On the Role of Culture in Color Naming: Remarks on the Articles of Paramei, Kay, Roberson, and Hardin on the Topic of Cognition, Culture, and Color Experience

Kimberly A. Jameson
University of California, Irvine
University of California, San Diego

Comment on the preceding articles is now provided to highlight some open questions and controversies raised in this issue. The primary goal is to provide a context for examining the long-accepted views from the cross-cultural color categorization and naming literature that are presented in the articles. For this reason alone, the articles by Hardin and Kay—which present new analyses compatible with the established theory in the area—receive more comment than do those of Roberson and Paramei—which, comparatively speaking, are less concordant with the established theory and reflect more culturally specific views of color naming phenomena. In my opinion, each of the four articles in this issue contributes important perspectives on cross-cultural color naming research. Although the order of articles in this volume is Paramei, Kay, Roberson, and Hardin, the comments presented here are organized to emphasize the authors' common view-

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points and to provide an ordered development of the issues the articles raise.

CLYDE L. HARDIN'S "EXPLAINING BASIC COLOR CATEGORIES"

The first paragraph of Hardin's article suggests that to assert that no significant properties of color categories are biologically based ("as some cultural relativists do") is to disregard all that perceptual psychologists know about color vision mechanisms. Most researchers would concur that sensible cultural relativists agree with universalists that biology serves as an important constraint for color perception. For example, consider that so-called color-blind individuals (who lack a type of color-sensitive cell in their retinae) perceive a reduced color category structure compared with otherwise normal individuals who possess a full complement of color-sensitive cells. In such a case, obviously retinal biology shapes perceptual experience in the color-blind dichromat.

The remaining portion of Hardin's argument is more difficult to accept, however, because although his first sentence acknowledges that there are "many properties of color categories that are a function of language and culture," he does not explore these much, opting instead to present support for the view that pan-human color processing is the basis for the universality seen in color naming and categorization behaviors.[1] A very different view, now gaining popularity among researchers, considers visual processing models inadequate as the primary basis for explaining complex color *categorization* behaviors seen within and across cultures. As an example, consider again the dichromat: Within a given culture, the anomaly of "color blindness" does not prohibit an affected observer from properly learning and using the culturally appropriate color terms and concepts (e.g., polar opposition of red and green) that refer to the color appearances that a dichromat observer has difficulty perceptually disambiguating (Izmailov & Sokolov, 1992; Marmor, 1978; Shepard & Cooper, 1992). This ability to adequately use and comprehend color language and color categories in the absence of perceptual correlates that differentiate between exemplars of various color terms and categories provides a clear indication that a substantial (if not central) portion of color categorization conceptual behavior is not attributable to the usual biologically based visual processing explanations.

In my opinion, both individual and cultural categorization behaviors require much more comprehensive explanations than that Hardin offers. Thus, Hardin's "accepted view" perspective is incomplete as an explanation of such behaviors, despite his correct assessment that empirical investigations of visual processing mechanisms make important contributions toward understanding the constraints and nonarbitrary features inherent in individual's early visual processing.

If the basis for cross-cultural color naming regularity is not universal biology, then what explains the frequently observed linkages described by Hardin between color naming universals and opponent-process-based perceptually elementary hues? To pursue this, we need to separately examine the links in the chain of the accepted explanation, essentially asking whether either of the nature or nurture views invoked by Hardin can be ruled out as possible (or partial) explanations for the apparent universality of perceptually elementary hues that seems to transcend the idiosyncrasies of particular languages (Hardin, 2005 [this issue]). Here, the goal is not to prove a "correct" alternative explanation for the phenomenon; rather, it is to ask whether a plausible alternative view can be specified, and if so, whether that alternative explanation can be similarly analyzed and ruled out as incorrect compared with the accepted view Hardin describes.

