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Are there nontrivial constraints on colour categorization?

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Abstract: In this target article the following hypotheses are discussed: (1) Colour is autonomous: a perceptuolinguistic and behavioural universal. (2) It is completely described by three independent attributes: hue, brightness, and saturation: (3) Phenomenologically and psychophysically there are four unique hues: red, green, blue, and yellow; (4) The unique hues are underpinned by two opponent psychophysical and/or neuronal channels: red/green, blue/yellow. The relevant literature is reviewed. We conclude: (i) Psychophysics and neurophysiology fail to set nontrivial constraints on colour categorization. (ii) Linguistic evidence provides no grounds for the universality of basic colour categories. (iii) Neither the opponent hues red/green, blue/yellow nor hue, brightness, and saturation are intrinsic to a universal concept of colour. (iv) Colour is not autonomous.

Keywords: categorization; colour; opponent processes; perception; universals

1. Introduction

In this target article we discuss whether colour is a case of scientific ekphrasis. The term *ekphrasis* refers to a device used by Greek and Roman writers to describe works of art that did not necessarily exist. Such accounts were not mere literary exercises but lavished detail on the most desirable, eloquent, and moving qualities a work might possess. Not quite wishful thinking or ideal types, these descriptions tell us much about the values towards which practitioners strove, which ideologies were applauded, and what kinds of rhetorical strategies were persuasive within those socio-historical milieus.

When linking propositions between theories of language, vision, and biology are taken to set nontrivial constraints on colour categorization, we think there may be an analogy with *ekphrasis.* The following quotations exemplify the sorts of links that have been claimed. "Hue boundaries, as well as hue foci are given by the biological sensitivity of the organism . . . [T]here exist natural divisions of the spectrum" (Bornstein 1987, p. 291; 1985); "beyond the shadow of doubt, the senses of basic colour terms are determined by the perceived characteristics of their denotata" (Allen 1986, p. 120); "[t]he epigenetic constraints in color perception are reflected in the verbal color classifications employed in the languages of all cultures thus far studied" (Lumsden & Wilson 1981, p. 45). In short, "the basic linguistic categories themselves have been induced by perceptual saliences common to the human race . . . biology determines phenomenology and, in consequence, a piece of semantic structure" (Hardin 1988, pp. 168, 156). We therefore examine the following hypotheses:

1. Colour is autonomous in the two senses of being subserved by distinct neural mechanisms and being readily abstracted as a property of objects independent of other properties by all human beings. It is therefore a perceptuolinguistic and behavioural universal, subserved by a distinct physical domain.
2. Colour is completely described by three independent attributes: hue, saturation, brightness.
3. There are four unique hues: red, green, blue, and yellow.
4. The unique hues are underpinned by two opponent psychophysical and/or neuronal channels: red/green, blue/yellow.

In reviewing these assumptions the following terminology is used. A *unique hue* (*Urfarbe*) cannot be described by hue names other than its own. Unique, unitary, principal, simple, purest, and primitive are synonyms. It is generally accepted that there are two pairs of opponent unique hues (red/green, blue/yellow). Often two unique brightness or lightness terms, black and white, are added. Unique colours should be distinguished from invariant, primary, and basic colours. Invariant hues are defined as those not affected by changes in luminance (luminance being the physical correlate of phenomenal brightness/lightness). A set of primaries is a minimal set of coloured lights, mixtures of which can match any other colour perceptually. Basic colours are the referents of Basic Colour Terms, which are identified by linguistic and/or psychological criteria (see sect. 2.1).

Although not the focus of concern, a central problem in reviewing evidence for the four assumptions is the relation between language and vision. Linguistic evidence has often

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been the starting point for research on colour. For example, a unique hue is defined by international convention as a “perceived hue that cannot be further described by the use of hue names other than its own” (CIE 1987; cf. Wyszecki & Stiles 1982). This definition is derived from Hering’s suggestion that “language has long since singled out red, yellow, green, and blue as the principal colors” (Hering 1964, p. 48). In a cross-cultural context the point was first made by Ladd-Franklin (1901, p. 400): “the acute tribe of Eskimo examined by Mr. Rivers have discovered for themselves that red, yellow, green and blue (and no other colors) are of a unitary character.” Although colour vision scientists tend to stress nonlinguistic evidence for the unique hues, throughout the literature unique hues are introduced in linguistic terms as in the CIE definition; see Hardin 1988, p. 66; Hurvich 1981, pp. 1–11, 53; Kuehni 1983, p. 39; Lennie & D’Zmura 1988, p. 337; Pokorny et al. 1991, p. 44; Quinn et al. 1988. This has methodological ramifications. A distinction must be made between nonlinguistic evidence being *consistent* with the received view and it providing *independent* evidence for it.

In section 2 the evidence most frequently cited for the universality of basic colours is reviewed. In sections 3 to 6 empirical evidence and justification for the four assumptions are examined from most specific (opponent pairs) to most general (autonomy of colour). Some evidence is relevant to more than one assumption. We conclude there is inadequate support for all four assumptions.

To avoid misunderstanding, we emphasize that we do not argue for the following hypotheses: (1) all researchers publishing on colour naming, categorization, or vision accept one or more of the four hypotheses; (2) there are no mechanisms concerned with colour vision; (3) in English more than four colour names are needed to describe the character of bright colour viewed in a dark surround; (4) relativism and unconstrained plasticity should prevail; the right approach is hermeneutics and/or social constructivism.

In this target article we examine the propositions listed above and conclude that there is little evidence for them. They turn out to be quite soft targets. In each subfield, although we make our own evaluations, there are respected local experts who have already made devastating criticisms. What we have here is an apparently coherent story of the type that most scientists would like to believe in, but is seriously entertained only because people over charitably assume that the parts in which they are *not* experts are sound.

2. The evidence for universality

2.1. Linguistic universals. In many disciplines Berlin and Kay’s (1969) *Basic Color Terms* is cited as support for the cross-cultural universality of a fixed number of basic colour categories. The exact meaning of basic colour terms (henceforth BCTs) has never been spelled out. Here is how BCTs might be understood in different contexts. The referent could be a set of colour chips (colour-in-the-world), a set of neurons in the brain or a functionally defined term in language-of-thought (colour-in-the-head), words in different languages labelling basic colours (colour semantics), or the experience or sensation associated with basic colour categories (phenomenal colour). As basic colours are claimed to be universals, the exact referent of a BCT is irrelevant because all levels are connected by linking propositions standing in one-to-one correspondence. A BCT names it all.

