakening of semantic content". In generative grammar as well, there is a prevailing position that the semantic reflex of the functional/lexical dichotomy is that functional

items are meaningless. Their contribution should be exhausted by the time semantic interpretation happens. In concert with the "Principle of Full Interpretation" (Chomsky 1986: every symbol in an LF representation must have an interpretation), it is then natural to assume that functional items are deleted by the time we reach the end of the syntactic derivation. In the parlance of the minimalist program, as soon as "checking" has happened, the job of functional morphemes is done and they quietly get erased.

But, are functional items really vacuous? We concede that case and agreement markers might be meaningless, although at least so-called inherent cases seem to have semantic content, and some parts of agreement morphemes carry semantic information (person, number and gender are not meaningless features). For most other functional morphemes, the view that they have no meaning is *entirely* mistaken. The semantics of determiners, modals, tenses, aspects etc. is after all the bread and butter of working semanticists.

Could there be languages that do without functional categories? As we have seen, the meanings of functional categories seem indispensable even in the most primitive examples. Nevertheless, particular linguistic systems may do without certain functional meanings. Below, we will discuss the claims that some languages have no quantifiers (3.1.3) and that some languages are tenseless (3.1.5). Are there systems that do without *any* functional categories? Claims like that have been made about early stages of the language acquisition process, about pidgin languages, about the state of language in patients suffering from agrammatism, and about the sublanguage used in telegrams. But crucially, none of these systems are natural languages in the full sense of the term. In each case, it has often been recognized that they are deficient in precisely the sense of not employing functional categories. It is important to realize that the claim is not that functional categories are necessarily overtly expressed. In fact, Carlson (1983) notes that functional meanings are often present even in the absence of overt morphemes expressing them.[24] There are various technical options at this point: empty functional items, type-shifting, features on lexical categories, "constructional" meaning. In the absence of overt marking, other mechanisms presumably apply that help identify the intended functional meaning. There is semantic work on how the functional glue is supplied in constructions like free adjuncts (Stump 1985) and genitives (Partee 1984).

The semantic glue we need has to have high-type meanings (operations that take predicates and yield propositions or at least intermediate meanings on the way to propositions).[25] This has been proposed, among others by Chierchia

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24. We therefore need to be careful whenever we are tempted to claim that a particular language lacks a certain functional category.
25. One kind of closed class morpheme that this high-type vision is not applicable to are pronouns. These clearly form a constrained system in languages and are the locus of interesting

(1984) and Partee (1987).[26] Let us assume that there are two basic levels of expressions: expressions that refer to entities or situations and expressions that denote predicates of entities. Functional categories have high semantic types, beyond these levels of entities, situations, and predicates. Quantificational determiners for example relate two sets of entities. Tenses (perhaps) are quantifiers relating two sets of situations. And so on. There are even fourth order operators: for example the higher order modifiers of modifiers *almost* and *very*. Chierchia links the high type proposal to another interesting property of functional items: there are no grammatical processes that involve quantification over functors. For example, *almost*, *again*, *too* etc. do not represent a possible answer to *how*-questions. Similarly, they do not enter comparative formation or act as the antecedent of proforms like *thus* or *so* (Chierchia 1984: 86).[27]

As we will see below in the section on "conservativity" (3.1.2), there is reason to think that functional meanings come from a small universal inventory, from a restricted set of "natural" meanings. If there are strong universal constraints on what a possible functional meaning is, we can envision that learning functional meanings will be considerably easier. In fact, authors like Partee and May have suggested that the inventory of possible functional meanings is innate:

> Another important general point . . . : the open-ended lexical classes, the lexical nouns, verbs, adjectives, etc., nearly all have interpretations that are virtually never higher than first or second order in the type hierarchy. Natural language expressions which seem to call for an analysis in higher types than first or second order . . . tend to belong to small closed syntactic categories whose members seem very close to being universal. Examples are determiners, conjunctions, words like *only*, *almost*, *very*, the *more than* construction, etc. These function words that belong to small closed classes typically involve the most lambdas when their meaning is spelt out explicitly. That suggests that in acquiring those items that really involve the higher types, children do not have to find their way through the whole domain of possible meanings. In the higher types we presumably do not conceptualize full domains, since we not only lack open-class lexical items but also anaphora, quantification, or question-words. Rather there seem just to be certain very useful particular meanings of higher types that we have evolved into our language faculty. (Partee 1992: 124–125)

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universals, as shown for example by Bobaljik (2007). Are pronouns functional morphemes? We will not have anything to say about pronouns in this article.

26. There are two old masters who envisioned a similar hierarchy of meanings. Sapir (1921: 101) has a four-level hierarchy: basic (concrete) concepts, derivational concepts, concrete relational concepts, and pure relational concepts. Jespersen (1924), who is cited as an inspiration by Chierchia, has a hierarchy of primaries, secondaries and tertiaries.

27. Landman (2006) has recently revived this proposal. She proposes a No Higher Types Variable Constraint, restricting traces and pro-forms to being of type *e*.

> In distinguishing the logical elements in the way that we have, we are making a cleavage between lexical items whose meanings are formally, and presumably exhaustively, determined by UG – the logical terms – and those whose meanings are underdetermined by UG – the non-logical, or content, words. This makes sense, for to specify the meaning of quantifiers, all that is needed, formally, is pure arithmetic calculation on cardinalities, and there is no reason to think that such mathematical properties are not universal. For other expressions, learning their lexical meanings is determined causally, and will be affected by experience, perception, knowledge, common-sense, etc. But none of these factors is relevant to the meaning of quantifiers. The child has to learn the content of the lexical entries for the non-logical terms, but this is not necessary for the entries for the logical terms, for they are given innately. (May 1991: 353)

If the inventory of possible functional morphemes is innate and small, is this reflected at all in some functional morphemes being universally attested? The question is made hard by the possibility that a functional morpheme might be covert in a given language, so that superficial surveys will not be able to establish that a certain functional morpheme is not universal. Nevertheless, among the lexical items that (Goddard 2001: 57) concludes are universal, there are quite a few functional morphemes (or ones that come close):[28]

(12)   I, you, someone, something/thing, this, the same, one, two, all, much/many, there is, when/time, now, before, after, not, maybe, because, if, like, very

In the following subsections, we will look at several questions of the form: does every language have functional category X? Before, though, we will discuss the archetype of a strong constraint on the inventory of functional morphemes.

3.1.2. Conservativity.   Among formal semanticists, the most celebrated semantic universals are those proposed by Barwise and Cooper in their seminal article on quantifiers in natural language (1981). Their particular concern are quantificational determiners, items such as *every, some, no, most, few, many, . . .* , which are treated as denoting second order relations between two sets, the first picked out by the common noun phrase argument of the determiner, the other supplied by the rest of the sentence – this makes them of semantic type $\langle\langle e,t\rangle,\langle\langle e,t\rangle,t\rangle\rangle$. For example, *every A B* claims that the *A*-set is a subset of the *B*-set, while *some A B* says that the intersection of the two sets is not empty. What Barwise and Cooper noted was that the first argument, the set

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28. Note that Bohnemeyer (1998) argues that Yucatec Maya does not have expressions meaning 'before' and 'after' (and no two-place temporal connectives, more generally), contrary to the NSM literature.

denoted by the common noun phrase, is special: this set *restricts* the quantifier, or supplies the *domain* of the quantifier. Barwise and Cooper claim that determiner-quantifiers universally "live on" their first argument. This property is now widely known as *conservativity*, so-termed by Keenan and Stavi (1986).[29]

(13)   A generalized quantifier q lives on a set X iff for all sets B:
$q(B) \equiv q(X \cap B)$.

(14)   A determiner δ is conservative iff for all sets A: $\delta(A)$ lives on A.

(15)   In relational terms: δ is conservative iff for all sets A, B:
$\delta(A)(B) \equiv \delta(A)(A \cap B)$.

What this means is that the first argument of a determiner "sets the scene". To evaluate the quantificational statement, we only have to look at the members of the set A picked out by the first argument. An intuitive test for conservativity is illustrated by the following equivalences:

(16)   Every man smokes $\equiv$ every man is a man who smokes.
Some man smokes $\equiv$ some man is a man who smokes.
No man smokes $\equiv$ no man is a man who smokes.
Most men smoke $\equiv$ most men are men who smoke.
Few men smoke $\equiv$ few men are men who smoke.
Many men smoke $\equiv$ many men are men who smoke.

Barwise and Cooper essentially formulate the following universal:[30]

(17)   Every determiner in every natural language is conservative.

This universal is of course only interesting if it is not taken as definitional, if the category of determiner is identified on independent grounds. If correct, conservativity could conceivably be taken as the reason why determiners form a constituent with this first argument. Barwise and Cooper have some speculations about the importance of conservativity to the semantic processing of quantificational statements, suggesting a psycholinguistic justification for the universal.[31]

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29. Higginbotham and May (1981) discuss the property under the term *intersectivity*.

30. We will see in the next subsection that this is not quite what they say. They in fact make much stronger claims of universality, which are more problematic than the conservativity universal that is entailed by them.

31. Chung and Ladusaw (2006) suggest that conservativity follows from a more general principle that semantic composition is always asymmetric. We cannot address this interesting suggestion here.