A simplified statement of the accepted chain of reasoning for the biologically based explanation that Hardin describes can be summarized by two statements:

A. The hues most frequently and reliably named by individuals, and most commonly named across cultures, are the perceptual elemental hues.
B. Pan-human uniformity in color vision (typically linked by color naming researchers to opponent-process visual mechanisms) is the basis for the widely shared significance of perceptual elemental hues (as compared with nonelemental hues).

The problem is that although Hardin's analyses (and much work in the Berlin and Kay tradition) show that Statement A is often supported, it can be argued that the explanatory causal connection in Statement B is not supported for (at least) two reasons:

1. Existing neurophysiological and psychophysical data indicate that individual variation in color vision mechanisms preclude the exist-

ence of uniform "elemental" stimuli that could produce unitary perceptual experiences across observers.

2. Existing psychophysical data indicate that there is considerable individual variation in empirically determined perceptual elemental hue settings across observers and to some extent across cultures (Kuehni, 2004; Webster et al., 2002).

Given that the biologically based rationale in Statement B is not the real explanation for observations in A, then why do so much existing data seem to suggest that "the hues . . . most commonly named across cultures are the perceptual elemental hues"? Following Hardin's usage, can we rule out the nurture-type explanation for Statement A? Or can nurture or cultural influences produce scenarios whereby perceptual elemental colors tend to be those most commonly named across many cultures?

At least one plausible nurture scenario—one emphasizing color naming and categorization as a *cultural practice* or *artifact*—explains the tendency to name elemental colors. That is, consider those cultures for which a linguistic gloss for *color* exists as an autonomous lexical construct. In such a culture, the gloss for *color* certainly operates as an important part of that culture's strategy for transmitting (to new generations of individuals) knowledge of all the separate linguistic terms that make up the culture's color lexicon. Under one such transmission strategy, individuals undergoing the process of acquiring a culture's color lexicon might first implicitly master the concept of color by recognizing that what is glossed by the term *color* is the culture's particular notion of how something appears or "looks." Many languages that are classified at or above Stage V (using the original framework of Berlin & Kay, 1969, or recent revisions) have a separate abstract concept for *color* represented by a superordinate linguistic term; however, many unwritten languages—those such as Bellona (Kuschel & Monberg 1974) and Yélî Dnye (Levinson, 2000)—do not have a native construct for *color* indexed by an unborrowed superordinate lexical term and thus may not have an abstraction for *color* integral to their cultural transmission of the color lexicon.

For cases where a superordinate term exists, during transmission of the concept *color*, differentiation of "pure" (or "unitary") or "blended" (or "binary") forms of color may occur. A tendency to begin by transmitting the simplest unitary cases first would imply that "pure" cases of black, white, red, yellow, green and blue (being appearances typified by less complex, or fewest number of, dimensional components) would be learned by individuals first. Indeed, this could be an advisable strategy for the cultural transmission of an abstract concept such as color, because using a simple set of items sharing common dimensional features (integral to the construct) may be optimal for the comprehension of the concept by individuals (cf. Feldman, 2003).

Under this first transmission scenario, colors that are unlikely to be classified as perceptually elemental hues would be categories of greater complexity due to *additional properties* (or dimensions) as *defining aspects* (Feldman, 2003). For example, appearances glossed by the term *gold* might additionally require *shimmer* as a defining aspect in addition to the typical lightness, purity, and hue aspects. Similarly, color categories composed of exemplars that *do not* reflect a representative range of exemplar variation along principle dimensions (e.g., saturation and lightness; see Jameson, 1997) in the space would require additional specificity regarding the defining aspects of the category. Thus, a defining aspect of a greenish-yellowish, "chartreuse," category could be distinctive *lightness* and *saturation* levels—traits that are characteristic of chartreuse exemplars (i.e., chartreuse exemplars rarely appear dark and seldom unsaturated). These unipolar lightness and saturation features (or defining aspects) of chartreuse category exemplars imply that the category is conceptually more complex than another color category that is composed of exemplars representing a range of values from the principle dimensions of the space (i.e., a full range of lightnesses, saturations, etc.) and which thereby is more simply and strictly summarized by a dominant hue feature as the defining aspect of the category (Jameson's [in press–b] contribution in the next issue discusses this further).