Berlin and Kay’s (1969) goal was to refute relativism. Drawing on an established research tradition (Brown & Lenneberg 1954; Lenneberg 1953; Lenneberg & Roberts 1956),[1] they aimed to show that colour naming was not linguistically constrained. Testing 20 languages experimentally they showed that the BCTs of different languages were congruent when identified by their “best examples” or foci. They proposed that all languages had words referring to the same 2 to 11 foci (labelled by BCTs). The claim to universality was based primarily on the clustering of these foci across the 20 languages. These experiments were further amplified by data on 78 languages gathered from dictionaries, ethnographies, and personal communications. A fixed, unilinear, evolutionary sequence prompted by environmental triggers explained why all languages did not automatically have 11 BCTs. A typology of seven evolutionary stages was proposed: WHITE/BLACK, RED, GREEN/yellow or YELLOW/green, blue, brown, purple/pink/orange/grey. A slash indicates equal probabilities of evolution; upper case terms refer to composite categories. For example in a language with three BCTs, the composite category RED agglomerates red, yellow, orange, pink, and purple. WHITE includes all light hues. BLACK includes blue and green. In a language with five BCTs, GREEN covers greens and blues until the BCT for blue evolves.

In general the theory received enthusiastic peer acclaim. Berlin and Kay’s results were assimilated in a variety of disciplines (see, for example, Allen 1986; Bickerton 1981; Billmeyer 1992; Brown 1991; Franzen 1990; Gardner 1987; Hardin 1988; Holenstein 1985; Miller & Johnson-Laird 1976; Pokorny et al. 1991; Sahlins 1976). It prompted the claim that the evolutionary sequence constituted “a primary epigenetic rule serving color category development” linking genes, neurons, and the evolutionary development of macrocognitive behaviour (Lumsden 1985, p. 5808). There are, however, many problems with this theory.[2]

Berlin and Kay presented their results as an empirical discovery. They proposed linguistic universals defined by the existence of 11 BCTs, the clustering of foci and the evolutionary sequence. Although the theory was thus received in the cognitive sciences and elsewhere, doubts can be raised about the empirical status of this theory. We find we can only understand this work on the assumption that Berlin and Kay had a strong *a priori* belief that just as “biological foundations of . . . language . . . must exist for syntax and phonology” so “basic color lexicons suggest such connections are also . . . found . . . in the realm of semantics” (Berlin & Kay 1969, p. 109f). In support of this contention we offer the following:

Berlin and Kay assumed that the perceptuolinguistic basic colour system is innate, biologically constrained, and (semi-) automatic. In the absence of any reason to suspect members of other speech communities having different automatisms, they felt justified in taking the American English colour lexicon as a standard. Experiments were set up in such a way that performance could be transposed into competence through a generating or translation rule. This revealed that at the meta-level, as in American English’s, there were exactly eleven BCTs. Although it is suggested that BCTs were the *result* of cross-cultural empirical research, this lexicon was in fact derived from the most popular American-English colour terms in Thorndike's & Lorge's *The Teacher's Word Book of 30,000 Words* (via Brown & Lenneberg 1954).

Of the 20 languages for which Berlin and Kay gathered data, 19 were represented by one bilingual speaker only, leaving out of consideration from the start any true cross-cultural significance. (The language studied in the field, Tzeltal, was being studied by Berlin for ethnobotanical classifications.) Methodologically, one bilingual speaker per language cannot be considered empirically adequate.

Despite the general acclaim for the theory, most detailed reviews of Berlin and Kay (1969) were critical of their methods of gathering and/or presenting data. There is an appearance of sloppiness that cannot but reduce one's confidence in their conclusions (see Durbin 1972; Hickerson 1971; and Newcomer & Faris 1971).[3] For example, apart from many printing errors and mislabelled colours in the mapping diagrams, there were also ethnographic errors and phonemic mistranscriptions. No straightforward information on the informant sample was provided and the choice of languages was not justified. In their use of data from the literature, Berlin and Kay seem to have used whatever came to hand. In reviewing the methodology and nature of the data, Durbin (1972, p. 259) concluded "the reliability and validity of the experiments are zero."

Several commentators pointed out that restrictions on the BCTs remove most of the world's colour vocabulary (see Panoff-Eliet 1971; Sahlins 1976; Shweder & Bourne 1984, p. 160). Moreover the defining criteria for BCTs were extremely plastic (see Bousfield 1979; Durbin 1972; Hickerson 1971; Kim 1985; Mervis & Roth 1980; Moss 1989a; Snow 1971; Wescott 1970; Zimmer 1982; Zollinger 1984; also Hamp's 1980 response to Branstetter 1977). For example, a whole industry developed to determine how many BCTs Russian has for blue (1 or 2?) and purple (0, 1, or 5?) (see Corbett & Morgan 1988; Davies et al. 1991; Morgan & Corbett 1989; Moss 1989b; Moss et al. 1990). Using Berlin and Kay's BCT criteria, Dournes (1978) reported that Jörai (Vietnam) has 23 BCTs (though no word translates as "colour"). Because of the fluidity of the rules, data manipulation is easy, as Hickerson (1971) illustrates. For example where RED is concerned, and no appropriate field gloss is forthcoming, a correction is made, as in Poto: *eyeyengo* (field gloss: yellow) becomes RED. As GREEN and YELLOW must precede BLUE in evolutionary emergence, so by the logic of the scheme, for Pukapuka, *yenga* (blue or yellow) must be YELLOW. By the same token, another correction is made for Daza: *zede* (jaune, bleu, vert) must be GREEN (see Berlin & Kay 1969, pp. 58, 72, 78).

Alternative explanations of tendencies to basic colour categories across languages were not considered. For example Tornay (1978a, p. xxxi) proposed the history of the progressive domination of the West and its values accounts for apparent universality. This seems a plausible suggestion with respect to what is often quoted as Berlin and Kay's most solid result: the clustering of foci. To repeat, this clustering was observed for speakers of 20 languages, 19 of which were represented by one bilingual speaker living in the San Francisco Bay area. According to Rosch Heider (1972a, p. 11) these were foreign students, being therefore literate and westernized.

Finally, although Berlin and/or Kay published various emendations to their theory, in particular to introduce more possible evolutionary sequences (Berlin & Berlin 1975; Kay 1975; Kay et al. 1991a), they have never addressed issues raised by their critics. In sections 4.3, 5.3, and 6.2 we present a range of empirical evidence that further undermines the validity, not only of Berlin and Kay's (1969) theory, but of their whole approach.

