Methodological Issues in Cross-Language Color Naming[1].

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Abstract

The universals and evolution (UE) model in cross-language color naming research, stemming from Berlin and Kay (1969) and most recently embodied in Kay and Maffi (1999) has been criticized on the grounds, among others, (1) that many languages contain words which express both color and non-color properties, (2) that in many languages words which express color properties do not form a coherent morpho-syntactic class, and (3) that the purported findings of this tradition of research are artifacts of a biased method of investigation. Each of these charges is answered.

Keywords: color, color terms, lexical semantics, language evolution, emergence hypothesis

Applying the color naming procedures of Lenneberg and Roberts (1956) to speakers of twenty languages in the San Francisco area and supplementing

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[1]I would like to express my appreciation to David Wilkins for his comments and for the use of his data and analysis regarding Arrernte word associations; these are presented in Figure 1. I would also like to thank Luisa Maffi for comments on an earlier draft.

these data with additional reports on the basic color lexicons of seventy-eight languages from the literature, Berlin and Kay (1969: 4f) advanced the following two hypotheses:

> In sum, our two major findings indicate that [1] the referents for the basic color terms of all languages appear to be drawn from a set of eleven universal perceptual categories, and [2] these categories become encoded in the history of a given language in a partially fixed order.

Following the appearance of these hypotheses, which ran counter to then standard Whorfian doctrine, a number of field studies were undertaken to subject them to field tests on monolingual speakers in their native surroundings (insofar as possible)[2]. Based on the results of these and other studies, there have been a number of revisions of the detailed model presented in Berlin and Kay (1969). The principle points of revision have been (1) addition of the idea of successively refined partitions of the perceptual color space to the orginal proposal of successive encodings of focal colors, (2) replacement of the idea of exactly eleven universal perceptual categories with the idea of the six Hering primaries (black, white, red, green, yellow, blue) along with a restricted subset of their possible unions and intersections, (3) recognition that some languages have terms spanning hue and achromatic categories – e.g., a term naming the union of black, green and blue, or of white, red and yellow, (4) recognition that there are really two –

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[2] For example, Berlin and Berlin (1975), Dougherty (1975, 1977), Hage and Hawkes (1975), Harkness (1973), Heider (1972a, 1972b), Heider and Olivier (1972), Heinrich (1972), Kuschel and Monberg (1974), MacLaury (1986, 1987, 1997), Maffi (1990b), Monberg (1971), Senft (1987), Snow (1971), Turton (1978, 1980).

occasionally overlapping but mostly successive – evolutionary sequences: (i) the division of the disjunctive categories, including those discussed under point (3), into the six Hering primaries and (ii) subsequent naming of the intersective categories, like pink, purple, brown, orange and gray, (5) full acceptance of the fact, entertained tentatively in Berlin and Kay (1969), that there is probably nothing magic about the number eleven as an upper limit on the number of basic color terms a language may possess, and (6) recognition of the "Emergence Hypothesis" according to which not all languages have a complete set of basic color terms, i.e., a set of lexemes of abstract color denotation whose denotata jointly exhaust the perceptual color space.[3]

There have appeared recently some objections to the methodology of the World Color Survey (WCS) and related studies in the tradition of Berlin and Kay (1969). I will review here what I consider to be the most important of these objections, attempting to sort out useful from invalid criticisms. The principle authors of these critiques are John Lucy, John Lyons, the team of B. Saunders and J. van Brakel and Anna Wierzbicka. I have dealt elswhere with

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[3] The principal works embodying these revisions are Berlin and Berlin (1975), Kay (1975), Kay and McDaniel (1978), Kay, Berlin and Merrifield (1991), Kay, Berlin, Maffi, and Merrifield (1997), and Kay and Maffi (1999). As pointed out in Kay and Maffi (1999), Kay and McDaniel (1978) helped disseminate an error in interpreting the individual cell responses recorded by De Valois *et al.* (1966) in the lateral geniculate nucleus (LGN) of macaque monkeys as providing the physiological locus for the Hering opponent hue responses. Accordingly, Kay and McDaniel misleadingly referred to the red, green, yellow and blue sensations as "fundamental neural response categories." The two main reasons for the rejection of the early interpretation of the macaque LGN data as providing the physiology of red, green, yellow, blue are, first, that the cross-over points of the wavelength oppponent cells were found to be in the wrong places to produce the R,G,Y,B sensations (Derrington *et al.* 1984) and, secondly, that the firing patterns observed by De Valois *et al.* provide no support for the psychophysically established short-wavelength red response (for further discussion, see Abramov 1997: 107).

the arguments of Lyons (Kay 1997) and will do so here only summarily. The criticisms of Saunders and van Brakel that I can understand are expressed equally clearly by Lucy. Berlin and I have already replied in print to Saunders and van Brakel's most recent criticisms (Saunders and van Brakel 1997, Kay and Berlin 1997). Consequently, I will not deal with the Saunders and van Brakel critiques other than indirectly through my replies to Lucy.[4]

Lucy's principal and most recent exposition of his objections to the methodology of the WCS, and to the whole UE tradition of research, are contained in Lucy (1997). In that paper, Lucy makes three main points:

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[4] Apart from their flamboyant contention that current psychophysical and psychophysiological vision theory is in total disarray as regards color perception, Saunders and van Brakel complain that Berlin and Kay (1969) *assumed* a vision-language correlation at the outset and set up their investigation so that it would produce the false appearance of having *discovered* one. Examination of the Saunders and van Brakel text uncovers no evidence supporting this allegation. Examination of the Berlin and Kay text and the circumstances under which it was produced shows the opposite. In particular, Berlin and Kay employed both the stimuli and the elicitation methods of Lenneberg and Roberts, who conducted their investigation in attempt to establish Whorfian effects in color vocabulary. Saunders and van Brakel, after claiming that the UE empirical findings are merely a methodological artifact, nevertheless go on to provide an alternative explanation for these findings! That is the explanation offered by Tornay (1978: xxxi), according to which universals in color term semantics are really the result of "the progressive domination of the West" (Saunders and van Brakel 1997:198). Berlin and Kay (1969) explicitly pointed out nine cases in which expansion of a color vocabulary involved borrowing a term from a major written language or a language influenced by a major written language and detailed studies in the UE tradition have documented such influence in detail (e.g., Dougherty 1975, 1977). While acknowledging the frequent influence of colonial languages on the unwritten languages with which they come in contact, Kay and Berlin (1997) point out that the widespread existence in unwritten languages of terms spanning green and blue and of terms spanning red and yellow cannot possibly reflect anything existing in European languages as recently as the colonial era.

Lucy's point 1: *In many or all languages, words that denote color properties also denote non-color properties.* (One of **Wierzbicka's** (1990) two main criticisms will be treated under this point as well.)[5]

Lucy's point 2: *In many or all languages, words that serve to express color properties do not constitute a morpho-syntactic class.*

Lucy's point 3: *The UE findings are a methodological artifact.*

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[5]Wierzbicka's other major criticism is that she believes that "...color PERCEPTION has very little to do with the question of color CONCEPTUALIZATION...Whatever happens in the retina, and in the brain, it is not directly reflected in language" (1990: 102-103, emphasis in original). I suppose we must interpret this passage generously to mean that whatever happens in the *visual areas* of the brain is not reflected in language. Presumably, Wierzbicka does not intend to claim that language is not itself represented anywhere in the brain. Rather, we must interpret her intent to be that, physiologically speaking, higher, cognitive, brain centers or processes mediate between visual inputs and the color categories expressed in languages. But if this is so, then these higher, cognitive centers governing linguistic categorization appear to be operative in some closely related species. Wierzbicka does not discuss the literature showing not only human-like color discrimination but also human-like color categorization to be closely approximated by Old World primates and prelinguistic human infants, but not by New World primates, e.g., Bornstein et al. (1976), DeValois et al. (1974), Essock (1977), Grether (1939), Matsuzawa (1985), Sandell et al. (1979). This literature suggests, for example, that Old World monkeys, chimpanzees, and young human infants may have the *categories* red, yellow, green and blue, while New World monkeys do not. Wierzbicka does not explain the distinction she makes emphatically between perception and conceptualization, but if the categories named by the English words *red, yellow, green* and *blue* reflect Wierzbickian conceptualization, then Chimpanzees, old world monkeys and human infants may also be also capable of Wierzbickian conceptualization. Wierzbicka's claim that the color categories found in human languages reflect conceptualization rather than perception would seem to predict that categories such as red, yellow, green and blue are absent in species not possessing language, but the facts appear to be otherwise.

Response to Lucy's point 1

Lucy's prime example of the encoding of non-color information along with color information is taken from Conklin's classic description of Hanunóo color words (Conklin 1955). That description made it clear that the word *(ma)rara* , covering "maroon, red, orange, yellow and mixtures in which these qualities are seen to predominate" may also express the property of "dryness or desiccation" and the word *(ma)latuy*, covering "light green and mixtures of green, yellow, and light brown" may also express the property of "wetness or freshness." Lucy quotes a favorite sentence of critics of the UE model: "A shiny, wet, brown-colored section of newly-cut bamboo is *malatuy* not *marara* " (Conklin 1955; 190 quoted in Lucy 1997: 324). Thus, *(ma)latuy* can mean something like English *green* in the sense of 'colored green' and it can mean something like English *green* in the sense of 'unripe, immature'. In English, a green twig may be brown in color and an apple which is green in color may or may not be ripe.[6] We don't suppose that these facts constitute a problem for the claim that English contains a basic color term *green*, because we take for granted that the color sense and the 'unripe' sense are just that, two distinct senses. But how do we know that this isn't also the case in Hanunóo? Conklin tells us that *latuy* can by used to predicate the property of green color and that it can be used to predicate the property of succulence. He does not consider the question whether *latuy* is vague with regard to the notions 'green color' and 'succulent' or polysemous. Lucy assumes the

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[6]A recently attested kitchen conversation:

She (cutting into a lemon): This is the greenest lemon I've ever seen.

He (craning his neck): Do you mean green-colored or unripe?