Although the foregoing scenario for cultural transmission in the context of a superordinate term for color may seem slightly counterintuitive, it reflects one plausible nurture-based hypothesis and is consistent with various developmental data indicating unexplained difficulties in color-term acquisition (summarized by Roberson, 2005 [this issue]).

As a consequence of this argument, one could assert that (a) a color is more likely to be considered a perceptual elemental hue when color space features needed to define that color category's best examples are minimal; (b) cultures' linguistic systems play a role in the definition of elemental colors when, as described, cultural variation in the occurrence of a native lexical term for a superordinate *color* concept influences which color appearances are naturally assigned lexical labels and the cultural transmission strategy for color lexicons; and (c) cultural practices and influences can play a role in the definition of elemental colors when the dimensions culturally deemed to have high pragmatic value (e.g., cultural preferences for hues; Saito, 1996a, 1996b) are strongly biased in ways that de-emphasize the dimensions that the elemental color categories classically represent. In summary, it is suggested here that nurture-type explanations should not be ruled out as a possible basis for why perceptual elemental hues are so widely named across cultures and that nurture-type explanations are plausible alternatives for explaining the highly cognitive and cultural behaviors observed in color categorization and naming results relevant to color appearances popularized by Hering's theory of color opposites.

Note the relevant detail that some informants used for the languages described in Berlin and Kay (1969) were bilingual, with English as one of their languages. Those authors acknowledge, "While it can be argued that bilingualism in English affects the results to some extent," they do not support the possible objection that bilingualism of their informants distorted their findings (p. 12). Nevertheless, English bilingualism among those subjects implies an awareness of English's superordinate-term *color* concept, even though the foreign language tested may have lacked a native term for that abstract concept. Whether assessment of naive foreign language speakers who have no term for *color* in their native language (and who have no familiarity with other languages with a term for that concept) would show a different tendency regarding the naming of elemental perceptual hues remains an open empirical question.[2]

For languages that lack a superordinate *color* term, a nurture-based conceptual explanation can also play a role in the cross-cultural similarity in elemental hues loci (e.g., those identified by MacLaury, 1997b, and discussed by Hardin, 2005). For example, in such languages, elemental hues can arise as salient colors for individuals and groups if either *polar opposition* or comparative color *purity* relations are linguistically salient. Thus, MacLaury's cross-culturally observed elemental hue positions may arise through a shared strategy of linguistically representing optimized polar opposites formed by the purest, unblended, extremes available in cognitive color appearance space. These two aspects of elemental hues, (a) color "purity" and (b) polar-opposition pairing with another "pure" color, may be a large part of developing a relational structure that distinguishes elementals from other nonelemental hues (see Jameson, in press–a, in press–b).

Thus, it is plausible that the difference distinguishing elemental hues from other hues could be a conceptual, or cognitive, difference, as opposed to a difference arising from a neural processing substrate or similar "channel" linked to opponent-process chromatic response functions present in low-level color vision mechanisms. Indeed, the latter linkage is seriously undermined by the fact that physiological response curve locations that the theory links to unique hue experiences do not actually coincide with spectral positions described as pure elemental colors (see Jameson & D'Andrade, 1997, pp. 301-308, for details; Gegenfurtner & Kiper, 2003).

Fortunately for the received view of color naming theory (i.e., the new variants of the Berlin & Kay, 1969, theory; see Kay & Maffi, 1999), describing elemental hues in this cognitive/conceptual way remains entirely compatible with Hering's original opponent-colors ideas (especially for color naming systems sharing the complexity of English), as well as with the principles on which the Natural Color System (NCS) is based (see Hardin's [2005] discussion). The only difference in the alternative explanation provided here is that cross-cultural elemental hue naming and elementary color salience are not determined by "biologically based" channels. Thus, color-naming researchers can continue to use the Hering elementary colors as part of the explanation for color categorization and naming results, as long as they do not rely on the typically implied opponent-process neurophysiological mechanisms as the basis for such results.