2.2. Perceptual universals. It was quickly established that many linguistic communities do not lexicalize 11 BCTs. This offered the opportunity to check whether BCTs were nevertheless in the head awaiting their evolutionary triggering. The work of Rosch (Rosch Heider 1971; 1972a; 1972b; 1973a; 1973b) is the most sustained exploration of this idea. Rosch hypothesized the foci of colour categories to be so perceptually salient as to draw attention more quickly and be more easily remembered. They would thus become more readily lexicalized. Learning colour names would merely be a matter of attaching labels to salient foci. These would be generalized to similar instances and accumulate in language and behaviour. These foci were "'natural prototypes' (rather like Platonic forms" (Rosch 1974, p. 114). It is this work on colour recognition, memory, and learning that led to the development of prototype theory (Rosch 1975; 1978). Our comments apply only to Rosch's work on colour.

Cross-cultural support for these natural prototypes was drawn from Rosch's work with the Dani people of Western New Guinea (Irian Jaya) who apparently used only two colour words *mili* and *mola*. The purpose of Rosch's investigation was to discover whether the foci of innate colour categories were learned faster and remembered better than nonfocals (Rosch Heider 1972a; 1972b; Rosch Heider & Olivier 1972). She hypothesized that the Dani might undergo a learning process recapitulating the Berlin and Kay evolutionary order (Rosch Heider 1972a, p. 13). Teaching the Dani people to label these innate colour categories, she concluded that categories containing the eleven prototype foci were the most easily remembered and the ease with which they were learned was roughly the same as Berlin and Kay's evolutionary sequence of BCTs.[4] However these experiments raise a number of questions (see Ratner 1989; Saunders 1992; 1995a; Saunders & van Brakel 1988).

First, Rosch did not inquire into what kind of words *mili* and *mola* were, assuming an unproblematic referent. However as this was not confirmed, she interpreted her results as requiring a modification of Berlin and Kay's Stage I. *Mili* and *mola* could not be translated as BLACK and WHITE, or DARK and LIGHT, or WARM and COOL as each term seemed to refer to both sides of the dichotomy (Rosch Heider 1972b).[5] Consequently, the division of the spectrum was neither one of hue nor of lightness. Furthermore, the most popular focus of *mola* was not white, but divided over two sorts of red. However Rosch raised no further questions about how such words were learned or used. Though linguists and ethnographers have pointed to complexities of the Dani language (K.G. Heider 1979; van der Stap 1966) suggesting that *mili* and *mola* are evaluatory words (K.G. Heider 1970, p. 175f), Rosch herself merely noted that the "Dani Ss tended to 'chant' the two names at a constant rate" (Rosch Heider 1972a, p. 16). This suggests there was more to their utterance than labels for the spectrum.[6]

Second, she noted that a problem in setting up the learning experiments was that the "Dani would not learn nonsense words" (Rosch 1974, p. 114). The experiments could only be carried out when indigenous words were used. If the theory were correct it would be difficult to explain why the Dani were not willing to learn new labels for salient, natural, universal, ideal-types "reflecting the perceived world structure" (Rosch 1978, p. 29), samples of which were presented repeatedly.

Third, she notes that Dani people "were unwilling to designate one of the color chips as the most typical member" of three chips of related hues, one of which was a focal colour (Rosch Heider 1973a, p. 340). This would seem to undermine the universal salience of both focality and prototypicality. Rosch then attempted to show the universality of the colour space and its focal structure by carrying out memory experiments. These experiments *did* show that the Dani remembered focal colours better than nonfocal ones, as did Americans. However when asked to point out a focal colour shown 30 seconds before in an array of 160 colours, Dani people were mistaken 75% of the time, Americans 34%. If humankind has a biological sensitivity to focals, it is difficult to understand how this level of error, or the difference between the Dani and Americans, can be explained.

Fourth, what was confirmed in Rosch's experiments (with Dani people and others) was the primacy of *focal* colours defined by saturation: "the most saturated colors were *the best examples* of basic colour names both for English speakers and for speakers of the other 10 languages represented" (Rosch Heider 1972a, p. 13, emphasis added) and "focal colors are all of the highest saturations available for that hue and value" (Rosch Heider 1972a, p. 19; see also Ratner 1989, p. 366). Given a particular hue category it would seem self-evident that the best example is the most saturated, because "most saturated" means "having most colour." At least one meaning of "being the best example" of something is having the most of whatever it is that it has. Unless experiments in which hue foci are identified independently of saturation are carried out, no conclusions can be drawn about the universality of foci.[7]

Fifth, a related weakness in Rosch's results was the pregiven nature of her colour categories. The most saturated exemplar of a colour was chosen not from a random array, but from shades of that colour fixed by herself. Participants were not asked to choose the best example of their own hue categories but only the best example of a hue category provided by Rosch.

Finally, although all Rosch's results are presented within the rhetoric of discovery none escapes her circular reasoning. For if the premise is that colour is both in the head and in the world then however that is experimentally realized will confirm (with more or less noise) that premise.

2.3. Infant and child development. According to one often-quoted study (Bornstein et al. 1976), four-month-old infants naturally partition the spectrum in four categories: red, green, yellow, and blue. As there are many problems involved in establishing just what an infant or other languageless creature is responding to (Brown 1990; Teller & Bornstein 1987), caution is needed in concluding that the spectrum is divided into four unequivocal bands. In support of this caution we offer the following circumstantial evidence.

First, special explanations have been invoked to explain why children do not easily learn correct colour words (Bornstein 1985). Correct usage begins between 3 and 8 years. Darwin (1877, p. 376) worried about this, observing the slow development of his own children. Bornstein mentions Boynton's 12-year old son (Boynton & Gordon 1965) who "named a number of the same wavelengths blue, yellow, or some combination of blue and yellow" (Bornstein 1985, p. 77) In general the correctness of colour names improves after children go to school. Attempts at colour-to-name training before the age of four seem ineffective. Reviewing a large number of publications Bornstein says that although one would expect that "linguistic identification simply overlays perceptuocognitive organization," paradoxically the supposed perceptuocognitive organization does not facilitate semantic development (Bornstein 1985, p. 74). Moreover the order of acquisition and use of colour terms is reported to be "wholly idiosyncratic" (Bornstein 1985, p. 87). Nonetheless he claims that the terms that go with "the basic fourfold color-name organization of the spectrum" are acquired first. Preuss (1981) however, in a study with 2-year-olds at a university day-care, found *orange* headed the list of both comprehension and production tasks. Orange also came first as the colour with the highest memory accuracy for both Dani and American adults (Rosch Heider 1972a; 1972b).