She: Unripe.

former tacitly. Interestingly, Lyons (1999) in making the same argument contra the UE approach as Lucy about Hanunóo *latuy,* and extending it to Ancient Greek *khlo ros* as well, is aware of the vagueness versus polysemy problem and states flatly that, in contrast to the English word *green*, "the colour-term sense of *khlo ros* is inseparable from its more general sense" (1999: 22-23). It is clear in context that Lyons intends this statement to cover Hanunóo *latuy* as well. Lyons, however, provides no support for his assertion of the monosemy of *latuy* and *khlo ros*. Wierzbicka takes a variant of the same line, as follows:

> Of course one could say the "wetness" implied by *latuy* is a separate semantic feature, which can be ADDED to a description in terms of hue, brightness and saturation. But the evidence presented by Conklin suggests that in the speakers' mind [sic] this "wetness" or "juiceness" IS NOT an independent semantic feature: rather, it is an integral part of the same prototype which accounts for the kind of greenness associated with this word... (1990: 119 emphasis in original).

What Conklin actually writes about the theoretical status of the relation of the color meaning of *latuy*, and other Hanunóo color words, to their non-color meanings is restricted to the following:

> The basis of this Level I classification [i.e., the color significata of the four Hanunóo basic color terms, including *latuy*] appears to have certain correlates beyond what is usually considered the range of chromatic differentiation, and which are associated with non-linguistic phenomena in the external environment (1955: 191).

This statement is silent on the issue of irreducible prototype versus separate features and similarly silent on the more general issue of vagueness versus polysemy. It is the only statement Conklin makes regarding the theoretical relation of the color significata of Hanunóo color words to their non-color significata. A metonymic or metaphorical relation between the meaning green (or grue) color and the notions of immaturity and/or succulence is widespread in the languages of the world, including, close to home, the Germanic, Romance and Celtic languages (Kay 1999: 84-85) as well numerous unwritten languages like Hanunóo. In the thoroughly documented European languages, the relation is clearly one of distinct senses rather than an irreducible prototype or some other sort of vagueness. The question needs to be investigated in the less well documented cases, not simply asserted to be the reverse of the known cases.

Lucy's interpretation of Conklin's "newly cut bamboo" example is as follows:

> ... the terms have other meaning values, meaning values which are not, despite assertions of others to the contrary[7], merely connotational colorings, but which have to do with other typical referential values... [This is] *not* 'mere'[8] connotation ..., it is direct reference pure and simple (Lucy 1997: 324, 326).

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[7]Lucy does not say who these others are.

[8]The source of the quotation is again not revealed. Lucy (1997) contains several other direct attributions of foolish or offensive usages to unidentified adversaries, for example those who have putatively advanced "premature judgments about 'deficient' color systems, or evolutionarily 'primitive' ones" (Lucy 1997: 341).

Lucy, thus, considers just two possibilities for the relation between the color and non-color meaning elements of color words: (1) the color meanings constitute the denotation, and the non-color meanings the connotations, of a single sense (wrong) and (2) both color and non-color meanings go to make up the denotation of a single sense (right). Lucy does not consider the possibility that *latuy*, for example, has more than one sense.

Lucy discusses a second example from a non-Western language in which color words embody non-color information. This example, involving the Zuni words for yellow (including orange), is instructive for two reasons. First Lucy's discussion, based on that of Hickerson (1975), inadvertently reveals how superficial analysis can obscure semantic similarities between languages. Secondly the juxtaposition of this example with that of Hanunóo *latuy*, etc. illustrates the useful distinction between conjunctive and disjunctive cases of association of color and non-color meaning elements in a single word.

After concluding his discussion of Hanunóo, Lucy continues as follows:

> Let us take a second example. In an early study of color terms in the Zuni language, Lenneberg and Roberts (1956: 24) claimed that Zuni speakers do not differentiate the colors "orange" and "yellow", but have a common lexical category *lhupz/inna* referring to the two. The Zuni terms used to refer to color seem to differ from ours in more than the ways just indicated, that is, in their general culture and

linguistic-systemic values as well. The linguist Stanley Newman (1954: 87-88), provided the following information concerning Zuni terms referring to the "color 'yellow'":

> Zuni has two lexemes expressing the literal notion of the color "yellow." Lexeme A would be used in contexts such as "yellow shirt" and "yellow paint". Lexeme B is employed in combinations such as "yellow skin" and "yellow leaves." The difference is not one of hue. Rather, lexeme A covers many shades of yellow characterizing an object while lexeme B refers only to an object that has become yellow (or a related hue...), as a result of ripening or aging ... [S]uch a distinction ... suggest[s] that an investigation of color terms must recognize that such terms may express discriminations other than those involved in the color spectrum. [...]

In a comparison of the morphological status of the various Zuni terms referring to color, Hickerson (1975) reached a similar, although more general, conclusion about Zuni color terminology, namely that there are two basic kinds of terms with color reference, broad, abstract terms deriving from verbs, and specific terms deriving from substantives (nouns, and particles). She says, "The verbs [referring to color] deal, ultimately, with processes of change or 'becoming': most of the actual forms indicate an apprehended verbal state. Nouns and particles refer to intrinsic color, specific to a substance or object, and are unchanging. In other words, these two types of terms, verbals and substantives, seem to reflect two basically different types of experience"

(Hickerson 1975: 228). Thus, the cultural and systemic meanings of the Zuni terms differ substantially from our own... (Lucy 1997: 337-338).