To see the empirical force of the universal, let's consider some possible non-conservative determiner meanings. First, an example discussed by Chierchia and McConnell-Ginet (1990: 426-427):

(18)   allnon $(A)(B) \equiv (D - A) \subseteq B$.

We would use this determiner to say things like *allnon students smoke*. This is a perfectly plausible thing to want to say. This determiner is not conservative, as you can see intuitively from the failure of the following equivalence:

(19)   Allnon students smoke $\not\equiv$ allnon students are students who smoke.

It is easy to imagine a situation where the first sentence is true while the second sentence is false (in fact, the second one will be false as long as there are non-students). As far as we know, there is no language that has a determiner expressing the meaning in (19). Since this is not an implausible meaning to encapsulate in a determiner (it is perfectly easy to convey the meaning in ways that do not involve a non-conservative determiner: *all non-students smoke* or *everyone who is not a student smokes*), this lexical gap is surprising. The semantic universal that says all natural language determiners are conservative therefore has real bite to it.[32]

Other quantificational determiners that are ruled out by the conservativity universal are determiners that would express that there are more *A*s than *B*s or that there are exactly as many *A*s as there are *B*s. It is crucial that all these meanings are meanings that are easy to express, just not by using a non-conservative determiner. (Note the recurrence of a theme we mentioned in Section 1.2: we need to distinguish the inventory of what gets lexicalized in individual morphemes from the awesome expressive power that natural languages display when they can use complex expressions.)

Another, much more important, candidate for a non-conservative determiner is *only*, with a (simplified) semantics as follows:

(20)   only $(A)(B) \equiv B \subseteq A$.

Again, it is easy to see the non-conservativity:

(21)   Only students smoke $\not\equiv$ only students are students who smoke.

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32. Keenan and Stavi (1986) show that in a toy universe of 2 individuals, there are 65,536 functions of the determiner type, but only 512 of these are conservative.

The second sentence is always true. In set-theoretic terms, it says that the students who smoke are a subset of the students. But, for any two sets A, B, it always holds that $A \cap B \subseteq A$. From this, it does not follow that $B \subseteq A$ as claimed in the first sentence. So, *only* is not conservative.

Barwise and Cooper's universal would be in trouble if we couldn't argue that *only* is not a determiner but more likely an adverbial category of some sort (albeit with a "quantificational" meaning). Some of the familiar arguments for this claim are that (i) *only* can combine with pronouns or names, other determiners cannot; (ii) *only* can occur "on top of" other determiners; (iii) *only* combines with categories other than noun phrases:

(22) a. Only John slept.
      b. Only two books were bought.
      c. John only bought jewelry.

Only is therefore not a determiner.[33]

The main proposed counter-example to conservativity is the occurrence of *many* in the following famous example from Westerståhl (1985):

(23) Many Scandinavians have won the Nobel Prize in literature.

Westerståhl observes that (23) can be interpreted as making the same claim as the following sentence:

(24) Many of the winners of the Nobel Prize in literature were Scandinavians.

Now, (23) is not conservative *on its first argument* but if it really is equivalent to (24) on the relevant reading, it would be conservative *on its second argument*, so to speak. These cases are still under active investigation (see for example, Herburger 1997 and Cohen 2001), but they are genuine problems for the strong conservativity universal. On the other hand, since the counter-examples are all cases where the determiner is conservative on the other argument, Keenan writes that "the whiff of generality is in the air" (Keenan 1996) and explores the notion of conservativity on either argument (Keenan 2002). Zuber (2004: 164), as well, concludes that "even though natural languages have non-conservative determiners, the class of such determiners is still very restricted."

It is clear that natural languages do not nearly make full use of the logically possible space of determiner meanings. The conservativity universal may be

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33. For a detailed investigation of how apparent determiner uses of *only* as in *only students smoke* are actually composed from the adverb *only* and the bare plural DP *students*, see von Fintel (1997). See Zuber (2004) for an exploration of non-conservative determiners in Polish that are related to *only* in meaning.

fraying at the edges, but the investigation of the limits on possible determiner meanings is still a promising avenue of research.[34]

3.1.3. No quantification?   Probably the two most influential universals in the area of quantification are Barwise and Cooper's (1981) U1 and U3, given in (25)–(26). We discussed a consequence of (26) in the context of conservativity (in the previous subsection); here we concentrate on the stronger claim that every language possesses determiners which operate on common noun denotations to create generalized quantifiers (GQs).

(25)   NP-Quantifier Universal (Barwise and Cooper 1981: 177)
        Every natural language has syntactic constituents (called noun-phrases) whose semantic function is to express generalized quantifiers over the domain of discourse.

(26)   Determiner Universal (Barwise and Cooper 1981: 179)
        Every natural language contains basic expressions, (called determiners) whose semantic function is to assign to common count noun denotations (i.e., sets) A a quantifier that lives on A.

A language which falsifies (25) automatically falsifies (26), since if a language lacks GQs entirely, it must lack determiners which create GQs. On the other hand, it is possible for a language to falsify (26) while satisfying (25).

Recent cross-linguistic research has produced many counter-examples to the NP-Quantifier Universal. For example, Baker (1995) argues that Mohawk lacks quantificational noun phrases; Jelinek (1995) proposes the same for Straits Salish, as does Vieira (1995) for Asurini do Trocará. Bittner and Hale (1995) argue that Warlpiri lacks the category Determiner, and imply that as a consequence, Warlpiri nominals are only of type e or $\langle e, t \rangle$. Faltz (1995) argues against (25) for Navajo, claiming that in this language, "the quantifier is not in construction with the noun it applies to" (Faltz 1995: 294). Speas and Parsons Yazzie (1996) similarly argue that Navajo lacks "true quantification" in the sense of a quantifier A-binding an anaphor.[35]

With respect to (26), Lee (to appear) claims that San Lucas Quiaviní Zapotec lacks quantificational determiners. A language which has been explicitly claimed to satisfy (25) but falsify (26) is St'át'imcets (Matthewson 1998,

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34. Another fascinating puzzle is the systematic non-existence of determiners meaning 'not all', discovered by Horn (1972, 1989); see Hoeksema (1999) for a recent discussion.

35. Unlike Faltz, Speas and Parsons Yazzie do not attribute the absence of quantificational noun phrases in Navajo to Navajo's supposed status as a pronominal argument language.

2001).[36] As shown in (27)–(28), St'át'imcets does not allow quantificational elements to combine directly with an NP; instead, they must co-occur with an article, which is obligatorily non-quantificational (see also Matthewson 1999). However, the entire nominal phrase is quantificational. For example, Matthewson (1999) shows that the bracketed constituent in (28) has only a proportional, not a cardinal, interpretation.[37]

(27)   [*tákem   *(i)   syáqts7-a*]   *q'weláw'-em*
        [all   DET.PL   woman-DET]   pick.berries-INTR
        'All the women picked berries.'

(28)   [*cw7it   *(i)   plísmen-a*]   *úxwal'*
        [many   DET.PL   policeman-DET]   go.home
        'Many (of the) policemen went home.'

The consensus in the literature thus seems to be that neither neither the NP-Quantifier Universal nor the Determiner Universal is correct. Of course, both these universals actually make claims about the syntax-semantics interface. Thus, it is possible that in spite of lacking the relevant syntactic-semantic correspondences, the languages listed above still express the same quantificational semantics as languages like English, and thus that there is a true semantic universal lurking somewhere.[38] In support of this we can cite for example Baker (1995), who suggests that Mohawk *akweku* 'all', while not syntactically a determiner and not able to create quantificational noun phrases, is a distributor with a semantics paralleling that of English *each* (as in Heim et al.'s (1991) analysis of sentences like *The men each left*).[39] For Navajo, Fernald et al. (2000a) write that "[r]egardless of the syntactic issues involved, quantificational statements can certainly be made in Navajo." And Bach et al. (1995a: 1), who are convinced that the NP-Quantifier Universal does not hold, nevertheless begin with the assertion that "[e]very natural language provides some means for making general statements."

Can we make any statements stronger than "all languages have some kind of quantification"? Matthewson (2004) reports that in a survey of 33 languages, all

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36. Matthewson argues that (26) is falsified across the entire Salish family, but that there is no strong evidence against (25) in Salish (pace Jelinek 1995).

37. The sound annotated as a "7" is a glottal stop in the official orthography of St'át'imcets; see Footnote 15.

38. Matthewson (2001) actually suggests that there may be a universal syntax-semantics correspondence inside quantificational noun phrases, but one which reflects the St'át'imcets surface structure, rather than the English. If that idea were right, then (26) would need to be changed to begin with "no" instead of "every".