As further support for categorical perception, Bornstein claimed that not only do infants categorize the spectrum as (American) adults do, they also display the same colour preferences: red/blue, green/yellow (Bornstein 1975). But adult colour preference is far from established: Is the preference for pleasantness, arousal, or conspicuousness (Wiegersma & van Loon 1989)? Is it for hue or saturation? Does the subject go for a strong, serious, or warm colour? Other studies reveal a variety of adult preference orderings both intra- and interculturally (see Davidoff 1991, pp. 115–19; Garth 1922; Helson & Lansford 1970; Martindale & Moore 1988; Wiegersma & van Loon 1989; Zöld et al. 1986). Although most studies report either blue or red as heading the preference order, no other study repeats Bornstein's ordering.

3. Opponent colours

3.1. The notion of opponency. Instead of the trichromatic theory of Young and Helmholtz (Helmholtz 1911; first published 1856–1866), Hering (1878, pp. 107–10) proposed six primitives: red/green, yellow/blue, and black/white. He claimed these three experiential opponent pairs reflect human neurophysiology. That is, there is a linking proposition between perceptual opponency and a mechanism at the neurological level (which is reflected in psychophysical models). Hering's theory was revived in psychophysics in a series of publications by Hurvich and Jameson in the 1950s (see Jameson & Hurvich 1955, and Hurvich & Jameson 1955).

Hering however was not the first to propose this theory. The concept of polarity had previously been formulated by Goethe. He claimed certain colours reciprocally evoke each other. In Goethe's colour circle there were *three* opponent hues: red/green, orange/blue and yellow/purple (Goethe 1840; Sepper 1988). Yet *if* the unique red/green, blue/yellow opponencies really were the timeless primitives of colour vision, why was so careful an investigator as Goethe unable to introspect them correctly?

Early research on colour opponent cells was carried out by De Valois et al. (1966) and De Valois and Jacobs (1968) at Berkeley where Berlin, Kay, and Rosch were working. Although they were not in personal contact (MacLaury, personal communication), colour research was clearly on the broader research agenda. Integration of its various strands might be suggested to have occurred with Kay and McDaniel (1978). Though such integration was intimated in Berlin and Kay (1969), Rosch (Rosch Heider 1971; 1972a; 1972b; 1973a; 1973b) did much to bring macrocolour naming and microreductive opponency together. Others who supported this move were Bornstein (1973a; 1973b; 1975); Ratliff (1976); Zollinger (1972; 1976) and Miller and Johnson-Laird (1976).

Using Hering and DeValois as validation, Kay and McDaniel (1978) claimed that the distinctive properties of the semantic categories black, white, red, green, yellow, and blue correspond *precisely* to the properties of fuzzy response functions describing the neural mechanisms that underlie colour vision. All other semantic colour categories derive either from fuzzy *unions* or fuzzy *intersections* among the six fundamental neural response categories. Intersections produce categories such as purple (red and blue) or brown (yellow and black), while unions give the composite colours such as blue-with-green. To reconcile opponent processes and basic colour terms/categories, Kay and McDaniel insisted that the categorization of the opponent colours developed according to the Berlin and Kay evolutionary rule. Where *unions* were found, the brain had not yet been prompted to produce the proper primitives of colour vision (evolutionary Stages I–IV); where *intersections* were found, the brain was well on its way to the full lexicalization of the colour space (Stages V–VII).

Although Mervis and Roth (1980) showed that Kay and McDaniel's fuzzy sets cannot differentiate basic from nonbasic colour categories, the neurophysiological underpinnings of Berlin and Kay (1969) by Kay and McDaniel's (1978) appeal to opponent unique colours is often quoted as received wisdom (for example Boster 1986; Lakoff 1987, pp. 26–30). In this way the belief in linking propositions between macrocolour names and the microfiring of neurons was made explicit.

3.2. Evidence from folk psychology. According to Hering, appeal can be made to tradition and folk psychology for the existence of opponent colours. Hardin (1988, pp. 40–45) quotes several experiments based on qualitative similarity judgments to support the existence of two pairs of opponent unique hues. But the interpretation of these results is not unequivocal (van Brakel 1993). Moreover it is dubious methodology to suppose that the phenomenological intuitions of twentieth century English speakers justify timeless generalizations.

The standard psychophysical explanation for opponency claims that the red/green channel either gives a positive or negative response (see sect. 3.3). There can thus be no red-green experience. However, the results of Crane and Piantanida (1983) suggest that whether or not this is so is an empirical matter. Teller (1984, p. 1239) shows the in-principle neurophysiological possibility of reddish-green. Moreover according to the colour circle, there are an infinite number of complementary colours with the same opponent property as blue/yellow and red/green.

Occasionally the naturalness of opponency is explained with reference to an intuitive synaesthesic warm/cool dichotomy, for which there is alleged psychological/phenomenal evidence (cf. Hardin 1988, pp. 129–31; Kay et al. 1991a). But, although the warm/cool opposition is well known in folk psychology, support for it is tenuous. Davidoff (1991, pp. 113–5) reviewing recent literature, cautions against associations with particular emotive values as they have no inter- or intracultural validity.

There is also empirical folk evidence *against* natural opponency. Because of a common root in Indo-European meaning "spring up," implying both "grow" and "glow," the same word may in different contexts have associations with red, golden, and green; in Sanskrit for example *hari* is translated as "reddish, golden, greenish" (Wood 1902, p. 37f). In medieval Christianity, red and green were thought of as interchangeable, as equal in value or as dual components of natural or mystical light. Similarly the Latin and French terms *glaucus*, *ceruleus*, and *bloi* could signify either blue or yellow (Gage 1993, p. 90) and there are "yellow, blue" words in other Indo-European languages (for example *plav* in old Serbo-Croatian; Herne 1954, p. 73f; Kristol 1978, p. 226). If the phenomenal self-evidence of opponency were true, there should be stronger historical evidence for it.

In conclusion, there appears to be no hard evidence for the *phenomenal* pervasiveness of colour opponency. However it can be argued that all this is irrelevant as opponency figures in models of colour vision. It is therefore to these models we now turn.

3.3. Psychophysical evidence. Generally it is thought there are three types of cones in the retina, each with a different photopigment and consequently different spectral sensitivity. The three cone types are maximally responsive to short (S), middle (M), and long (L) wavelengths of light. Monochromatic light at the maximum of the absorption curves for S, M, and L cones looks violet, blue-green, and yellow-green respectively. However individual cones are "colour blind," preserving no details of wavelength. Only from the outputs of different types of cones (responding to the same part of the field of vision) can information be extracted about the activating wavelength.