The Lucy-Hickerson interpretation of Newman's report makes Zuni color terms sound exotic: "two basically different types of experience", and so on. But English morphology expresses a similar, if not identical, semantic contrast. The basic form class for color words in English is adjectival, while apparently it is nominal in Zuni. We should not be surprised at this; in Somali, for example, some basic color terms are intransitive (stative) verbs, others are adjectives and one is a noun (Maffi 1990b). In both Zuni and English there are verbal forms denoting events in which something becomes a certain color. In English these are derived from the color adjectives by processes of limited productivity: *to whiten, to redden, to yellow,* etc. The past participles of these derived causative and inchoative verbs serve in turn as the sources of secondarily derived resultative adjectives, *whitened, yellowed,* etc., which denote the state of having become a certain color. Recall Newman's example of becoming yellow via a process of ripening or aging, perhaps the kind of thing Hickerson has in mind when she talks of "an apprehended verbal state." Zuni may well contain distinct sets of color words "which reflect two basically different types of experience." If so, English does too, and, so far we can tell, pretty much the same two types of experience: colors as inherent properties versus colors as resultant properties. Morphologically speaking, while in English the inchoative and resultative color forms are derived, in Zuni the corresponding words appear, in at least some cases, to represent distinct root morphemes. Since the derivational processes involved in the English resultative color words include some that are minimally productive, the distinction between the distinct root morphemes of Zuni resultative color words and the derived status of English resultative color words does not amount to a distinction between the rote-learned and the compositionally generated parts of the two languages. To sum up, a single semantic contrast (inherent color versus resultant color) is expressed in the two languages by somewhat different morphological means.[9]

The second observation suggested by the Zuni yellow example is of the widely overlooked contrast between conjunctive and disjunctive encodings of color and non-color information in a single word. Lucy is no exception among the critics of the UE tradition in citing examples such as the Zuni words for yellow and English words like *blond* and *palomino* as examples on a par with Hanunóo *latuy* (Lucy 1997: 343-344). But while the Zuni yellow words and words like English *blond* have meanings of the form 'yellow' AND 'result' or 'light-colored' AND 'hair or (furntiture)', a word like English *green* or Hanunóo *latuy* has the meaning 'green-colored' OR 'unripe/succulent'.[10] To be aptly called *blond* or *palomino,* something has to have *both* a color property *and* a non-color property. But with *latuy*, as with English *green*, a thing may be aptly characterized by the word if it possesses *only* the color property or *only* the non-color property. We recall that in Conklin's famous example, the brown-colored piece of freshly cut bamboo is *latuy* only in that it is wet and, of course, a green-colored piece of dyed thread or a green color card are *latuy* only in that they are green in color.

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[9]In the case of Zuni yellow words, examination of the full range of facts from a *familiar* language shows the semantic exoticness of a less familiar language to have been exaggerated. Some other examples of this type are discussed in Kay (1996).

[10]OR here denotes logical or. Something can, of course, aptly be called *green* if it is both unripe and colored green.

The disjunctive character of *latuy* suggests that *latuy* may be ambiguous, like English *green*, rather than vague, as Lyons and Wierzbicka claim and Lucy tacitly assumes. Future studies of color vocabulary should, in any case, attend to the distinction between conjunctive and disjunctive combinations of color and non-color information. Critics of past and current research efforts might also benefit from a recognition of this distinction, especially as Conklin's Hanunóo observations, published over twenty years ago, play such a prominent role in contemporary critiques.

These problems aside, what can we retain of value in the observation that many languages contain words that denote both color and non-color properties, sometimes within a single sense? Berlin and Kay (1969) tacitly assumed that each language contains a small set of words (more carefully, word senses) whose significata jointly partition the perceptual color space. All the major critics of the UE view have proposed, with varying degrees of clarity, that although this is true for English and familiar European and Asian languages (and surely also of many carefully documented languages of the Americas, Africa, Australia and Oceania), it may not be true of all languages. It is possible that some languages do not have any set of word senses whose significata jointly exhaust the perceptual color space. This proposal has been dubbed the Emergence Hypothesis (EH) (Kay 1999; for an earlier statement of essentially the same idea, see Maffi 1990a, who speaks of evolution *toward* basic color terms). It is possible that preceding – or accompanying – the familiar evolution *of* basic color term systems, there may be an evolution toward basic color term systems. Only detailed field investigations by workers familiar with the language(s) they are studying will be capable of making this kind of determination. A full assessment of the place of color words in the grammar (as properly urged by Lucy) and extensive observations on natural usage – as well as mappings to standard color stimuli – must all be part of an investigation capable of evaluating whether a language is an 'EH language'.[11]