39. In fact, one reading of Baker (1995) (the reading gleaned from the Appendix) is consistent with the claim that Mohawk does not falsify (25), but only (26) (as in Matthewson's analysis of Salish).

of them possess at least universal and existential quantifiers, so perhaps there is a universal claim ripe for the making there. On the other hand, Matthewson (2004) finds that two-thirds of the languages examined seem to have no strong quantifiers other than universals – at least as far as the information in descriptive grammars can be trusted.[40]

We need to exercise caution here, however; let's take the English strong quantifier *most* as an example. According to Ken Hale (p.c. to Lisa Matthewson), almost no languages have a lexical element corresponding to *most*, and the descriptive literature very often fails to mention such a quantifier. As mentioned in section 1.2 above, Everett (2005) certainly denies the existence of 'most' in Pirahã. However, a closer look often reveals some construction which appears to express the same meaning as English *most*. For example, Keenan (to appear) shows that in Malagasy, proportionality quantifiers are formed via nominalizations which yield expressions meaning 'a majority of' or 'a minority of'. An example is given in (29); the form for 'majority' is formed from the quantifier *be* 'big, many':

(29)   *tsy   mianatra   Alahady   ny   ankabeazan'ny   mpianatra*
      not   AF+study   Sunday   the   majority'the   student
      'The majority of the students don't study on Sunday.'
      (Keenan to appear: 348, taken from Rakotondranaivo 1986: 55)

Similarly, Zimmermann (to appear: 453) shows that Hausa expresses 'most' by means of a noun meaning 'majority', and Zerbian and Krifka (to appear: 398) show that in Swahili, 'most' is rendered by a nominalization translated as 'a big part of' (although Zerbian and Krifka also observe that this construction has a wider range of use than English *most*, apparently being possible in cases where English would use proportional *many*). The point here is that the Pirahã use of a nominalization of the word for 'big' to express 'most' (Everett 2005: 624) may be (i) cross-linguistically quite common, and (ii) not that significant from a purely semantic point of view, as long as the truth conditions turn out to be those of a proportional quantifier.[41]

We are not in a position to propose any new semantic universals in the realm of quantification here. It seems clear to us, however, that the field needs to move beyond Barwise and Cooper's U1 and U3 – inspiring and beneficial as

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40. One respect in which it is difficult to draw conclusions about strong quantifiers from descriptive sources is that such sources rarely, if ever, say whether forms translated as 'many' or 'few' admit proportional readings.

41. There is a parallel with comparatives here: though explicit items like *more than* are not universal, expressions like *you are big from me* are commonly used to create comparatives (Kennedy 2007).

they have been – and propose new universals to be subjected to empirical testing. Generalized quantifiers are not the be-all and end-all; they are just one of many attested strategies for expressing quantificational notions.[42] It may be fruitful to search for cross-linguistic regularities at a more fine-grained level, perhaps even at the level of individual quantifier meanings or of individual syntax-semantics correlations. To take one example, very many languages appear to possess both distributive and non-distributive universal quantifiers, and in many cases, that semantic difference correlates with a difference in syntactic behaviour. Why is this, and is the phenomenon universal, or merely widespread?

We conclude this section by briefly introducing a more radical idea about quantification cross-linguistically. Bittner and Trondhjem (to appear) discuss Kalaallisut constructions they call Q-verbs, which appear to be quantificational in meaning, but from which there is no way to create tripartite logical forms without violating lexical integrity. Bittner and Trondhjem propose that Q-verbs involve discourse reference to distributive verbal dependencies. (This approach is comparable to Carlson's (1977) idea that genericity involves reference to kinds rather than quantification.) Bittner and Trondhjem do not go so far as to assert that the standard analyses of quantification are incorrect; they do speculate, however, about the possibility of extending their analysis of Kalaallisut Q-verbs to all quantifiers in all languages. According to this idea, all quantifiers would evoke discourse referents for distributive dependencies, with the values being either episodes (for verbal predicates) or individuals, times, places, or propositions (for nominal predicates).

3.1.4. No logic?   We have seen that languages can forego using the kind of quantificational determiners so familiar from English and similar languages. Nevertheless, every language seems to be able to express quantificational claims – it's just that languages employ different encoding strategies. Is the same true for other "logical" operations?

Our discussion here centers around Gil's (1991: 97) proposals that (i) Maricopa (Yuman) "has no uniform device for expressing any of the basic logical connectives of the propositional calculus", (ii) the Maricopa translations of English sentences involving coordination are vague and are interpreted using extra-grammatical processes, and (iii) the logical connectives may be absent

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42. A reviewer suggested that the adverbial quantification strategy may qualify as a (syntactic-) semantic universal. However, Bittner and Trondhjem (to appear: 11) argue that the class of A-quantifiers is not only syntactically non-uniform (comprising as it does adverbs, auxiliaries, affixes, and argument-structure adjusters (Partee 1991)), but semantically non-uniform, quantifying over a diverse range of semantic objects cross-linguistically.

from the logical forms of Maricopa. However, we will argue that the evidence Gil provides for each of these three claims is not convincing.

Gil argues that Maricopa has no word for 'and' or any other coordinator, and no syntactic category of coordinator.[43] The evidence comes from translations into Maricopa of English sentences as in (30):

(30)   John and Bill will come.

The simplest strategy used in Maricopa to express (30) involves concatenation of the two noun phrases:[44]

(31)   *Johnš*   *Billš*   *v7aawuum*
        John-nom   Bill-nom   3-come-pl-fut
        'John and Bill will come.' (Gil 1991: 99)

Gil considers the possibility that there is a zero coordinator in (31), but concludes that there is not, partly because he is "aware of no evidence to the effect that the two NPs in these constructions form a syntactic constituent" (Gil 1991: 101). He therefore assigns to (31) a ternary-branching flat structure. However, no constituency tests are applied to the sequence of two NPs in (31). It would seem crucial to provide syntactic evidence that the NPs fail constituency tests before a flat structure is adopted.

Gil also provides a semantic argument that there is no coordination in (31), namely that its interpretation is vague, a claim supposedly evidenced by the fact that a very similar string can be interpreted as a disjunction:

(32)   *Johnš*   *Billš*   *v7aawuumšaa*
        John-nom   Bill-nom   3-come-pl-fut-infer
        'John or Bill will come.' (Gil 1991: 102)

According to Gil, (31) vs. (32) provide evidence that the concatenated NPs in Maricopa do not correspond to any specific English coordinator, since the same simple concatenation translates both *and* and *or*. However, caution is required! It is a far cry from the observation that Maricopa uses concatenation of two NPs to render both English *and* and *or*, to the conclusion that juxtaposed NPs

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43. According to Payne (1985), there are other languages that obligatorily use juxtaposition to express coordination (*optional* juxtaposition is of course widespread and occurs in English as well). See also Haspelmath (2000).

44. Other strategies include the use of subordinated verbs meaning 'accompany' or 'be together'. Gil does not provide data concerning other logical connectives apart from ∧. Also note that the assumption that (30)–(31) are a test for the propositional connective ∧ entails potentially problematic assumptions about conjunction reduction. It would have been better to use a full clausal conjunction.

receive their interpretation via "extra-grammatical inferences", similar to those involved in interpreting a string of NPs in a shopping list (Gil 1991: 102).

There is an interesting analysis of these kinds of facts within a principled formal framework proposed by Winter (1995). The idea is that the logical operation conjunction is *never* carried by items like *and*. Instead, such coordinators simply create tuple-structures, which are then interpreted via logical conjunction as a default composition rule. Winter formulates the following principle:

(33)   The Universal of Coordination
        Conjunction is *syn*categorematic. Disjunction is *cat*egorematic.

Examples like (32) are treated as involving the same kind of tuple-construction but the presence of the special modal meaning indicated by the suffix *šaa* on the verb triggers a disjunctive rather than conjunctive interpretation of the tuple.[45]

What's the upshot? Gil's claim that Maricopa doesn't express logical conjunction does not seem right. On the other hand, Maricopa seems to be a very useful case for figuring out *how* conjunction is semantically expressed. If Winter is right, Maricopa reveals the universal mechanism of conjunction more directly than English does.

We should note that one other logical connective, → (material conditional), is arguably absent not just from Maricopa but from all natural languages. The story is ably told by Kratzer (1986).[46]

Does any of this suggest deep cross-linguistic differences? Gil himself draws radical conclusions from the purported absence of elements corresponding to connectives in Maricopa. He argues that if we assume that logic is universal, Maricopa linguists "must derive a way to get from the exotic syntactic structures of Maricopa to the familiar, comforting forms of classical logic" (Gil 1991: 119). He argues instead that perhaps classical logic is not universal, and perhaps logical forms look very different in Maricopa from in English. (Exactly what the Maricopa logical forms would look like is not spelled out.) However,

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45. Winter notes that something similar seems to happen in Dyirbal, as reported by Dixon (1972: 363, example (693)) with the particle *yamba* ('perhaps, might be'). Also Winter points out a parallel to English juxtaposition:

(i)   Who's coming to the party?
         Mary is coming, John is coming . . . I don't know.

46. It is interesting to note that proponents of Natural Semantic Metalanguage do not list *and* or *or* among the semantic primes, and although they do list *if* (as well as *not*), it is highly unlikely that NSM *if* corresponds to logical →. The canonical contexts for *if* provided by Goddard and Wierzbicka (1994: 52) are 'If it rains, I won't come' and 'If you do it, people will say something bad about you.'

the force of this argument escapes us, since classical logic is not in any case intended to reflect the compositional structure of natural language sentences (as the logical translation of a sentence containing a universal quantifier immediately confirms). The question is, are there languages which differ from English in such a radical way that the devices of formal logic are an inappropriate tool for making precise the truth conditions of their sentences? We doubt it.

We conclude that while perhaps none of the logical connectives are universally lexically expressed, there is no evidence that languages differ in whether or not logical connectives are present in their logical forms.