While the properties of the three types of cones are relatively uncontended, the evidence for the opponent process theory of colour is less clear. The psychophysical story is that there is one achromatic and two chromatic channels (Hurvich 1981). The first channel processes overall luminance within the broad range of spectral frequencies that excite L and M cones; this is the L + M channel (also called the brightness channel). The second channel processes the relative intensities of long and mid-spectral light, but is insensitive to absolute levels of illumination; this is the L/M channel (also called the red/green channel). Activity in the third channel is proportional to the difference between the activation of S cones and the combined activation of L and M cones; this is the S/L + M channel (also called the yellow/blue channel). But this simple psychophysical model is at odds with many experimental results.[8]

Discrepancies display themselves in psychophysical experiments primarily in the form of nonlinearities that are then attributed to various nonstandard interactions between cone types. Nonlinearities are very prominent in the yellow/blue channel and also occur in the red/green and brightness channels (see, for example, Ayama & Ikeda 1986; Boynton 1988; Burns et al. 1984; Ejima & Takahashi 1984; 1985; Ejima et al. 1986; Elsner et al. 1987; Lee et al. 1989; Lennie & D'Zmura 1988; Mollon & Sharpe 1983; Shevell & Humanski 1988; Suppes et al. 1990; Yaguchi & Ikeda 1982). There is no agreement whether there is S cone input to the L + M channel (i.e., whether the S cones contribute to the processing of luminance contrast), and it is unclear whether yellow input comes from the L or M cones or both (see Gouras 1984; Hess et al. 1989; Rabin & Adams 1992; Shevell 1992; Stockman et al. 1991; Stromeyer et al. 1991; and several contributors in Mollon & Sharpe 1983).

Consequently there have been many proposals for adjustments and modifications to the model. For example on a functional level it has been suggested that colour vision proceeds by a *mixture* of opponent and nonopponent channels (cf. sect. 6.1) and that different mechanisms process short- and long- wavelength parts of the spectrum that look reddish (Ingling et al. 1978; Paulus & Kröger-Paulus 1983; Zrenner 1983, p. 83). As tuning to wavelength may be drastically changed by size and duration, the degree of opponency becomes a variable. Multiple L/M channels (presumably corresponding to multiple cell types) have been proposed (see Finkelstein et al. 1990; Finkelstein & Hood 1984; Hood & Finkelstein 1983, p. 37; Webster & Mollon 1991, p. 238; Zrenner 1985). But this implies that the idea of a fixed spectral sensitivity function has to be given up and with it we have lost a simple explanation of why a light cannot appear red and green at the same time (Shevell & Handte 1983). In addition, it has been suggested that at some level, either there are more than three axes or the cardinal axes are more mutable than was previously thought – where "three axes" corresponds to one brightness dimension and two opponent hue channels (see Albright 1991; D'Zmura 1991; Flanagan et al. 1990; Hurlbert 1991; Krauskopf et al. 1982; 1986; Lennie et al. 1990; Webster & Mollon 1991).

One problem in assessing these anomalies is that describing standard summation and differencing of cone outputs requires empirical curve-fitting with a number of free parameters. The coefficients of the opponent-processes are calculated from absorbance data for the three cone types combined with chromatic-response curves. The latter are determined in experiments in which subjects are presented with spectral lights. First the four unique hues are fixed: The subject is asked which spectral light is pure blue, and so on. This sets the zero level for the L/M and S/L+M channels. The subject is then asked to mix particular combinations, say a red/yellow light, with so much of an opponent hue, say blue, that a unique hue is obtained (in this case, red). This is what is called a cancellation or differencing experiment: the blue cancels the yellow. Similar cancellation experiments are carried out for other combinations. Then the L/M and S/L + M cancellation curves are matched at the point of balanced orange. They are further matched with an achromatic response curve to calculate saturation. Let us pass over the problem of polymorphism and anomalous receptors (Jordan & Mollon 1993; MacNichol et al. 1983; Neitz & Jacobs 1990; Neitz et al. 1993), the complexities of determining the achromatic response curve, the fact that only spectral lights are used, or that a subject is presented with carefully selected (hence unnatural) viewing conditions (Hurvich 1981; 1985; Werner & Wooten 1979). The crucial question is: Why start with the four unique hues in the first place?

The evidence does not support the postulate of exactly three opponent channels of a specified sort. As argued in the next section, there is no neurophysiological evidence for a fixed number of opponent neural pathways. Psychophysically distinct channels might well emerge in particular types of well-determined experimental conditions, but these channels do not necessarily exist outside those conditions. Psychophysically speaking, there might be *many* opponent channels while neurophysiologically speaking there are none.

3.4. Neurophysiological evidence. Single-opponent cells among retinal and LGN ganglion cells are considered to display spatial opponency, wavelength opponency, or both (Gouras 1984). Such cells have receptive fields consisting of an on-or-off circular centre and an annular surround of the opposite sign, where the centre and surround are connected to different parts of the retina. Such cells do not respond to the absolute level of illumination, but rather to differences in illumination in different regions of their receptive field. If the same cone type subserves centre and surround, the cell is spectrally nonopponent. If centre and surround are connected to different cone types, the cell is also wavelength-opponent. Note that wavelength opponency might be a trivial consequence of the receptive field centre being driven by a single cone and the surround by more than one cone (Lennie & D'Zmura 1988, p. 372).

The suggestion has been made that some ganglion cells are L/M opponent and others are S/L+M opponent. These would thus form neural pathways corresponding to the psychophysical channels introduced above. But again, it is not that simple. As De Valois and Jacobs (1968) already noted, there are no neat correspondences between the psychophysical channels and neurophysiological pathways (cf. De Valois & De Valois 1993). It appears that the neutral point of the L/M cells (i.e., the cross-over point from excitatory to inhibitory responses) is not fixed in one spectral region. Even with identical spatial and temporal stimuli and identical adapting conditions, there can be considerable variation in the neutral point among individual cells. Zrenner (1983; 1985) gives a range of 420 to 650 nm for the L/M cross-over point (virtually the whole spectrum). If the results for many L/M cells are averaged, the "average cell" does give opponent signals when stimulated by L and M lights respectively and there are good grounds for talking about *opponent* cells. But that is a far cry from therebeing *individual* red/green opponent cells. For one thing, whether a cell behaves *as if* responding differently to a red or green light may depend on characteristics of the stimulus other than being red or green (see sect. 6.1). For another, it leaves unclear what the purpose of a variety of cells higher up the visual system is, which seem to duplicate the procedure.