Hardin states: "Names for the Hering elementary colors are necessary and sufficient for naming all of the colors, a fact that justifies singling them out" (2005, this issue). The rationale for asserting this is strained because the same argument would apply equally well to a subset of three elementary colors consisting of red, yellow, and blue. Indeed, where strictly chromatic proportions are concerned (i.e., those Hardin discusses for the NCS color samples), one can describe color appearances equally well as ratios of only red, yellow, and blue (as Goethe and others have proposed), similar to the way they are typically described using ratios of red, yellow, blue, and green. In such a scenario, green colors could simply be described as proportional combinations (i.e., product mixtures) of blue and yellow. Thus, all green-containing colors, and any other color in the NCS atlas, can be described just as well using three red, yellow, and blue proportions, as they can using Hering's four elementary colors.[3] The question then is, Why is this strict summing of named proportions for red, green, yellow, and blue primaries preferred as the operational definition for estimating NCS color proportions when a simpler, more parsimonious subset of primaries is given by three primaries (red, yellow, and blue)? In my view, these three primaries seem to serve just as well as a set of "necessary and sufficient" components for describing NCS color samples. The answer to this question may follow from a general tendency to extend psychophysical color cancellation primaries and their theoretical constructs into color naming research. In any case, I do not think the argument Hardin provides for "singling out" the names of Hering elementary colors is any more compelling than one that could be developed for the alternative set of primaries suggested.

Hardin's article also compares data from human and chimpanzee color naming (Matsuzawa, 1985). Hardin argues that similar chimp and human color naming behaviors support the notion that human color naming and categorization are based on the visual processing mechanisms shared by both species. It is true that chimpanzee and human color vision systems are highly comparable; however, another plausible nurture-like interpretation of Matsuzawa's (1985) findings is that these chimp and human color naming data are similar because the chimp tested has acquired through training a similarity structure for the Munsell colors that resembles the similarity structure of human observers. Indeed, the chimp's acquisition of this similarity structure is implicit in its successful training (the set of training colors varied along all three dimensions of Munsell hue (H), value (V), and chroma (C), and only these three dimensions). Thus trained, generalizing such a similarity structure to categorizing other Munsell samples would minimally consist of recognizing stimulus differences in hue (H), lightness (V), and saturation (C)—a task for which the primate visual system is ideally suited—and appropriately sorting such samples into highly overlearned hue "bins." Pigeons, which have a very different visual system from humans (see Varela, Palacios, & Goldsmith, 1993), can also be trained to sort color stimuli into the 11 basic color categories. Indeed, pigeons' internal representation for basic colors forms a hue circle, suggesting "that similarities among colors for pigeons are organized in a manner generally like those for humans. This is quite interesting in view of the anatomical differences between the species" (Schneider, 1972).[4] Thus, whereas such results are important for understanding generalization principles that are easily learned across species, they do not provide proof that similarity in color chip sorting across species is a consequence of shared color vision mechanisms.

Obviously, the present discussion of alternative nurture-like hypotheses reveals that I oppose the view that the strongest argument for the primacy of the Hering elementary colors in color naming is a neural processing argument, and prefer instead a more plausible higher-order cognitive argument. Instead of considering that the "names for the Hering elementary colors are necessary and sufficient for naming all of the colors, a fact that justifies singling them out" (Hardin, 2005, this issue), I believe the emphasis should be placed on seeking empirical explanations as to why the color opponents black/white, red/green, and yellow/blue seem to represent phenomenal opposites and can serve as useful markers in cognitive color space across a range of individuals who (a) vary considerably regarding the physical stimuli that evoke unitary hue experiences (e.g., Kuehni, 2004; Webster et al., 2002) and who (b) often vary considerably regarding individuals' point locations for category best examples (MacLaury, 1997a). In sum, this is not to say that Hardin's arguments for nativistic explanations are incorrect per se, rather it is to suggest that in light of plausible nurture-like explanations it seems too early for him to declare that the biologically based explanations he describes are the only major determinants of the cross-cultural color naming behaviors he reviews.