3.1.5.   Tense, aspect, modality.   Are there tenseless languages? It is not obvious how to define what one might mean by this question. As we discovered in the discussion of quantification, we could search for syntax-semantics universals, or for purely semantic universals. We could also propose stronger or weaker universal statements, depending on whether we believe that it is a defining property of being a "tense" that the relevant distinctions are obligatorily encoded (as claimed by for example by Smith et al. 2003). Some potential universals are listed in (34)–(36); note that these already presuppose a certain analysis of what "tense" does semantically, which, although fairly widespread, is probably not uncontroversial.

(34)   All languages possess a syntactic head T whose function is to locate the reference time with respect to the utterance time.

(35)   All languages obligatorily locate the reference time with respect to the utterance time in all finite clauses, by means of some (null or overt) grammatical morphology.

(36)   All languages have ways to locate the reference time with respect to the utterance time (e.g., adverbials, contextual inferences, . . .)

Recent literature on tenselessness presents some fairly convincing arguments against (34) and/or (35) for various languages; see, for example, Bohnemeyer's (2002) work on Yukatek Maya. We probably need to conclude that (34) and (35) are both false, and that while (36) may be true, it is weak enough to not be particularly interesting.[47] However, this does not mean that there are no universals to be found in the area of temporal semantics. On the contrary, there is a lot of work still to be done establishing the limits on variation in this area.

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47. Other relevant references include Baker and Travis (1997), Lee (1999), Shaer (2003), Smith et al. (2003), Sybesma (2003), Wiltschko (2003), Ritter and Wiltschko (2004, 2005), Bittner (2005), Hayashi and Spreng (2005), Matthewson (2005, 2006b), Smith and Erbaugh (2005), Hayashi (2006), Lin (2006), among others.

The first reason there is work still to be done is that some current proposals (as in any area) may turn out not to be correct. For example, Ritter and Wiltschko (2004, 2005) argue that Blackfoot and Halkomelem lack a syntactic head T, based in large part on diagnostics which do not relate to temporal notions, but instead to nominative Case (under the assumption that T is responsible for nominative Case checking). Reis Silva and Matthewson (2007), on the other hand, argue that contrary to claims made by Ritter and Wiltschko, Blackfoot does display an overt contrast between present and past tense. One possibility, then, is that the claimed lack of nominative Case in Blackfoot should not be derived from an absence of T. In other words, perhaps Blackfoot possesses T, but it is the link between T and nominative Case which is not a language universal.

The second reason that universals may yet be uncovered in the realm of tense is that even if (34) and (35) are false, languages do not seem to vary in an unlimited fashion in their temporal systems. There are some intriguing similarities between languages which intuitively do not seem to be learnable from primary linguistic data, and which therefore deserve further investigation as potentially deriving from UG. For example, Matthewson (2006b) shows that in St'át'imcets, a language which does not overtly distinguish between present and past tense, the temporal interpretation possibilities for past as opposed to future-tense embedded clauses are strikingly parallel to those of English.

Just as with tense, current research is also uncovering cross-linguistic differences in the areas of evidentiality, modality, and aspect. For example, Faller (2002b, 2006) argues that some evidentials in Cuzco Quechua are not epistemic modals, but instead are speech-act operators, and that languages vary in this respect. While evidentials are still most commonly analyzed as some kind of epistemic modals, the conclusion that their semantics varies cross-linguistically is supported by an array of formal analyses of evidentials in different languages; see e.g., Izvorski (1997), Garrett (2000), Ifantidou (2001), Rooryck (2001a,b), Faller (2004), McCready and Asher (2005), McCready and Ogata (2006), Matthewson et al. (2006), Chung (2007), among others. One challenge facing universals research in the field of evidentiality is that there is a rather large disconnect between the abundant typological research (which rarely, if ever, applies any rigorous semantic testing) and the formal theoretical research (which does not usually make universal claims). One notable attempt to bridge the gap is Faller (2002a), who proposes a universal hierarchy of evidence types; see also de Haan (1998).

In the area of modality, we again find evidence that languages may share fundamental aspects of meaning, while differing in the lexicalization of certain distinctions or in the syntactic means they use to achieve a similar semantics. For example, Rullmann et al. (2006) show that modals in St'át'imcets encode the exact opposite distinctions to those encoded by English modals. While En-

glish modals lexically mark quantificational force, leaving the conversational background up to context (Kratzer 1991), St'át'imcets modals lexically mark conversational background, and leave quantificational force up to context. In spite of the different places in which the languages make these distinctions lexically explicit, the basic modal semantics is entirely parallel in the two languages.

Finally, with respect to aspect, it seems as if there are no primitive or universal "constructions" such as the perfective, the imperfective, or the perfect. Much of the research on individual languages advances non-English-like semantics for viewpoint aspect operators; see for example Singh (1998), Koenig and Muansuwan (2000), Wilhelm (2003), Bar-el (2005), Kiyota (2007), among many others, and see in particular Smith (1997) for cross-linguistic discussion. Still, the variation is undoubtedly not random; we strongly suspect that there are common semantic building blocks to be found (cf. the discussion of lexical aspect in section 2.2.3). But as far as we know, and just as in other functional domains, the study of aspect is not yet at a stage where universal claims of this sort are being made.[48]

3.2.   *Universals of composition*

3.2.1.   Compositionality and recursion.   The principle of compositionality is given (in one of its formulations) in (37).

(37)   The meaning of a compound expression is a function of the meanings of its parts and of the syntactic rule by which they are combined. (Partee et al. 1990: 318)

This principle is foundational to Montague Grammar and is widely assumed within many modern formal semantic frameworks. Whether one adopts compositionality or not greatly affects the types of analyses and theories one proposes or favors.

Is (37) a semantic universal? Szabó (2000: 479) argues that for compositionality to be meaningful, it must be assumed to apply to all languages, and should therefore be formulated as in (38):

(38)   For every possible human language L and for every complex expression e in L, the meaning of e in L is determined by the meanings of the constituents of e in L and by the structure of e in L.

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48. We don't have space to discuss all classes of functional meanings here, even though there is interesting work on some of them; see for example Harbour (2007) on universals about number.

However, it is not clear whether the question of whether compositionality is universal can meaningfully be asked, since it is not clear to what extent it is empirically falsifiable, rather than simply being a methodological principle. As Groenendijk and Stokhof (2005) (among many others) point out, falsifying compositionality depends on the existence of independently motivated constraints on syntax and on meaning. The absence of such agreed prior constraints explains why many empirical challenges to compositionality have later been countered with compositional analyses, by dint of proposing abstract syntactic ambiguities, previously unobserved lexical ambiguities, and so on. See Hintikka (1983), Partee (1984, 1988), Pelletier (1994), Fischer (2003), among others, for discussion, and see Janssen (1997) and Dever (2006) for overviews of the issues and arguments.

To our knowledge, no-one has yet argued that languages differ in whether they obey compositionality.[49] However, someone has recently argued against the universality of syntactic recursion. Everett (2005: 622) claims that Pirahã is "the only language known without embedding (putting one phrase inside another of the same type or lower level, e.g. noun phrases in noun phrases, sentences in sentences, etc.)."[50] Syntactic recursion ties into the issue of compositionality in the following well-known way. Recursion explains how language users can produce a potentially infinite variety of well-formed sentences from a finite lexicon and computational system. Compositionality (in tandem with syntactic recursion) explains how language users can understand a potentially infinite variety of different sentences, using only a finite lexicon and set of compositional procedures.[51]

This means that a language which lacked recursion would potentially not have need of compositionality. What would such a language look like? Its sentences would not necessarily consist merely of structure-less strings of lexical items; it is possible to construct non-recursive phrase-structure rules (cf. Selkirk's (1980a; 1980b) non-recursive prosodic hierarchy). However, in the absence of recursion, the number of possible sentences would be finite. This would mean that a child could potentially simply memorize lexical items and structures along with their corresponding meanings. (In a sense, every construction could potentially be an idiom.) Such a language would therefore not

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49. Bach (1994, 2002) argues that at the level of word-internal morpheme combination, standard composition rules do not apply. This would imply that polysynthetic languages do a lot of their semantic composition in a different way from non-polysynthetic languages. However, Bach does not claim that polysynthetic languages lack compositionality entirely.

50. In this quote, Everett talks about self-embedding without using the term "recursion" but in the rest of the article and in the follow-up (Everett 2007), it is clear that the claim is that Pirahã has no recursion.

51. While this is a standard argument for compositionality, it has been argued against; see Schiffer (1987), Fischer (2003), etc.

have need of compositionality, and one of the main arguments for its use as a heuristic tool would disappear.

With respect to Everett's (2005; 2007) proposals about Pirahã, we happen to side with Nevins et al. (2007) in not being convinced that this language lacks recursive structures. However, even setting aside the empirical issues discussed by Everett and Nevins et al., we do not find Everett's claims about the recursion issue in Pirahã to be coherent from a semantic point of view. Contrary to the scenario painted in the preceding paragraph, Everett (2007: 7) states that he is not arguing that Pirahã lacks the property of "discrete infinity".[52] His position is that "Pirahã grammar has no phrase structure" (Everett 2007: 4), and that constructions which might appear to involve recursion involve either "parataxis" or "juxtaposition" of items in a string. What do these claims mean for the semantics? If a potentially infinite set of strings can be generated, then the language learner or user cannot rely on memorization to help decipher meaning. But in the absence of any phrase structure, compositionality cannot apply. It is under this scenario somewhat of a mystery how sentences are assigned interpretations.[53,54]

3.2.2.   Constraints on composition principles.   The Principle of Compositionality does not restrict the composition functions themselves. For example (as noted by Pelletier 1994), nothing in principle prevents composition functions from inserting extra elements of meaning – as a case in point, we already mentioned Winter's proposal that there is a default composition rule that interprets tuples via conjunction (Winter 1995). In this section we discuss attempts to place limits on composition principles, as well as arguments that those limits need to be weakened.