If we look at the possibility of a blue/yellow neural pathway the situation is still more confused. As many as nine different ways have been reported for cones and rods to be connected to retinal ganglion cells (Gouras 1984). L/M cells predominate, but a small number of ganglion cells are definitely connected to S cones. They have been called blue/yellow opponent cells, but this means no more than that they are connected to S cones as well as to the L and/or M cones. They respond positively to blue/violet light, but it is not clear how they respond to a yellow signal (Gouras 1984; Zrenner 1983).

The suggestion that there is no neurophysiological evidence for the existence of exactly two pairs of opponent hues is not new. Gouras and Eggers (1984) deny Hering's opponent colour channels in the primate retinogeniculate pathway. D'Zmura (1991, p. 951) says "observers [also] possess chromatic detection mechanisms tuned to intermediate hues such as orange." Teller (1991, p. 530) suggests: "the retinal coding scheme requires further recoding if neurons fully worthy of the name red/green and (particularly) yellow/blue are to emerge. Such neurons have not yet been seen in primate visual systems, and no one knows where or whether they will ever be seen." Similarly Mollon (1992a) reviewing Davidoff (1991), claims the latter's appeal to "chromatically opponent neurons that signal redness and greenness or blueness and yellowness" is pseudophysiology because "the neurons he requires to substantiate his view are not those that have so far been found electrophysiologically in the visual pathway."

None of this denies the existence of a variety of opponent and other types of cells. That however is not the issue. The issue is that the alleged evidence for exactly *two* pairs of opponent hues is not well grounded.

We recognize that neurophysiological data are inherently error-prone due to fluctuations in the organism's level of arousal, alertness, anaesthesia, and so on, and that variability in the neutral point of a putative chromatic opponent channel may arise either because of measurement error or because two opponent channels simply do not exist. Nonetheless the neurophysiological evidence for red/green and blue/yellow opponent pathways is reviewed because the cautions and hesitancies of the neurophysiologist are frequently lost when adjacent disciplines adopt their findings. Nothing we have said diminishes the importance of neurophysiological research or the functional characterization of the visual tract.

4. Four primitive hues?

4.1. Intuitive phenomenal categorization and its dependence on surrounding culture. Textbooks state that Newton established the fact of seven colours in the natural spectrum or rainbow: red, orange, yellow, green, blue, indigo, and violet. How reliable is this scientific fact? Newton himself did not see seven colours:

> I held the Paper so that the Spectrum might fall upon this delineated figure . . . whilst an Assistant, whose Eyes for distinguishing Colours were more critical than mine, . . . noted the Confines of the Colours, that is . . . of the red . . . orange . . . yellow . . . green . . . blue . . . indigo . . . and . . . violet. (Newton 1952, p. 126; see also Gage 1993, Ch. 9; Topper 1990).

As Campbell (1983) notes: "If Newton's assistant had not been so eager to please his master our current textbooks would be different." Newton's assistant saw seven colours probably because the harmonic series of Pythagoras still dominated mathematical thinking. Newton himself (and many before and after) studied the analogy between the colour spectrum and musical chords (Gage 1993, Ch. 13). Not all historians of science agree that the analogy fixed the number of colours in the spectrum (Topper 1990). But one thing is clear: The reported number of colours in the spectrum was not determined by what Newton or anyone else saw.

What other evidence from direct observation do we have? Thomas Young at one time divided the spectrum into three primaries: red, yellow, and blue. In 1802 Wollaston reported to the Royal Society (Sherman 1981): "the colours into which a beam of white light is separable by refraction, appear to me neither 7 . . . , nor reducible . . . to 3 . . . but . . . [to] 4," namely, red, yellowish-green, blue, and violet. Young at once rejoined that red, green, and violet were the primitives. In 1822 Brewster claimed to have shown conclusively that the spectrum contains yellow too. But his experiments were unrepeatable (Sherman 1981; cf. Mach 1919, p. 53). According to Helmholtz, a spectrum short enough to be viewed in its entirety consists of four colours (red, green, blue, and violet), but added (1909, p. 117): "there are no real boundaries between the colours of the spectrum. These divisions are more or less capricious and largely the result of a mere love of calling things by name."

There is still disagreement about how many colours can be seen in the spectrum. The majority opinion seems to be that there are five: red, yellow, green, blue, and violet. This is also the hue family of the Munsell system. However Biernson (1972) claims that orange must be added, and Campbell (1983) doubts whether yellow can be seen between green and red (see also Duck 1987 and Kidder 1989). As to the colours of the rainbow Gage (1993, p. 93) suggests "the very delicacy of the transitions of the bow . . . makes it extremely hard to number and name the colours. This has made the phenomenon especially apt for interpretation according to any number of prevailing schemata."

But which colours one sees in the spectrum/rainbow is irrelevant to the question of how many unique hues there are. Unique hues would cause lines, not graded bands in the spectrum and no reason exists why people should use only unique hues to report what they see. The purpose of the example is to illustrate the difficulty of separating what is seen from theoretical presuppositions and prejudices. It would not be too farfetched to suggest that the observation of seven colours in the spectrum was fixed by a prevailing number-7-cosmology, comparable to cosmologies elsewhere.[9] Probably many will agree that the suggestion of Paritsis and Stewart that (1983, p. 109): "at the cortical level, colours are classified into seven classes of cells" is nonsense. But if in the twentieth century some scientific encyclopædiae illustrate a spectrum by the rhetorical *seven* bands of colour, then diagrams in current textbooks depicting the opponent pairs as red/green and yellow/blue must also be considered rhetorical.

4.2. Psychophysical evidence for four unique hues. A considerable range of wavelengths are identified as unique green, blue or yellow (whereas unique red, not being in the spectrum, raises questions about the operational meaning of a unique hue): unique blue is found between 462–496 nm, unique green between 488–545 nm, and unique yellow between 566–588 nm (Dimmick & Hubbard 1939; Schefrin & Werner 1990). These points cover the humanly visible spectral range apart from 21 nm of yellow-green. Moreover since Hering (1964, p. 58), uncertainty abounds as to whether or not brown is unique (Bartleson 1976; Fuld et al. 1983; Quinn et al. 1988). It is true that if one averages the data into a Standard Observer the unique hues become more constant – that, after all, is what averaging does. But the issue is not whether English speakers roughly agree on what green is or whether brown is, or is not, unique. The issue is whether asking people to point out the unique hues reveals anything more than their command of English (defined by the average speaker). Therefore quoting Sternheim and Boynton (1966) or others as having shown that English speakers can describe all spectral colours with the four unique-hue names misses the point. It is also unclear how to deal with people who do not agree with the average observer. According to the CIE definition, green is unique if it cannot be further described by hue names other than its own. But in the Euro-American tradition many people are trained to see green in terms of blue and yellow. They will say green should not count as an *Urfarbe*. If green is an *Urfarbe* "intuitively" then all arguments against the uniqueness of purple, orange, aqua, and so on fall away.