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[11]Levinson's report on the language of Rossel Island (1999) satisfies many of these criteria. This language appears to be an EH language more by virtue of simply not naming all the color percepts than by naming some of them only with words of the *blond* type. Kay and Maffi (1999) review the data available on the 110 languages of the World Color Survey for evidence of the EH. Acknowledging that these data were not gathered with the EH in view, Kay and Maffi find four languages that appear to provide direct evidence for the EH and three more languages which may provide indirect evidence for the EH. (The latter possibility depends on the correctness of Kay and Maffi's speculation that the EH plays a role in the genesis of yellow/green categories.) An anonymous referee for *Anthropologie et Sociétés* [where the original version of this paper appeared] voiced what is probably a widespread concern: that the methods of the World Color Survey, like those of the original Berlin and Kay (1969) study, militate against the discovery of EH languages. This is probably correct, despite the mitigating findings of Kay and Maffi and it is why Kay (1999), comparing the Berlin and Kay (1969) and WCS findings to those of Levinson (1999), urges, "the WCS data ... were not systematically gathered with the EH in mind and only data gathered *in situ* with the EH specifically in mind are likely to shed more than pallid light on this hypothesis." If one distinguishes data from analytical method, the problem may be somewhat less grave than the quoted warning suggests. To be sure, "data gathered *in situ* with the EH specifically in mind," are greatly to be desired. But once the analyst entertains the EH, existing data can sometimes be newly appraised. For example Berlin and Kay (1969: 57) classify Pomo as a Stage II language with terms for BLACK, WHITE, and RED. (No designation more precise than 'Pomo' is furnished for this language.) This was one of the twenty languages treated experimentally by Berlin and Kay. In the data section of Berlin and Kay (1969: 127) one observes that the (single) Pomo collaborator assigned two of the 329 color chips to the term glossed BLACK, one chip to the term glossed WHITE and six chips to the term glossed RED, leaving 320 chips unnamed. In retrospect, it seems that glosses of 'black', 'white' and 'red' would have been more justified than 'BLACK', 'WHITE', and 'RED', the capital letters of the latter implying that these three words partition the entire perceptual color space. Knowing what we do now about EH languages, we would not jump from these data to the conclusion that Pomo is simply a Stage II language, with three basic color terms covering the perceptual color space. Clearly, more collaborators and a wider range of tasks would be necessary to decide the issue definitely, but it

Response to Lucy's Point 2

Lucy finds the fact that the basic color terms of a language do not always constitute a natural class on morphological or syntactic grounds to constitute a devastating critique of the UE program. He stresses repeatedly that some work in the UE tradition, a tradition going back to Lenneberg and Roberts (1956), is based on a correlation of word denotations with a set of color stimuli and is not embedded in a thorough morphosyntactic analysis of the words in question. Methodologically speaking, any task of mapping word denotations to color stimuli should ideally be pursued within a complete description of the relevant morphology and syntax. Lucy is entirely correct in this. It is for this reason that in the original *Basic Color Terms* study, we did extensive interviewing to discover which were the basic terms, relying on a mixture of morphological, syntactic and semantic observations, before assessing the denotations of any terms.

In analyzing the WCS results, our ability to control the relevant grammatical facts is perforce more limited. Our instructions to field workers have urged sensitivity to morphological and syntactic issues and we have frequently corresponded with the field workers in the course of interpreting their records, in order to obtain greater grammatical detail. Using internal evidence from the forms themselves along with consultations with the original field workers, and sometimes with other workers on the target languages or closely related ones, the WCS staff believes it is doing a

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is clear that the data presented in Berlin and Kay (1969: 127) do not justify their assignment (1969: 57) of this language to Stage II.

competent job of assessing the grammatical issues that go into deciding which are the basic color terms of the languages under study. It is of course likely that we will end up making some mistakes and it is certain that even with full grammatical knowledge, there are terms in some languages whose basic status is marginal. I believe that the overall results of our study will nonetheless be grammatically sound. In any case, the data and the inferences from them will all be made available in the forthcoming monograph for those who prefer to withhold judgment until they know the facts.

At the methodological level, Lucy's insistence on maximum possible knowledge of the grammatical status of color terms when assessing their semantic value is a valuable contribution. At the theoretical level, however, Lucy appears to suffer from a view of language according to which there is a one-one mapping between grammatical and semantic categories. For example, Lucy notes that several English color adjectives like *red* form verbs in *-en* while others like *yellow* don't, that we have nouns like *yellowing* but no analogous nouns **bluing, *orang(e)ing,* etc. and a number of similar facts regarding partially overlapping morphological sub-groupings of English color terms. Lucy states, without supporting argument, that:

> These differences in [morphological] potential both contribute to and arise from the meanings of the terms. In particular, what accounts for the absence of the *-en* forms ... or the absence of the *-ing* forms...? There is clearly some difference in lexical meaning here which prompts the differential treatment (Lucy 1997: 328).

There could, of course, be a semantic basis for the morphological distribution observed, although we are obliged to view with suspicion Lucy's confident claim that such a semantic basis exists since he is either unwilling or unable to reveal its identity. Lucy simply assumes, here and elsewhere, that *every* semantic category corresponds to a morpho-syntactic category and conversely. That is the basis on which he concludes that if the basic color terms of a language don't form a morpho-syntactic class they can't form a semantic class.[12] Contrary to Lucy's belief, grammars are full of semantic arbitrariness. There is, for example, no plausible semantic reason why English should allow all the sentences in (1)a-c but not (1)d or allow the complex pattern seen in (2).