One very strong constraint would be what Heim and Kratzer (1998) call "Frege's Conjecture", given in (39).

(39)   The only semantic composition rule is Functional Application (FA).

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52. Although at the same time he claims (Everett 2007: 7) that "There is no infinite language, nor could there be one spoken by humans." This seems to result from a misunderstanding of what we mean when we talk about the potential infinity of human languages.

53. We also do not see even how ungrammatical strings are ruled out, if not by means of phrase structure rules and if a potentially infinite range of strings can be formed. We will leave this issue to the syntacticians however.

54. One would also have to study in what sense the Pirahã situation differs from claims made about "non-configurational" or "pronominal argument" languages, for which hierarchical structure is preserved at the predicate-internal level and in which ordinary composition rules can therefore apply (Baker 1996; Hale 1983; Jelinek 1984).

As observed by Klein and Sag (1985), (39) frees us from having to write semantic translation rules for each syntactic rule, as Montague did in PTQ, allowing instead for type-driven translation.

Bittner (1994) explicitly adopts (39), as part of a proposal for a universally restricted set of semantic operations. Bittner's system allows for type-lifting and lambda-abstraction to adjust types where necessary for FA to apply.[55] However, it does not allow for any language-specific or construction-specific semantic rules.

Although (39) has been challenged, it is clearly viewed as the null hypothesis.[56] The burden of proof is seen to be on those who weaken (39) by arguing for additional composition rules. This means that a system which includes additional composition rules still makes a universal proposal: natural language allows only FA plus whatever new rule is proposed, and no other composition methods.[57]

In the remainder of this section, we briefly introduce two of the most well-known non-FA composition rules: Predicate Modification (Heim and Kratzer 1998) and Restrict (Chung and Ladusaw 2004). Other well-known non-FA composition mechanisms include Event Identification (used to add an external argument; Kratzer 1994) and Function Composition (used, for example, to combine raising verbs like *seem* with their complements, Jacobson 1990).[58]

Heim and Kratzer's (1998) Predicate Modification rule is designed to deal with modifiers such as adjective phrases, prepositional phrases and relative clauses.[59] Heim and Kratzer observe that the most natural denotations for many of these modifiers are of type $\langle e,t \rangle$, and they are therefore unable to compose with NPs by FA. The rule which solves the problem is given in (40) (in a

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55. Heim and Kratzer (1998) add an abstraction rule to their system, which is triggered syncategorematically by variable binders introduced by moved phrases.

56. Pietroski (2005, 2006) argues that there is no functional application and that instead all composition is done via conjunction of predicates. This is achieved at the price of a more complex ontology than usually assumed. Pietroski's project bears some similarity to the system proposed by Hobbs (1983, 1985, 1995, 2003).

57. We should mention at least in passing that there is another famous constraint on composition principles: Kaplan's Prohibition Against Monsters (Kaplan 1989), which has come under intense scrutiny recently, see Schlenker (2003), Anand and Nevins (2004), von Stechow and Zimmermann (2005), among others.

58. We have no space here to discuss the question of whether the thetic/categorical distinction (Kuroda 1972) shows a fundamental distinction in semantic composition or is a matter of two different ways of packaging information at the topic/comment/focus level, see Sasse (1987), von Fintel (1989), Ladusaw (1994), Rosengren (1997), McNally (1998), Sæbø (2007) for discssion.

59. As Paul Pietroski (p.c.) pointed out to us, Higginbotham (1985) also ends up with three modes of composition: theta-linking, theta-binding, and a schema that effectively introduced a Tarski-style treatment of quantification.

version which ignores variable assignments as well as the possibility of uninterpretable sub-trees).

(40)   Predicate Modification (PM)
       If α is a branching node, $\{\beta, \gamma\}$ is the set of α's daughters, and $[\![\beta]\!]$
       and $[\![\gamma]\!]$ are both in $D_{\langle e,t \rangle}$, then $[\![\alpha]\!] = \lambda x \in D_e.[\![\beta]\!](x) = [\![\gamma]\!](x) = 1$.
       (Heim and Kratzer 1998: 65)

Do we really need PM? Heim and Kratzer observe that the alternative – assigning modifiers denotations of type $\langle \langle e,t \rangle, \langle e,t \rangle \rangle$ – requires us to weaken other aspects of the analysis. For example, Frege's Conjecture can be maintained if we assign multiple denotations to those elements which can both modify NP (where they will be of type $\langle \langle e,t \rangle, \langle e,t \rangle \rangle$) and can also appear as main predicates (where they will be of type $\langle e,t \rangle$). Adopting PM, on the other hand, allows us to maintain uniform denotations for such modifiers. Heim and Kratzer also discuss and reject the idea that there are at least some adjectives which *must* be of type $\langle \langle e,t \rangle, \langle e,t \rangle \rangle$ (Heim and Kratzer 1998: 68–73).[60]

    Chung and Ladusaw (2004) propose a new mode of semantic composition called "Restrict", and argue that languages can overtly signal which mode of composition is being used. Restrict is illustrated in (41). A transitive verb (of type $\langle e, \langle e,t \rangle \rangle$) composes directly with an indefinite NP (of type $\langle e,t \rangle$), without type-shifting of either element being necessary. The operation does not decrease semantic valency; a function is returned which still has two unsaturated argument places.

(41)   Restrict $(\lambda y \lambda x[\text{feed}'(y)(x)], \text{dog}')$
       $= \lambda y \lambda x[\text{feed}'(y)(x) \wedge \text{dog}'(y)]$ (Chung and Ladusaw 2004: 5)

These unsaturated argument positions are available for saturation later on, either by FA or by Existential Closure.[61]

    Restrict contrasts with "Specify", a more standard (compound) composition mode whereby an indefinite is shifted to type e by a choice function, and then saturates the relevant argument position via FA. Restrict and Specify give rise to different scope possibilities, due to a principle (Chung and Ladusaw 2004: 11) which requires that all arguments are semantically saturated by the level

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60. In fact, Heim (1999) argues that there are no adjectives of type $\langle \langle e,t \rangle, \langle e,t \rangle \rangle$ at all, except for ordinals and superlatives.

61. Chung and Ladusaw (2006) argue that neither Restrict nor Predicate Modification (which they call 'Modify') should actually have the fully commutative (conjunctive) semantics given in (40) and (41) but instead involve a more asymmetric operation. Space precludes discussion of this interesting idea here.

at which the event argument is closed off. Argument positions which are initially targeted by Restrict must therefore be saturated by the event level, and obligatorily take narrow scope with respect to anything higher than this level.

Chung and Ladusaw apply their analysis to the determiner system of Maori and to object incorporation in Chamorro. For Maori, they propose that the determiner *he* signals composition via Restrict. This correctly predicts that *he*-DPs take narrow scope with respect to negation:

(42) *Kaore he tangata i   waiata mai*
      T.not   a   person   T   sing   to.here
      'No one at all sang.' (But: '* A (particular) person didn't sing.')
                                           (Chung and Ladusaw 2004: 41)

The determiner *tetahi*, in contrast, signals composition via Specify. This correctly predicts that *tetahi*-DPs can take either wide or narrow scope.

(43) *Kaori tetahi tangata i   mahi*
      T.not   a   person   T   work
      'No one worked. / A particular person didn't work.'
                                           (Chung and Ladusaw 2004: 52)

As mentioned above, we can understand Chung and Ladusaw as proposing that natural language composition modes include only those few required to account for Maori and Chamorro. But Chung and Ladusaw make a reverse universal claim as well, namely that if Restrict is present in Maori and Chamorro, it must be universally available. This is illustrated by their assertion (2004: 6) that the ungrammaticality of (44) (which are constructions utilizing Restrict plus subsequent Specify) "is not a semantically interesting property of English":

(44)   a. *John fed (a) dog Fido.
       b. *John dog-fed Fido.

The idea is that since semantic composition cannot vary cross-linguistically, the ill-formedness of (44) must be a syntactic matter. Of course, this issue is at least partly empirical. That is, it is not logically inconceivable that (44) are bad because of the (semantically interesting) absence in English of Restrict.

One question for Chung and Ladusaw's system concerns the apparent cross-linguistic rarity of strong evidence for the existence of Restrict. For example, Matthewson (2007b) argues that Salish determiner systems are compatible with, but do not provide language-internal evidence for, Chung and Ladusaw's account. A similar situation appears to hold with incorporation: West Greenlandic is compatible with Chung and Ladusaw's analysis, but does not display the Chamorro facts which motivate Restrict. The question then arises of why, if

Restrict is universally available, few phenomena in few languages show strong evidence for its existence.