PAUL KAY'S "COLOR CATEGORIES ARE NOT ARBITRARY"

Kay's article discusses two principle issues: First, he examines data originally published as a challenge to the universal-evolutionary (UE) model, namely, the Berinmo data of Roberson, Davies, and Davidoff (2000) and the Yélî Dnye data of Levinson (2000). Second, he presents statistical analyses testing the plausibility that color naming systems tend to be based on primary basic color categories (black, white, and the Hering opponent colors red/green and yellow/blue).

Kay suggests that the results support two conclusions: (a) the reexamined data do not challenge but support semantic universals and evolutionary regularity (the UE model) through a correspondence with elemental hues (MacLaury, 1997b), and (b) Berinmo and Yélî Dnye color naming show a statistical tendency toward a basic primary colors bias.

With regard to the first claim, would Kay's computed centroids from Berinmo and Yélî Dnye data continue to closely correspond to elemental hues points if the stimulus sets used permitted greater freedom of indexing between color appearances and color names? It is known that sampled empirical stimulus sets can produce patterns of naming uniformity that diminish if the available choice options approximate the variety found in natural scenes (cf. Braun & Julesz, 1998). The centroids Kay computes are not exempt from influences of empirical stimulus properties and settings on informants' response tendencies. Influences of this kind can lead participants to uniformly respond using an improper exemplar because it is the best available among the choices offered. Various results in the literature suggest that Kay would not likely observe the same correspondence between his computed centroids and elemental hues under different empirical settings or stimulus sets.

Second, Kay's assumption of equal probability computationally benefits his alternative hypothesis. He assumes that "each of the 320 cells has an equal probability of receiving a hue centroid." This benefits the computations needed to reject his null hypothesis and accept his alternative hypothesis that the observed data closely fit the predicted model. This uniform probability assumption amounts to a zero-constraint scenario on the labeling of color—a scenario that clashes with the sensible construct of "target areas" that he defines.

Moreover, Kay's highly improbable null hypothesis stating that "hue naming centroids will bear no particular relation to the red, yellow and green elementals" is impossible not to reject and in essence requires that for categories roughly glossed *red*, *yellow*, and *green*, the Yélî Dnye share no features with the many languages examined by MacLaury when identifying his universal elemental hue samples (MacLaury, 1997b). This seems to me a little too much like a straw man hypothesis set-up to be easily rejected.

To summarize, both Hardin and Kay are rather adamant about the fact that significant constraints on color naming exist. Kay closes his article with the comment, "Neither the Berinmo nor the Yélî Dnye data . . . weaken the hypothesis that there exists universal constraints on cross-language color naming; indeed, they strengthen it," and Hardin opens his article with the comment, "To assert that no significant properties of color categories are biologically based is to disregard what perceptual psychologists know about color vision mechanisms." The directness of these comments supporting universal constraints on color naming and categorization is understandable given the prolonged debate between a group of scholars who espouse very culturally relative, anti-empirical views (cf. Saunders & van Brakel, 1997) and those who prefer to rely on what can be learned from the sometimes imperfect empirical data (authors in this journal issue and the next issue). Research included in this two issue series represents the empirically-oriented group mentioned and consists of a continuum of views from more "nativist than relativist" (e.g., Kay, Hardin, and MacLaury) to "more relativist than nativist" (Paramei, Roberson, Alvarado, Jameson, and Dedrick). I believe that the heart of the difference in views expressed by the contributors of these issues, then, is not so much whether human visual processing constraints exist but rather to what degree those constraints determine color categorization and naming behaviors within a culture and across diverse cultures. As seen in this issue's articles, opinions diverge, with Kay and Hardin vigorously advocating universal categories and focal examples related to Hering opponent colors theory and Paramei and Roberson arguing that certain social and linguistic factors varying across cultures contribute to color categorization and naming in ways that supplant what others attribute to universally prevalent opponent color appearance phenomenal channels. In my view, the crux of the debate rests in specifying what forms of proof are amenable and appropriate for addressing the very different forms of phenomena—cultural, psychological, and neuropsychological—that contribute at one time or another to the development and maintenance of color naming and categorization systems across cultures.