Although there is a considerable amount of nonlinguistic evidence for trichromaticity and some sort of opponent processing, it is less clear what exactly the nonlinguistic support is for four unique hues. There are many conceptual unclarities here that would justify a separate review. First, most measurement techniques rely on some sort of threshold detection not representative of ordinary colour vision at suprathreshold levels. Second, many experiments are carried out with spectral lights (as distinct from coloured surfaces), which are not representative of the chromatic world humans live in. (Neither of course are colour charts, as Klee [1961] amongst many others has shown.) Third, it is a matter of controversy whether the chromatically opponent channels suggested by additivity experiments are the same as those that account for the results of cancellation experiments. Finally, in introductions to the subject, the Bezold-Brücke phenomenon of invariant hues is usually quoted recycling data from Purdy's 1929 dissertation (Purdy 1931; 1937). These invariant hues are then assimilated to the unique hues (Hurvich 1981, p. 73). But Purdy (1931) himself noted that the invariant hues of the Bezold-Brücke effect are not the same as the unique hues (cf. Ejima & Takahashi 1984; 1985; Paulus & Kröger-Paulus 1983; Pokorny et al. 1991, p. 45; Suppes et al. 1990; Vos 1986; Zrenner 1985). It seems therefore premature to conclude that psychophysical evidence for four well defined unique hues or simple colours has been established.

4.3. Problems identifying the analytic mind. If four unique hues were a universal human perceptual grounding, cross-cultural research would confirm it. But empirical evidence for a fixed number of primitives (whether four or any other number) is utterly evanescent. Of the Munsell colour chips commonly used in cross-cultural experiments, 60–80% often remain unnamed (see Berlin et al. 1991). When presented with chips people get confused and give inconsistent answers. Tougher-minded subjects find the naming and/or categorization tasks absurd, not self-evident. For example Berlin and Berlin (1975, p. 85, n. 5) recount their difficulties with monolingual Peruvian Aguaruna informants. Many would simply stare at the array. Others who could bring themselves to place the pen to the plastic would begin drawing individual black circles around chip after chip often moving along some level of brightness, completely ignoring hue. Several attempted to provide a different name for each perceptually different chip, employing terms that later proved to be the names of trees, plant dyes, and parrot feathers. One informant, when asked to show where all the red chips were, took the pen and very carefully circled the entire board. Similar examples can be found in Berlin et al. (1991), Bulmer (1968), Conklin (1955), Friedl (1979), Kuschel and Monberg (1974), Luria (1976), Rivers (1903), and Saunders and van Brakel (1995).

The common solution to such problems is to conclude that lack of abstract colour categories is the result of evolutionary backwardness (see comments of field workers in Berlin et al. 1991). For example, in the nineteenth century, drawing on the absence of a word for blue in Ancient Greek, Hebrew, Aramaic, and Akkadian (Brenner 1982), it was argued that colour vocabularies must have had an evolutionarily determined physiological basis (Geiger 1871; 1872; Gladstone 1858; 1877; Magnus 1877). Although contested (Allen 1879; Kirchhoff 1879; Krause 1877; Magnus 1880; Rivers 1901; 1905; Titchener 1916; Virchow 1878; Woodworth 1905; 1910), Berlin and Kay (1969) nevertheless revived the idea in terms of evolutionary stages (though insisting on universal physiology). Kay (1977, p. 30) for example, stated the broad goal: "The direction of linguistic evolution is toward the precise and explicit speech of the analytic philosopher, the scientist and the bureaucrat." Using this analytic outlook, the highest stage of chromatic evolution is perhaps to divide the Munsell chart into four vertical bands (corresponding to the four unique hues; cf. Lumsden's 1985 presentation of an epigenetic rule for colour). Some speakers of Mixtec (MacLaury & Galloway 1988) and Shuswap (MacLaury 1987) divide the Munsell chart into *three* vertical hue bands and thus approach this level of analyticity. But what about horizontal rather than vertical bands? MacLaury (1992, p. 151) reports data from speakers who divide the chart into three horizontal bands. Are vertical bands more analytic than horizontal?

Why is calling the sky "blue" more analytic than calling it "light blue" (for example *celeste* in Mesoamerican languages; see Bolton 1978; Harkness 1973; MacLaury 1986; 1991; Mathiot 1979). Why is it more analytic than calling it "bright" as in Mursi (Turton 1978, p. 366), "clear, serene" as in Sanskrit (Hopkins 1883; cf. Wood 1902), "whitish" as in Batak (Magnus 1880), *verde* "green" as in Tlapenec (Dehouve 1978), or nothing at all (some Italian dialects: Kristol 1980, p. 142)? Many languages appear to differentiate between light and dark blue.[10] This converges with observations of western painters (e.g., Cezanne) who felt that chromatic blue has a double nature, related to light and dark. Dark blue and black are also frequently considered a unified entity (see Almquist 1883; Berlin et al. 1991; Forge 1970, p. 283; Gage 1978, p. 110; Gatschet 1879; Hopkins 1883; Jochelsen 1908; Rivers 1901; Tornay 1978b; Wierzbicka 1990; Wood 1902). Within the Berlin and Kay tradition however this is considered an earlier stage of evolution. But what makes unifying dark blue and black or separating light blue and dark blue an *inherently* primitive habit? That is, is hue discrimination more analytic than brightness/lightness discrimination? The burden of proof rests with those who wish to argue the point.

In many languages blues and greens are mapped together under one BCT (see Berlin et al. 1991; Bornstein 1973a; 1973b; 1975; MacLaury 1986). Less common is the problem of a yellow-with-green category. This was submerged in Berlin and Kay (1969), though long known. Of the literature to which they refer, Ray (1953), Rivers (1901), and Magnus (1880) mention it.[11] The yellow-with-green category is common in North America, in particular on the Northwest Pacific coast (see Gatschet 1879; Holmer 1954/55; Kinkade 1988; Proulx 1988). But yellow-with-green terms in Indo-European languages have also been discussed since the nineteenth century (see Schulze 1910; Schwentner 1915, p. 68; Weise 1878, p. 281). Proto-Indo-European *ghel – yellow, green, grey, and blue, from which yellow is derived – has cognates for green in daughter languages, for example Lithuanian *zelvas* (Pokorny 1959, p. 429–30). Some etymologies provide the gloss "yellow, green" for older Indo-European languages: for example, Sanskrit *harita*, Greek *chloros*, and old-Slavic *zelenu*. (see Filliozat 1957, p. 305f; Hopkins 1883, p. 175; Irwin 1974, p. 77; Pokorny 1959, p. 429; Rowe 1974, p. 341; Schulze 1910, p. 800; Schwentner 1915, p. 68).