In conclusion to this section: we have seen that, as one might have expected, when formal semanticists study cross-linguistic patterns, they focus in on the central question of the field: how are meanings composed? So, we have seen many exciting topics where results are starting to come in and where further questions abound. Clearly, here is where the action is as far as possible semantic universals are concerned.[62]

4.   Universals in pragmatics

The last component of meaning that we are going to inspect for universality and variation is pragmatics. We will concentrate on two particular questions: (i) is presupposition a universal phenomenon? (ii) are the Gricean maxims of rational conversation obeyed universally?

4.1.   Universal presuppositions

Probably almost every semanticist implicitly believes the statement in (45):

(45)   All languages have presuppositions.

There are many competing analyses of presupposition, and this is not the place to decide between them. Instead, let us re-phrase (45) a bit more precisely, but still theory-neutrally, as in (46):[63]

(46)   All languages allow their speakers to express aspects of meaning which
      a.   are not asserted, but somehow taken for granted,
      b.   impose some constraints on when an utterance is felicitous, and
      c.   project through certain entailment-canceling operators.

Unfortunately, since the literature devoted to presupposition concentrates almost exclusively on English, we have little empirical evidence either for or

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62. Among exciting ideas in this field, we should mention Krifka's (2006) recent proposal that the pervasive presence of topic/comment structuring in natural language is connected to the evolution of bimanual coordination in hominids.

63. Beaver (1997: 941–942) notes that while there is no single definition of "presupposition" which will satisfy everyone, there is at least an accepted set of basic data which we agree are covered by the term.

against (45)/(46).[64] Here we will review some preliminary evidence suggesting that some languages might possess only elements with properties (46a) and (46c), not (46b).

Why would we assume that every language has presuppositions? One obvious reason is that if, as E.L. Keenan (1974) maintains, effability requires the preservation of the division between asserted and presuppositional content, effability entails (45); see Section 1.2 above. However, it seems clear that this strong version of effability cannot be maintained, since not all languages possess exactly the same presupposition triggers. For example, Matthewson (1998) argues that (along with all other languages of the Salish family), St'át'imcets lacks any determiners which presuppose familiarity or uniqueness. (47)–(49) show that the same determiner (*ti. . . a* or *ta. . . a*, depending on dialect) is used both in novel and familiar contexts, and in unique as well as non-unique contexts.

(47) a. *húy'-lhkan*   *ptakwlh,* *ptákwlh-min* *lts7a* [*ti*
    going.to-1SG.SUBJ tell.story tell.story-APPL DEIC [DET
    smém'lhats-a] . . .
    girl-DET]
    'I am going to tell a legend, a legend about [a girl]$_i$ . . .'

  b. *wa7* *ku7* *ílal* *láti7* [*ti* *smém'lhats-a*]
    IMPF REPORT cry DEIC [DET girl-DET]
    '[The girl]$_i$ was crying there.' (van Eijk and Williams 1981: 19)

(48) *Context: There is one puppy in the room. It is sleeping.*
  cw7áoz-as *kw-á-su*   *wenácw-ts!* *wa7* *guy't*
  NEG-3CONJ DET-IMPF-2SG.POSS true-mouth IMPF sleep
  [*ta* *sqéqx7-a*]
  [DET puppy-DET]
  'Be quiet! The puppy is sleeping.'

(49) *Context: There are five puppies in the room. One of them is sleeping.*
  cw7áoz-as *kw-á-su*   *wenácw-ts!* *wa7* *guy't*
  NEG-3CONJ DET-IMPF-2SG.POSS true-mouth IMPF sleep
  [*ta* *sqéqx7-a*]
  [DET puppy-DET]
  'Be quiet! A puppy is sleeping.'

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64. Levinson and Annamalai (1992) is a notable exception, dealing with presuppositions in Tamil. Levinson and Annamalai (1992: 229–231) in fact propose a version of (45):

 (i) "it is at least likely that presuppositional phenomena are associated with certain semantic categories by some language universal mechanisms."

Matthewson proposes a semantic parameter which states that some languages lack presuppositional determiners:

(50)   Common Ground Parameter (Matthewson 1998)
        Determiners may access the common ground of the discourse:
        Yes: English, . . .
        No: Salish, . . .

St'át'imcets also does not possess presuppositional *it*-clefts, as shown by Davis et al. (2004). (51) shows that clefts are felicitous in discourse-initial contexts and therefore do not presuppose existence[65], and (52) shows that exhaustivity is a cancelable implicature, unlike in the English gloss for this sentence.

(51)   *na   s-pála7-s-a,   nilh   káti7   ta*
        DET   NOM-one-3SG.POSS-DET   FOC   DEIC   DET
        nk'yáp-a   t'ak
        coyote-DET   go.along
        'Once upon a time, it was a coyote who was going along.'

(52)   *nilh   i   sk'wemk'úk'wmi7t-a   q'7-ál'men,   múta7*
        FOC   DET.PL   children-DET   eat-want   and
        i   lalíl'tem-a   t'it
        DET.PL   adult-DET   also
        'It's the children who are hungry, and also the adults.'

The cleft facts are derivable from the determiner facts, under a recent analysis (Hedberg 2000; Percus 1997) according to which clefts contain concealed definite descriptions. If St'át'imcets lacks definite determiners, and if its clefts therefore contain concealed indefinite determiners instead, we predict the absence of presuppositions in clefts. We can therefore maintain the claim that the absence of presuppositions in these languages is localized to the particular domain of one functional category, *D*.

However, the results so far already raise a serious challenge for effability, since St'át'imcets has no way to convey all aspects of the meaning of an English sentence containing a definite determiner or an *it*-cleft, at least not while preserving the assertion/presupposition division. This in turn nullifies one theoretical reason for believing in (45)/(46), and raises the question: if languages can lack some presupposition triggers, could a language get by without any presuppositions at all?

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65. Note that these examples are distinct from the kind of discourse-initial *it*-clefts in English studied by Delin (1992).

What would we predict for such a language? Would we expect that the language could possess items corresponding to English presupposition triggers (e.g., words for *know, stop, also, again, too*), but that these words would simply not convey those aspects of meaning which in English are presuppositions? Or that those aspects of meaning would behave differently from English, e.g. in not projecting through operators? Or would the language have to lack such words entirely?

The answer to these questions depends in part on one's theory of presupposition. There is one line of thought which says that presuppositions are not conventionally attached to particular lexical triggers; see e.g., Levinson and Annamalai (1992), Simons (2006). According to this idea, many (or perhaps all) presuppositions are non-detachable, meaning that any two items with the same truth-conditional meaning will give rise to the same presuppositions. It follows from this that the presupposition need not be written into the lexical entry as a definedness condition. For example, Levinson and Annamalai (1992) argue that it is not an accident that *regret* is presuppositional – rather, it follows automatically from its truth-conditional contribution. Similarly, Simons (2006) argues that a large class of presuppositions cannot be conventional, since we can't imagine them being absent. We could not, for example, invent a new change-of-state verb similar to 'stop' but which lacks the presupposition that the state held immediately prior to the reference time. Simons (2006: 6) states that "(as far as I am aware) synonymous items in these classes *across languages* also share the same presuppositions" (emphasis original). This would predict that a language which lacked presuppositions would have to lack items corresponding to all English presupposition 'triggers'.

On the other hand, we are not convinced that elements like 'stop' or 'become a US citizen' (Simons 2006: 5) must necessarily be pragmatically asymmetric in all languages. It is possible that there are languages where elements corresponding to these English presupposition triggers express the relevant aspects of meaning as part of their assertions instead.

With this background in mind, let us look at some more St'át'imcets data, this time involving words for 'more', 'stop', 'too', and 'again'. It is important that these items are involved, because at least 'too' and 'again' fall into the class of items which seem somehow more "strongly" presuppositional than other triggers. See, for example, Abusch (2005) for the distinction between "soft" and "hard" presupposition triggers: soft triggers include 'stop', 'know', and 'win', while hard triggers include 'also', 'even', 'again', 'too', NPI 'either', and *it*-clefts. Simons (2006) notes that *too*, *again*, and *even* seem to have no purpose other than the presupposition (and therefore do have the content of the presupposition conventionalized, although not the fact that that content is a presupposition). Zeevat (2003) argues that *too*, *again*, *instead* and *even* differ from other presupposition triggers in that they cannot be accommodated; see

also von Fintel (2000) for this claim about *too*.

The data to follow (taken from Matthewson 2006a) are intended to show that the closest St'át'imcets equivalents of English presupposition triggers do not place the same requirements on the state of the common ground as the English ones do. To diagnose this difference, von Fintel's (2004) 'Hey, wait a minute!' test was used. The test works as follows. A presupposition which is not in the common ground at the time of utterance can be challenged by 'Hey, wait a minute!' (or other similar responses). In contrast, an assertion which is not in the common ground cannot be challenged in this way. This is shown in (53), from von Fintel (2004: 271), for the existence presupposition of *the*.

(53)   A: The mathematician who proved Goldbach's Conjecture is a woman.

B: Hey, wait a minute. I had no idea that someone proved Goldbach's Conjecture.

B′: #Hey, wait a minute. I had no idea that that was a woman.