In contrast to the widely accepted establishment views presented by Hardin and Kay in this issue, Galina Paramei and Debi Roberson's separate articles describe empirical results that illustrate serious limitations of these views.

GALINA PARAMEI'S "SINGING THE RUSSIAN BLUES: AN ARGUMENT FOR CULTURALLY BASIC COLOR TERMS"

Paramei's analysis of the Russian "blue" categories makes a number of important observations regarding the dependence of results on empirical methods and data analysis techniques employed. For example, her empirical review for the Russian color terms *Sinij* and *Goluboj* illustrates the challenges inherent in generalizing "foci" findings across color order systems (e.g., the Munsell, the Natural Color System, or the Color-Aid System) and demonstrates that the stimulus set used greatly determines if a color term tested is found to empirically satisfy "basicness" criteria. (For example, the 330-chip Munsell stimulus grid does not permit proper assessment of saturation and lightness variations that play a large part in differentiating appearances named *Sinij* and *Goluboj*—a stimulus-set attribute consistent with MacLaury's [1997a] findings for *Goluboj*). Paramei's observations regarding the biasing potential of stimuli are consistent with the known stimulus-set effects in other perception and choice behaviors (cf. Braun & Julesz, 1998, Kahneman & Tversky, 1984).

Touching on another methodological problem, Paramei implicitly cautions against making inferences about individual cognitive and linguistic representation of color when aggregate data are used—a point frequently illustrated with the data of MacLaury's (1997a) Mesoamerican color survey. This methodological consideration presents a challenge to the general practice of mapping discrete color category foci using aggregate data.

There is a natural question of whether color category boundaries are an issue that should garner as much attention as have color category foci. In the Paramei article, this takes the form of a discussion on the proper definition of color term "basicness," and it is implicit in her observation that individual differences in exemplar naming provide a starting point for examining category boundary differences. Indeed, the question implicit in Paramei's analysis of the two Russian blue terms is, "What dictates that there will always be not more than 11 basic color categories and terms in a given culture's color lexicon?"

In view of her in-depth analysis of *Sinij* and *Goluboj*, Paramei raises the epistemological issue of whether a proper definition of basicness exists. There are languages for which all the criteria that are typically used to identify a basic term are satisfied by both the term considered an alternate category label and that same category's basic term. For example, such criteria apply to some languages in the Baltic and Caucasus regions when describing blue-category stimuli. However, despite the fact that in such languages two blue terms may satisfy the crucial basicness criteria proscribed by theory, there is tendency among some of them to limit color term basicness to a single gloss for each category. Such a practice seems to diminish the validity of the criteria used to define basicness and further underscores the question that Paramei raises of what actually are the proper grounds for defining basicness and nonbasicness of color terms across languages.

For improving color naming theory, Paramei suggests the use of pragmatic psychometric measures and social and cultural constraints as important components for a theory of color naming and categorization. She also generalizes the Interpoint Distance Model (Jameson, in press-a; Jameson & D'Andrade, 1997) to argue for a nonlinguistic cognitive basis for the emergence of *Goluboj* from the natural partitioning of a large segment of perceptual color space that is "unnamed," according to basic color theory (see discussion in Jameson, in press–b). She also argues for a greater recognition that color naming systems are dynamic linguistic entities, so that a given language's color lexicon should be expected to vary over time as locally salient cultural referents, linguistic drift, color term borrowing, and technological influences present pressure to change the language that indexes color.