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[12]In the assumption that every semantic class corresponds to a formal class, Lucy falls into the same trap Whorf (1956 [1941]) did in inferring a difference in Hopi and so-called Standard Average European *weltanschauung* from the fact that the grammar of Hopi does not yield a past/present/future semantic contrast – while not noticing that the grammar of English contains no such contrast either. It appears that Whorf was so sure *a priori* that the grammar of English *must* contain a paradigmatic contrast corresponding to the notional contrast past/ present /future that he didn't bother to look. Had he done so, he could not have failed to notice that past and present in English are expressed by inflections of the finite verb stem while future is expressed either by the modal auxiliary *will*, which precedes the main verb stem and may be separated from it by other auxiliaries and adverbs, or by a raising version of the present participle of the main verb *go*. (Chomsky has made this point in lectures.)

Neither French nor German (presumably also 'Standard Average European' languages) conform to Whorf's SAE tense picture, either. Briefly, while English has finite inflections for present and past and an auxiliary for future, French has finite inflections for present and future and an auxiliary for (non-progressive) past. Older and formal German has a system essentially like that of English while modern colloquial German expresses future with the traditional present tense inflection, relying on adverbs or context to convey the notional distinction between present and future time. Incidentally, these grammatical differences among Whorf's so-called Standard Average European languages not only show no correlation with the past/present/future notional opposition, but also cast doubt on Whorf's notion of Standard Average European grammar.

(1) a. The motor began to vibrate.

b. The motor continued to vibrate.

c. The motor ceased to vibrate.

d. *The motor stopped to vibrate.

All English aspectual verbs take gerundial complements, *(began vibrating)*, none take bare infinitive complements *(*began vibrate*) and all except *stop*, take marked infinitive complements. It is doubtful that this can be explained semantically.

English adjectives of probability show similar vagaries of syntactic valence that resist semantic explanation.

(2) a. It is likely/unlikely that Pat will win.

b. Pat is likely/unlikely to win.

c. It is certain that Pat will win.

d. Pat is certain to win.

e. *It is uncertain that Pat will win.

f. *Pat is uncertain to win.

g. It is probable/improbable that Pat will win.

h. *Pat is probable/improbable to win.

Among adjectives of probablility, *likely* and *unlikely* permit both extraposed sentential complements (2a) and raised NP subjects (2b). Both Extraposition and Raising structures occur with *certain* (2c, 2d), but neither occurs with *uncertain* (2e, 2f).[13] Extraposition is possible with both *probable* and *improbable* (2g) while Raising structures are compatible with neither *probable* nor *improbable* (2h). Distributional facts like these defy semantic explanation. These, and many analogous observations, show that distributional classes need not correspond to notional classes.[14] Just as, *contra* Lucy, every distributional class need not corespond to a notional class, so every notional class need not correspond to a distributional class. We will see below an example from Wilkins' work on Arrernte word associations of a notional class, color, that fails to match any distributional class.

Lucy holds that the UE enterprise is invalid because the basic color terms of a language do not always constitute a morpho-syntactic class and only sets of items that form a morpho-syntactic class are valid subjects of semantic investigation: "To repeat," he writes, "meaning is not reducible to denotation but is also a function of and a determinant of structural position. Yet in this attempt to probe the semantics of language, attention to linguistic structure is virtually lacking ... A content-based collection of lexical items does not constitute a linguistic system." (Lucy 1997: 328,330). It is this belief which allows Lucy to discount reports like that of Maffi (1990b) on Somali, which after careful evaluation of all factors, morphological, syntactic, historical and denotational concludes that the language does contain a set of basic color terms and that they do not constitute a homogenous morpho-syntactic

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[13] An anonymous referee for *Anthropologie et Sociétés* found (2)e acceptable. Interpersonal variation in this set of judgments serves only to reinforce the point that the syntax doesn't correlate in any uncomplicated way with the semantics, since disagreements about the acceptability of these sentences do not appear to be accompanied by corresponding disagreements about what they mean (or would mean if grammatical).

[14] See Hudson et al. (1997) for a collection of examples of this type.

category. Maffi goes on to point out, as several others have done for other languages – and as even Berlin and Kay noted for several languages in 1969 – that morphological subsets within the color terms may coincide with the basic-nonbasic cut and also with evolutionary stage sets, such as black-white-red, within the basic terms. In Somali the terms for black, white and red belong to one morpho-syntactic class, those for yellow and green to a second and the term for blue to a third.

Some recent unpublished work by David Wilkins on Arrernte color-word associations demonstrates that a set of words isolated, not by distributional criteria but only by the fact that they all denote colors, apparently forms a cognitively real unit.[15] The results of Wilkins's elicitation of free associations of six Arrernte speakers to 125 lexical items of mixed form class are shown in Figure 1. Figure 1 is abridged from a handout prepared by Wilkins and all the text that appears on Figure 1 is Wilkins'. The figure is self-explanatory. Figure 1 shows that although the four basic color terms of Arrernte are "on formal grounds ... part of a much larger set of terms," these four terms elicit each other almost exclusively in a free association elicitation paradigm (21 out of 24 responses) and never occur as responses to any of the other 121 terms used in the test.

Response to Lucy's Point 3

Lucy is aware of the cross-language findings of the UE tradition and is at pains to discredit them.

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[15]Arrernte is the language known in the older literature as Arunta, Arranda etc.