The 'Hey, wait a minute!' test is the best way we know of to test for presuppositions in a fieldwork context. Its usefulness lies in the fact that it directly tests for (in)felicity of an utterance in a particular discourse context, without resorting to meta-linguistic judgments on the part of the consultant (of the form "sentence p takes sentence q for granted"), or about sentences containing operators. [66] And the test works very well for English; naïve English speakers not only provide judgments in accordance with (53), they also spontaneously offer 'Hey, wait a minute!' responses in real-life situations; see Matthewson (2006a) for examples.

Given this, it is striking that a range of different fieldwork strategies have failed to elicit any 'Hey, wait a minute!' response from St'át'imcets speakers to presupposition failures. Examples are given in (54)–(58). The B utterances in each case are the consultants' spontaneous responses to A.

(54)   Context (social, not an elicitation context): B has just walked into A's house and there has been no prior conversation apart from greetings.

A: *wá7-lhkacw   ha   xát'-min'   ku   hu7   ku   tih*
IMPF-2SG.SUBJ YNQ   want-APPL DET   more   DET   tea
'Would you like some more tea?'

B: *iy*
'Yes.'

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(55)   Context: Interlocutors all know that Henry is not a millionaire.

    A: *t'cum   múta7 k   Henry l-ta   lottery-ha*
        win(INTR) again DET Henry in-DET lottery-DET
        'Henry won the lottery again.'

    B: *o, áma*
        oh good

(56)   Context: B has been a teetotaler for several decades.

    A: *xat'-min'-lhkácw   ha   ku   hu7   ku   qvl*
        want-APPL-2SG.SUBJ YNQ DET more DET bad
        s-7úqwa7
        NOM-drink
        'Do you want some more alcohol?'

    B: *káti7. qyáx-kan   kélh t'u7*
        DEIC drunk-1SG.SUBJ FUT just
        'No way. I'll get drunk.' (laughs)

(57)   Context: B has no knowledge of anyone planning a trip to Paris.

    A: *nas t'it áku7 Paris-a   kw   s-Haleni*
        go   also DEIC Paris-DET DET NOM-Henry
        lh-klísmes-as
        HYP-Christmas-3CONJ
        'Henry is also going to Paris at Christmas.'[67]

    B: *o áma*
        oh good

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67. A reviewer asks whether *t'it* in (57)–(58) really corresponds to English *too* and 'associates' with the subject, or whether it could correspond instead to the easier-to-accommodate 'list' *also*, giving readings such as 'Henry just finished his book, he's taking the week off, and he's also going to Paris at Christmas'. *T'it* certainly *allows* the subject-associating reading, as demonstrated by VP-ellipsis:

(i)   *cuz'   nas áku7 Paris-a   kw   s-Henry,   múta7 cuz'   t'it*
       going.to go DEIC Paris-DET DET NOM-Henry and   going.to too
       kw   s-Lisa
       DET NOM-Lisa
       'Henry is going to Paris, and Lisa is too.'

However, *t'it* also allows the 'list' reading, and future fieldwork will need to eliminate that possible reading from examples such as (57)–(58). At the same time, however, we note that English *too* also allows a 'list' reading, and nevertheless is perceived as infelicitous in parallel examples. (Imagine the English translation of (57) in a discourse-initial context, for example.)

(58)   Context: No prior discussion of anyone being in jail.

    A: *wá7 t'it l-ti   gélgel-a   tsitcw k   Lisa*
        be   also in-DET strong-DET house DET Lisa
        'Lisa is also in jail.'

    B: *stam' ku   s-záyten-s*
        what   DET   NOM-business-3POSS
        'What did she do?'

One might suspect that consultants fail to give "hey, wait a minute!" responses in (54)–(58) because they are accustomed, in a fieldwork context, to being asked to imagine all kinds of situations.[68] This is certainly a potential interfering factor, and we acknowledge that caution is required whenever discourse (in)felicity is tested via formal elicitation. However, we are fairly confident of our results, for the following reasons. First, failed presuppositions were tested outside an elicitation context as far as possible (as in (54)). Second, the same consultants freely challenge several different types of pragmatically anomalous utterances, involving unclear pronoun reference, unclear DP reference, or pragmatically odd situations (see also discussion below). Finally, the same methodology has been replicated with speakers of other languages (English, Portuguese, Spanish) with strikingly different results – in particular with an abundance of "hey, wait a minute!" responses.

Consultants will sometimes challenge failed presuppositions, but they use exactly the same constructions to respond to failed presuppositions in St'át'imcets as they do to assertions they believe to be false. In (59), the B and C responses challenge the presupposition, but the B′ and C′ responses challenge the asserted material in an exactly parallel manner. Since the entire point of the 'Hey, wait a minute!' test is that it distinguishes presuppositions from assertions, (59) reinforces the claim that St'át'imcets lacks a 'Hey, wait a minute!' effect.

(59)   A: *plan   tsukw k-wa-s   mán'c-em   kw*
        already stop   DET-IMPF-3POSS smoke-INTR DET
        s-Bob
        NOM-Bob
        'Bob stopped smoking.'

    B: *aoz   t'u7 kw-en-s-wá   zwát-en   kw*
        NEG   just   DET-1SG.POSS-NOM-IMPF know-DIR DET
        s-tu7   mán'c-em   s-Bob
        NOM-then smoke-INTR NOM-Bob
        'I didn't know Bob smoked.'

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68. This was suggested to us by a reviewer.

> B′: *aoz*   *t'u7* *kw-en-s-wá*   *zwát-en*   *kw*
>   NEG   just   DET-1SG.POSS-NOM-IMPF   know-DIR   DET
>   *s-tsukw-s*
>   NOM-stop-3POSS
>   'I didn't know he stopped.'
> C: *wa7*   *ha*   *tu7* *mán'c-em*   *kw*   *s-Bob*
>   IMPF   YNQ   then   smoke-INTR   DET   NOM-Bob
>   'Did Bob used to smoke?'
> C′: *tsukw* *ha*   *tu7*
>   stop   YNQ   then
>   'Did he stop?'

What could be the source of this cross-linguistic difference? Matthewson (2006a) argues against a cultural explanation (i.e., that St'át'imcets culture prohibits challenges of unclear utterances) by showing that St'át'imcets speakers are willing to offer challenge responses in cases of unclear DP or pronoun reference, or when an utterance is contradictory. If we assume that the source of the difference is linguistic, we have to conclude either (60a) or (60b):

(60)   a.   The St'át'imcets elements corresponding to *stop, again, more, too* are not presuppositional.
        b.   The St'át'imcets elements are presuppositional, but presuppositions do not enforce exactly the same common ground/context update constraints in St'át'imcets as they do in English.

(60a) would entail that presuppositions do not follow without stipulation from truth-conditional content; e.g., it could not be an intrinsic result of the meaning of *stop* that it presuppose that the relevant state/event held before. Matthewson (2006a) concludes (60b), based on some indications that we can still detect projection effects in St'át'imcets. Thus, while consultants never give 'Hey, wait a minute!' responses, they will often give meta-linguistic judgments that certain sentences "should not be said" in certain contexts; these judgments remain even when the potential presupposition trigger is embedded. In (61), for example, the consultant judges that one "should not say" the sentence if the hearer has not yet eaten any salmon. This suggests that (61) does not mean 'if it is the case that you have eaten salmon recently and you want some more, take some'.[69]

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69. Example (61) incidentally reinforces the usefulness of the 'Hey, wait a minute!' test, as opposed to merely asking a consultant whether a sentence *p* 'takes for granted' a sentence *q*. The latter strategy would obscure the St'át'imcets/English difference with respect to the discourse effects of presupposition failure.

(61)   *lh-xát'-min'-acw*   *múta7   ku   ts'wan,*
       HYP-want-APPL-2SG.CONJ   more   DET   wind.dried.salmon
       kwan   láta7
       take(DIR)   DEIC
       'If you want some more wind-dried salmon, take some.'

Matthewson (2006a) proposes that languages vary in whether they have Stalnaker-type presuppositions – which are restricted according to the state of the common ground and which therefore give rise to 'Hey, wait a minute!' responses – or Gauker (1998, 2003)-type presuppositions. Under Gauker's analasis, presuppositions are not shared assumptions, but represent merely the speaker's own "take" on what the relevant propositions for the purposes of the discourse are. Gauker's proposal predicts no 'Hey, wait a minute!' responses, since there is no expectation that the speaker's presuppositions belong to the hearer's set of assumptions. (The effect is as if accommodation is available everywhere, although there is no actual accommodation process taking place.) As pointed out by von Fintel (2000), Gauker's analysis over-generates felicitous discourses for English, predicting acceptable sequences which in fact give rise to accommodation failure. However, Gauker's predictions – while incorrect for English – are exactly what we want for St'át'imcets, as argued above.[70]

An alternative possible analysis of St'át'imcets is that the language lacks what von Fintel (2004) has called Stalnaker's "Bridging Principle". This principle says that semantic presuppositions – encoded in the lexical meaning of presupposition triggers – automatically become pragmatic presuppositions – felicity conditions on the common ground. The absence of such a principle would account for the difference in 'Hey, wait a minute!' effects between the two languages.