In the end, Paramei presents a convincing argument that Russian's *Sinij* and *Goluboj* represent a challenging "special case" to many of the accepted tenets still robust in the recent updates of the Berlin and Kay Basic Color Term theory. As such, Paramei's article builds a strong argument for exploring new explanatory perspectives for what is typically viewed as a solved issue in cognition and culture research, and she presents several leads as to what those alternatives might be.

DEBI ROBERSON'S "COLOR CATEGORIES ARE CULTURALLY DIVERSE IN COGNITION AS WELL AS IN LANGUAGE"

Paramei's analysis of two Russian blues asks, "Why does accepted theory dictate a limit of 11 universal basic color categories and terms?" Roberson's article asks, "What aspects of color category universals are truly independent of language?" Roberson raises this question through a review of empirical data showing that color categories are greatly determined by a culture's color language. If, as Roberson suggests, cognitive color categories are highly dependent on one's learned language, and if color lexicons vary considerably across cultures, then the basis for the establishment's panhuman universal color category explanation is greatly undermined.

Bear in mind that Roberson's position is not as neo-Whorfian as it first sounds. Rather, Roberson and her colleagues believe that learned linguistic categories *facilitate* recognition of categorical partitions and correct perceptual classification of spectral continua, as opposed to strictly determining partitions of spectra. As such, Roberson's article represents a new version of linguistic relativism that contrasts with Kay and colleagues' widely accepted universalist perspective.

Specifically, Roberson examines the issue of whether speakers of a language can have more and different cognitive color categories than are represented linguistically. She reviews results from the larger field of categorization—some developmental, some cross-cultural—that support a tight link between language and thought. Through her review, she concludes that to the degree that color classification and categorization resembles other classification tasks, it is very unlikely that cognitive color categories are independent of the terms used to describe them.

She also presents convincing cross-cultural data comparing native speakers of Berinmo and English. Her results suggest that even in nonlinguistic sorting tasks, native speakers engage in categorical perception in a manner congruent with their native color lexicon and that neither group can easily make consistent classifications when taught the other group's color-language distinctions. Her results support a tight link between linguistic labeling and cognitive categories.

In the end, Roberson presents the explanation that in acquiring color categories, individual learners master their culture's specific lexical designations for the continuously varying stimulus domain of color. Her conclusion is that "there are no color categories that are independent of the terms used to describe them."

Roberson's language-relatedness proposal is consistent with a predicted result made earlier in the context of discussing the Hardin article. That is, cultures that do not have a linguistic term for the abstract concept *color* should have among its members a less robustly shared set of the classical elemental hue constructs. The rationale for this is that in the absence of an abstract concept such as color-of-a-thing, a culture's color labels should be largely determined by contextualized pragmatics such as "good-to-eat appearing," "bad-to-eat appearing," "dangerous appearing," and so on. In such a culture, the classical elemental hue constructs (unitary red, green, yellow, and blue) would likely be of less communicative value (and therefore less frequently used), when compared with another culture that has a robust abstract concept for color and a frequently used lexical term to express that concept.

Thus Roberson, like Paramei, calls for a reconsideration of the more culturally dependent aspects of color categorization and naming. In doing so, both reconfirm the views of others who have suggested that the historical emphasis of perceptual processing universals has hindered development of a full and proper explanation of color categorization across cultures (see also Jameson, in press–b; Dedrick, in press; and Alvarado & Jameson, in press, for similar or related views). In contrast to the Kay and Hardin articles, both Roberson and Paramei call for a more comprehensive view of color categorization and naming—to perhaps curb the overreliance on low-level visual processing as the explanatory mechanism and to integrate into empirical research the aspects of color naming and categorization that, to quote Paramei (2005 [this issue]), find "impetus from sociocultural mechanisms."