> ...what about the success of the [UE] approach? After all, as apologists for this tradition often note, it works! These color systems are there! Surely that is an interesting and important fact in its own right. Well I agree that something is there, but exactly what? I would argue that *what is there is a view of the world's languages through the lens of our own category*, namely, a systematic sorting of each language's vocabulary by reference to how, and how well, it matches our own (Lucy 1997: 331 italics in original).

Lucy says that starting with the color space and looking at how different languages lexicalize it guarantees findings of the UE type. But if this were the case it would be very hard to understand how all the mid-century relativists assumed the contrary. H. A. Gleason summarized a dominant anthroplogical consensus of the forties, fifties and sixties in his influential *Introduction to Descriptive Linguistics*:

> There is a continuous gradation of color from one end of the specturm, to the other. Yet an American describing it will list the hues as red, orange, yellow, green, blue, purple, or something of the kind. There is nothing inherent either in the spectrum of the human perception of it which would compel its division in this way (1961:4).

Gleason is saying, "If you examine the way the words of another language split up the perceptual color space, you will find no reflection of the distinctions you find in English." I want to focus first, not on the consequent of this conditional statement but on the method implied by its antecedent, which is: *start with the perceptual color space and see how the lexicons of* *different languages segment it*. Gleason and his fellows assumed, just as Berlin and Kay did, that every language contains a set of words which jointly denote all the colors. This assumption may be slightly wrong. But it doesn't follow from this possibility that examining color denotation cross-linguistically *ipso facto* guarantees a universalistic result. Gleason (and Ray 1952, 1953, and Bohannon 1963, and Nida 1959, and Krauss 1968, and many others) in effect predicted that research conducted in the UE manner would find no universals. It was and remains *logically* possible that every language cut up the color space in a way unrelated to that of every other language, as Gleason & Co. thought. If an arbitrarily selected language were as likely to have a color category spanning, say, orange, yellow and chartruese as to have one spanning green, turquoise and blue, then that is what the UE tradition would have found. Lucy says, "The universalist conclusions are built into the methodology and conceptualization of language employed in this research ... the universal finding is packed into ... the use of the Munsell array ... (1997:338)." We could have used a Munsell array and found what the Whorfians said we would find if the color nomenclatures of the languages of the world were the way the Whorfians thought they were.[16]

Lucy declares that he is going to tell us how the UE methodology locks in universal results from the outset: "To see how the universal result is guaranteed, let us look at the procedure in its most usual form" (1997: 332). But he does not do this.

Lucy's first argument is that many languages don't have a word meaning 'color'. He says without such a word, "we have a conceptual or

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[16] This point has been made before, e.g., by Maffi and Hardin (1997: 350).

cognitive category, but not a linguistic one"(1997: 332). If true, this statment might fit into an argument that the UE findings are not about language but about something else. But this assertion has no discernable connection with the claim that the UE results are guaranteed by their method.

Lucy's next argument is that the basic color term concept ignores the morphology and syntax of the language. "The actual grammar of the language plays almost no role in the analysis,..." (1997:333).[17] Again, this could conceivably contribute to an argument that the UE results are about something other than language, but it has no relation to the claim that the method guarantees the results.

The remainder of Lucy's argument that UE methods guarantee UE results consists in the following assessment of the scientific probity of current research in the UE tradition. Lucy writes:

> ... when a category is identified now, it is really the investigator who decides which 'color' (or 'composite color') it will count as. What are the odds that an investigator would ever report a system with terms corresponding to dark, white, purple, and brown? My suspicion is that it would be coded either as a two-term system of dark/cool versus light/warm with two other non-basic terms, or perhaps as a four-term system of black, white, red, and yellow. Either way purple and brown simply will not emerge (1997: 334).

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[17]Unaccountably, Lucy completes this sentence "yet our own grammatical pattern is applied as the standard for identifying appropriate color forms," although he has been at considerable pains five pages earlier to argue that English color terms do not form a grammatical class.

It is not clear what sort of hypothetical data Lucy has in mind. If the term he calls 'dark' includes the denotata of the terms he calls 'brown' and 'purple', the brown and purple terms are, by definition, not basic. If, on the other hand, the denotata of the brown and purple terms are not included in those of the dark term, then 'dark' is an incoherent gloss. This problem aside, it is unclear from this description whether or not Lucy intends in this hypothetical color lexicon that large regions of the color space are unnamed, and if so just what regions these are. Lucy's description of these hypothetical data does not add up to any clear picture. The remaining element of the argument consists in Lucy's suspicions about what a UE analyst would say about this hypothetical case. Even if the made-up data were clear, Lucy's suspicions regarding the conclusions an unidentified UE analyst would draw from them would still not constitute a scientific argument.

The current (unpublished) WCS analyses contain numerous cases of both brown and purple basic terms that occur in relatively early systems, not to mention several cases of terms that cover just brown and purple, others that cover just brown and gray, and even a few that cover just brown, purple and gray. WCS analysts can and do recognize data sets that challenge the theory. Many details of the theory have changed since 1969, in response to new data as they have been encountered. When the WCS analyses are published, the full set of individual chip naming choices for every informant will be made available, along with several kinds of machine-generated summary arrays. The scientific community will then have before it the evidence necessary to judge whether the assignment of categories to data sets is, as Lucy claims, subject to the unconstrained whim of the analyst.