We can therefore summarize as follows. There are several proposals in the literature according to which some (Abusch) or all (Simons, Gauker) of what are traditionally called presuppositions do not place the restrictions on the common ground which are standardly assumed in a Stalnakerian approach. (Going back to (46) above, this means that Simons and Gauker claim that in English, presuppositions lack property (46b)). In St'át'imcets, even the 'hard' presupposition triggers seem to lack common ground effects, as detected by the ab-

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70. Simons's analysis also fails to predict 'Hey, wait a minute!' responses. According to Simons (2006: 26), "*p* is a presupposition of an utterance *U* iff (i) it is not part of the speaker's primary intention to convey *p* and (ii) the interpreter of *U* must take the speaker of *U* to accept *p* in order to make sense of *U*." As noted by Simons (2001: 445, cited in Abusch 2005), this means that at least some presuppositions "are neither required nor expected to be entailed by the common ground", and in fact are basically conversational implicatures. As with Gauker's analysis, this may give the correct results for St'át'imcets, but cannot be adopted for English without losing the ability to capture the English-St'át'imcets difference.

sence of 'Hey, wait a minute!' responses even with these triggers. This means that whatever the correct analysis of presupposition, it must be parameterized so as to predict the different discourse effects in English and St'át'imcets. We therefore tentatively conclude that all languages *do* have presuppositions, but how those presuppositions behave may differ from language to language. We also observe, as noted earlier in this section, that even if all languages possess presuppositions, there is cross-linguistic variation in whether or not certain elements (such as determiners) are presuppositional. The St'át'imcets determiner and cleft data therefore support E.L. Keenan's (1974) proposal that translatability cannot be maintained in its strongest form.

4.2.   *Grice everywhere*

Grice (1967) argued that purposeful language use is governed by what he called the Cooperative Principle:

(62)   The Cooperative Principle (CP) (Grice 1967: 26)
        Make your conversational contribution such as is required at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged.

Grice characterizes this principle as a way of spelling out what it means to act rationally while engaged in purposeful language use. And in as much as we assume that language communities do not differ in whether or not they are rational, one expects that the CP is universal. Grice further specified some families of maxims attendant to the CP that have been used extensively in pragmatic research since then: Quality, Quantity, Relevance, Manner. Again, since these are simply more specific ways of spelling out what it means to be rational in one's language use, we expect the maxims to be universal.

This universality (which Grice actually never claimed explicitly) leads us to expect that there is cross-linguistic uniformity in the kinds of inferences people draw based on the assumption that their partners in conversation (try to) act rationally – inferences that Grice called *conversational implicatures*. As Prince (1982: 7) says, for example, we expect people to interpret B's answer in (63) as an attempt to make a relevant contribution to answering the question asked by A:

(63)   A: How can I get to Penn?
        B: There's a bus that stops in front of City Hall.

The natural inference that A would draw here is that B thinks that (it is possible that) the bus that stops in front of City Hall goes (at least part way) to Penn. It

would be astonishing to find a language community in which there wasn't this kind of inferencing (modulo the existence of buses, of course).

Implicature calculations, especially of quantity implicatures, often kick off as follows: *S* said ϕ, there is an obvious alternative ψ that *S* could have said, it must have been more rational to say ϕ than ψ, how can that be? Then, one considers which of the maxims saying ψ would have violated. So, when in Grice's (1967: 32) example, B answers *Somewhere in the South of France* to A's question *Where does C live?*, the fact that B didn't give a more precise answer is reasonably traced back to B's not knowing where C lives.

In this kind of machinery, there is in fact some space for cross-linguistic variation. As discussed by Matsumoto (1995), quantity implicatures do not always work the way just sketched. Apart from the speaker not being in possession of the relevant piece of information, another reason (among others that Matsumoto discusses) for not giving more information than one actually does is that the extra information would go beyond the expected level of specificity. Consider the following example:

(64)   This is Andrew's brother Peter. (Matsumoto 1995: 30, ex. (10))

We would not infer here that the speaker does not know whether Peter is Andrew's older brother or younger brother. Matsumoto proposes that the reason for the absence of a quantity implicature in (64) is that *older brother* and *younger brother* are not basic level sibling terms in English. The prediction is that this may be different in other languages. And it is; consider the following example from Japanese:

(65)   *Kochira wa   Takashi-kun no   kyoodai no   Michio-kun*
        this   TOP Takashi-Mr. GEN   brother GEN   Michio-Mr.
        desu
        COP
        'Michio is Takashi's brother.' (Matsumoto 1995: 30–31, ex. (11))

Japanese has basic level sibling terms that incorporate relative seniority (*ani* 'older brother', *otooto* 'younger brother') and it also has terms that are not basic level sibling terms (like English *sibling* but with sex information: *kyoodai* 'brother'). Consequently, (65) does give rise to a quantity implicature, in contrast to the English (64): the speaker of (65) is inferred to not know whether Michio is Takashi's older brother or younger brother.

It should be clear that this cross-linguistic difference is not a difference in the applicability of the Gricean machinery but a difference in what the relevant alternatives to a given utterance are and thus what kinds of rationality comparisons need to be calculated.

We know of only one attempt in the literature to argue against the universality of Gricean pragmatics. E.O. Keenan (1974) claims that speakers of Malagasy (spoken on Madagascar) do not obey the Quantity maxim:

> Interlocutors regularly violate this maxim. They regularly provide less information than is required by their conversational partner, even though they have access to the necessary information. If A asks B 'Where is your mother?' and B responds 'She is either in the house or at the market', B's utterance is not usually taken to imply that B is unable to provide more specific information needed by the hearer. The implicature is not made, because the expectation that speakers will satisfy informational needs is not a basic norm.   (E.O. Keenan 1974: 258)

Keenan explains that there are two reasons for this regular withholding of extra information:

> (i) "New information is a rare commodity ... Information that is not already available to the public is highly sought after. If one manages to gain access to new information, one is reluctant to reveal it. As long as it is known that one has that information and others do not have it, one has some prestige ... [I]nterlocutors are generally aware of the reluctance to give up requested information. They expect the response of the addressee to be less than satisfactory. Normally, if the information requested is not immediately provided, the two interlocutors enter into a series of exchanges whereby the one tries to eke out the new information from the other."

> (ii) "Individuals regularly avoid making explicit statements about beliefs and activities. They do not want to be responsible for the information communicated."

In response to Keenan, Prince (1982) points out that A in Keenan's example dialogue actually does clearly employ the Gricean implicature mechanism, including the Quantity maxim: A reasons that B could have given more information but that B must have had rational reasons not to do so. Green (1990: 419) proposes that

> being cooperative in making one's contribution "such as is required at the stage at which it occurs" to accomplish one's goal(s) may involve following other principles as well, such as
>
> A. Assigning responsibility for a state of affairs that could be construed as undesirable is counterproductive.
> B. Making the addressee disinclined to cooperate (e.g., by making the addressee uncomfortable) is counterproductive.
> C. Maintaining social advantage is useful.

> What counts as an undesirable state of affairs, as making someone uncomfortable, as constituting social advantage, are of course defined relative to a particular culture.

Thus we expect variation in the details of specific Gricean calculations but not in the shape of the machinery. We concur with Green when she writes that "it would astonish me to find a culture in which Grice's maxims were not routinely observed, and required for the interpretation of communicative intentions, and all other things being equal, routinely exploited to create implicature" (Green 1990: 419).[71]

5.   Conclusion

Let's summarize the results of our survey of semantic universals. We discussed three major areas: (i) lexical/content morphemes, (ii) semantic "glue" (functional morphemes and composition principles), and (iii) pragmatics.

First, constraints on the lexicon. We found no useful list of meanings which are universally lexicalized, but we did find many proposed constraints on the semantics of content morphemes, some of which may be amenable to non-linguistic explanations. We argued against a universal primitive set of verbal classes, although we observed that variation in this area is not without limit. We also reported on the ongoing controversy surrounding universal constraints on semantic types.

Next, semantic glue. We argued that all languages need glue, in the form of functional morphemes and semantic composition principles. We agreed with the widespread idea that there are only a small number of universally available composition principles, including Functional Application and a few others. Work is still ongoing as to exactly what that set of principles consists of. We proposed that functional morphemes are universally of higher types, and are subject to strong constraints provided by UG (for example, conservativity). However, we did find evidence that languages differ in their inventories of functional categories, and in the distinctions encoded by their functional morphemes.

Finally, pragmatics. We suggested that languages may differ in the discourse effects of their presuppositions, but that speakers of all languages make use of Gricean machinery. Cultural differences may, however, influence the content of the conversational implicatures which arise.

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71. It is thus not surprising to us to see that Levinson (2000) found that Gricean calculations are useful for explaining facts about the meaning of various expressions in Yélî Dnye, the language of Rossel Island in the Pacific Ocean. Similarly, Fernald et al. (2000b) argue that Gricean implicatures arise in Navajo just as they do in English.

A recurring theme throughout our discussion was effability. On the one hand, we observed – following E.O. Keenan (1974) – that effability does not hold in its strongest form, since languages differ in their ability to express certain aspects of meaning as presuppositions. On the other hand, we found that languages often express strikingly similar truth-conditions, in spite of non-trivial differences in lexical semantics or syntax. We suggested that it may therefore be fruitful to investigate the validity of "purely semantic" universals, as opposed to syntax-semantics universals.

Another recurring theme was how much work there still is to be done. The relatively young field of cross-linguistic formal semantics is now at the stage where universals research can really take off. We hope that it will.

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