CONCLUSION

The four articles in this issue have had the common goal of trying to make sense of a range of empirical findings for color naming and categorization across different ethnolinguistic cultures. As is representative of the field, the explanations offered range from universalistic to culturally relativisitic. A popular version of the universalist perspective asserts that although color naming differences exist across cultures, they are largely explained by a model of pan-human shared color experience, and this determines the nonarbitrary basis by which all cultures categorize and name color sensations. On the other hand, a moderate form of the relativist perspective argues that too much of color naming and categorization behavior is shaped by sociocultural influences, and these influences produce differences in cross-cultural color naming that cannot be explained by the most popular universalist explanations. I do not think either opinion disputes that human visual processing imposes constraints on human color behaviors. However, I do believe that enough questions are raised by the relativistic perspective to warrant the development of a more comprehensive explanation of cross-cultural color naming phenomena, and I believe that cultural studies and cultural factors should play greater roles in the new perspectives developed. The four articles presented in the next installment of this two-issue series aim to provide starting points and new approaches for developing a more comprehensive perspective, and they set the goal as gaining an improved explanation of the large corpus of existing color naming and categorization data.

Notes

1. Hardin frames his support for the accepted view through an analysis of several universal color processing results that are "primarily due to nature" as opposed to those "mostly due to nurture" (this issue). For the sake of comparing the two different views Hardin invokes, my comments here frequently refer to this highly simplified nature/nurture dichotomy.

2. Jameson and Alvarado (2003) found that compared to monolingual Vietnamese speakers tested, Vietnamese/English bilingual speakers assessed in their native-language Vietnamese nevertheless showed patterns of color naming and categorization similar to monolingual English speakers. The authors suggest that despite the context of Vietnamese asessment, bilingual subjects' Vietnamese naming behaviors clearly reflected the influences attributable to second-language English color categorization structures.

3. One might criticize this alternative idea by asserting that estimating the proportion of green in a sample is in some way an easier judgment than estimating the proportion of yellow plus blue. To assert such a criticism, comparative judgment difficulty should first be empirically examined. Moreover, empirical judgment difficulty is not an explicit part of the rationale used by Hardin when he argues for the necessity and sufficiency of the Hering color descriptors.

4. Schneider (1972) trained pigeons to sort 11 colors (importantly, spectral lights ranging in wavelength, not Munsell samples). His results suggest that human color categories represent a trainable, predictable subset of pigeons' categorical color perception, despite the tetrachromatic (or even pentachromatic) properties of those birds' color vision (Varela, Palacios, & Goldsmith, 1993). Jacobs (1981) generally cautions the use of human hue categories in cross-species research. However, as Schneider implies, it seems likely that the color space dimensions arising from human trichromacy are interpretable in terms of the color space dimensions of tetrachromat pigeons. Even so, pigeon (or chimp) sorting into human color category bins does not imply color appearance equivalence between observers from these different species.

5. Indeed, although the physiological interpretations he makes are not correct from the point of view of our current understanding, in statistical analyses Ember showed that cultural complexity combined with distance from the equator are together a better explanation for color lexicon complexity than either factor alone. This implies that environment and culture are also contributing factors which the accepted view theory needs to account for in explaining color lexicons across cultures (Ember, 1978).

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Kimberly A. Jameson has a Ph.D. in psychology from the University of California, Irvine. Her research seeks to understand the ways individuals and groups conceptualize naturally occurring categories in the real world. This includes empirical studies of color categorization and naming, the modeling of concept formation for perceptual stimuli (e.g., individual cognitive organization of color sensations and its relation to linguistic classifiers), evaluation of performance in tasks involving color-coded information, and identification of cross-cultural cognitive universals of color appearance.