To summarize point 3, Lucy states clearly and repeatedly that the UE method has the UE results built into it. Two of the three observations he presents as arguments for this claim do not address the claim and the one argument which does address the claim is based on Lucy's suspicions regarding a hypothetical analyst's classification of ill-defined hypothetical data.

Saunders and van Brakel (1997 and many earlier papers separately and jointly) echo Lucy's claim that UE methods guarantee UE results. In replying to Saunders and van Brakel, Berlin and I have made the point that the repeated occurrence of only a few of the logically possible composite categories in the world's languages, demonstrates an order in the cross-language data that cannot be a projection from English (or other languages of industrial societies). Suppose for purposes of argument, that red, green, yellow and blue were pure creations of English, not evidenced, for example, in the behavior of macaques (Sandell, et al. 1979) and chimpanzees (Matuzawa 1985). Even so, there is nothing in English which suggests that green-or-blue and red-or-yellow should be popular composite categories in the world's languages, that green-or-yellow should be an unpopular one and that a red-or-blue composite should not exist in any language (despite the subjective shading of red into purple and purple into blue). Lucy alleges (1997: 334) that diagnosis of composite categories is unconstrained by the data and strictly at the whim of the investigator. But the myriad reports in the literature of green-or-blue categories, for example, antedating Berlin and Kay (1969) show that this claim cannot be correct (e.g., Franciscan Fathers 1910, Cuervo Marquez 1924, Prost 1956, Voegelin and Voegelin 1957, Gudschinsky 1967 –

not to mention numerous personal communications cited in Berlin and Kay 1969, whose authors could only have been influenced by UE theory if they were able to foresee the future.)

Conclusion

Regarding Lucy's first point, that color words may also signify non-color properties, this fact about Hanunóo color words was discused in the first paragraph of *Basic Color Terms*. In light of the facts touching conjunctive and disjunctive combinations of color and non-color meanings considered above it appears likely that the four Hanunóo basic color terms are each ambiguous between a color and a non-color sense, according to one or more systematic metonymies. But even if Hanunóo color words are monosemous, the fact that their color meanings neatly fit the UE classification has yet to be successfully explained away. More field work, and less textual exigesis, needs to be done on color systems like Hanunóo, where major color words appear to conflate color and non-color information.

Lucy's second point, that basic color terms do not always form a unified morpho-syntactic class, is also frequently recognized in the UE literature. It has been pointed out that morphological subsets of the basic color terms of a language may correlate with UE evolutionary stages and also that a morphological distinction sometimes obtains between the basic and the non-basic terms. Lucy is simply wrong that semantic classes in general always correlate with morpho-syntactic classes, as shown both by numerous English non-color examples and Wilkins' free association work on Arrernte color terms. But Lucy is right that more attention needs to be paid to the grammar of color words in future studies, particularly those attempting to evaluate the Emergence Hypothesis, according to which there are languages which do not have full-fledged basic color term systems in the UE sense.

Lucy's third point (echoed by Saunders and van Brakel), that the UE results are methodological artifacts, is supported by no sound argument. On the other hand, both the confidence of the mid-century relativists in an approach of precisely the UE type and the non-English character of the UE findings on composite categories provide *prima facie* evidence that these findings are not methodological artifacts.

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• Four "colour" terms embedded in a word association tests of 125 Arrernte terms
• Other terms in list are a mixed bag of nominals, adverbs, verbs, etc.
• List randomly ordered
• Task done with 6 adult Arrernte speakers
• Arrows go to responses, and number on arrows indicate how many people gave that response.

ankeyalthe 'greedy person' 1↑
urlpe 'red ochre' 1↑

4. athetheke 'red-pink-orange' → 2 51. urrperle 'black and very dark colours)very dark blue; very dark green; very dark red)'

1↓ 3↑ 1 ↘ ↖ 1 6↓ 4↑

67. atherrke-atherrke 'green-yellow-blue' → 3 60. mperlkere 'white and very light colours'

1↓
tywelke 'white ochre'

• The words identified as 'basic colour terms' overwhelmingly call up other 'basic colour terms' [There are 66% to 100% intrafield responses to stimulus]
• There appears to be a particularly strong association between *urrperle* 'black and very dark colours' and *mperlkere* 'white and very light colours' - For all 6 respondents, *urrperle* calls up *mperlkere* , and for 4 respondents *mperlkere* calls up *urrperle* .
• Of the three responses outside the semantic field (interfield choices), two were given by the oldest respondent (a man), and were the names for ochre types which exemplify the colour term given as the stimulus terms. The remaining term, *ankeyalthe* 'greedy person', was given in response to *athetheke* 'pink-red-orange'. This colour is associated with 'greed', and several idioms referring to greed include the colour term.
• NONE of the other 121 stimulus terms were responded to with one of these four terms. [Further consolidating the view that this may be a close-knit semantic set.]
• On formal grounds these four terms are actually part of a much larger set of terms to do with the visible surface (reflective) properties of objects.

Figure 1. Arrernte Word Associations, from a handout by David Wilkins