Current linguistic interest in yellow-with-green was stirred by MacLaury (1987) who asserted that its presence in Shuswap "contradicts present physiological knowledge." Kwakw'ala (spoken on Vancouver Island and the adjacent mainland) also has one word for yellow-with-green: *lhenxa* (Saunders 1992). On the grounds of theory one would expect the anomaly to evaporate if speakers were reminded that yellow and green are two different *unique* colours, two of the four *built-in* opponent hues. But though most current speakers of Kwakw'ala are bilingual and know perfectly well the difference in *English* between yellow and green, they stick to *lhenxa*. The yellow-with-green category is particularly intransigent compared to say, *ayendzis* "orange" a recognized loan word from Chinook jargon, and *pinkstu* an obvious neologism (Saunders 1992). The counter-suggestion is to say that *lhenxa* supervenes on the union of two Urfarben at a deeper level. But then why do the innate categories always coincide with twentieth century American English? One reason might be that linguistic research tends to confirm the cross-cultural validity of Euro-American categories by imposing them on non-written languages when first inscribed. Boas for example, invariably glossed *lhenxa* as "green" (Boas 1892; 1931; 1934; Boas & Hunt 1905), as did Curtis (1915). But Dawson (1887), Grubb (1977), and Lincoln and Rath (1980) provide the more nuanced yellow-with-green gloss (Saunders & van Brakel 1996). Similar problems arise with the history of European languages. But the contingency and sociohistory of current colour categories in English fails to be appreciated in the colour science literature.

In conclusion, before deciding there is *scientific* evidence for four unique hues (or any other number of unique or basic colours), it is necessary to be sure one is not simply fitting one's data to modern English.

5. Hue, brightness, and saturation

5.1. Attributes of colour. It is generally assumed that colour has three independent psychological dimensions: hue, brightness, and saturation (Munsell's *hue*, *value*, and *chroma*). We take this assumption to be an empirical statement about the properties of colour. If colour is *defined* as hue, brightness and saturation, our comments dispute the autonomy of colour.

Narrowly construed brightness tends to be used both for lights and for surface colours viewed in aperture mode. The corresponding term for the appearance of surface colours is lightness. In ordinary language, words like shade, tint, and colour tend to be used for *hue*. Alternative adjectival forms for *lightness* are light, bright, pale, colourless, shaded, whitish; similarly for *saturation*: deep, dense, intense, vivid, pure, lustrous, faded, lacklustre, permeated, infused, full, vibrant, dull. But it is notoriously difficult to separate saturation and lightness, and even quite technical words tend to overlap in these dimensions; for example faded, bright, and lacklustre might be considered both lightness and saturation terms. In addition, the status of the concept of saturation when applied to lights is unclear, particularly in the case of negative contrast.

There are various standardized systems of colour classification and measurement (Derefeldt 1991); for example Munsell, NCS, DIN, OSA/UCS, and CIE. The Swedish NCS is most in line with Hering's ideas of opponency, unique hues, and a natural order. Munsell however is the most widely used system in the English-speaking world. The chips representing the colour space eliminate all aspects of the location of objects, their surfaces, and their relations in the world – the difference between related and unrelated colours. They remove such features as duration, size, texture, glossiness, lustre, fluctuation, flicker, sparkle, glitter, shape, insistence, pronouncedness, brilliance, fluorescence, glow, iridescence, colourfulness, nuance, background or surround colour – all of which have been proposed as specific attributes of particular coloured surfaces or volumes (see Beck 1972; Evans 1974; Gibson 1979, p. 31; Hunt 1977; Pokorny et al. 1991; Pointer 1980). Preliminary research on colour phenomenology from satellites suggests there may yet be more differences (Vasyutin & Tishchenko 1989). As Pokorny et al. (1991, p. 45) say: "No comprehensive theory of colour appearance can be based only on the properties (hue, saturation and brightness) of unrelated colours." Further, there is much unclarity about the (inter)relation of surface colours (as modelled by the Munsell system) and colours in other modes of appearance (Beck 1972; Katz 1935; Nickerson & Newhall 1943).

What experimental support is there for the assumption that colour in daily life consists of three psychologically salient components: hue, brightness, and saturation? Burns and Shepp (1988) argue that there are serious problems about a three-dimensional spatial metrics as the proper psychological dimension of colour vision. They review evidence that physical attributes of colour do not independently affect the psychological dimensions. Chang and Carroll (1980) suggest that the psychological colour space has seven (± 1) dimensions. Work on the OSA system has shown it impossible to represent colour in Euclidean space (Nickerson 1981; Man & MacAdam 1989). None of the existing systems of colour classification achieves the goal of uniform perceptual intervals between any two adjacent colours (Derefeldt 1991; Indow 1988). Therefore hue, brightness, and saturation, notwithstanding their usefulness for particular technical purposes, can only be claimed to describe the Munsell and similarly artificial colour spaces.

5.2. Interdependence of hue, brightness, and saturation. Because of the Bezold-Brücke and Abney effects there seems little doubt that hue, brightness, and saturation are interdependent. This undermines the appeal to linking propositions connecting the phenomenal and physical description of colour. There is no consensus on the relation between luminance, brightness, lightness and/or whiteness (see Beck 1972; Boynton 1988; Heggelund 1992; Pokorny et al. 1991; Whittle 1994). Definitions of non-hue attributes differ in different systems (Derefeldt 1991). Two stimuli matching for CIE-defined luminance may differ radically in brightness (Ikeda & Nakano 1986; Wyszecki & Stiles 1982, pp. 411–20). Boynton (1988, p. 82) quotes a report of Ware and Cowan in which 63 studies of the brightness-luminance phenomenon are fitted to a fourth-order polynomial to calculate the brightness/luminance ratio for any chromaticity. The brightness/luminance discrepancy depends on both saturation and dominant wavelength (Uchikawa et al. 1984). In addition, two separate neural mechanisms for brightness induction have been proposed (Shevell et al. 1992). Similarly purity (the physical correlate of psychological saturation) fails to dovetail with saturation in any clear way (Hunt 1977; Pridmore 1990). A saturated colour is perceived as brighter than a desaturated one when the two are equated in luminance (Yaguchi & Ikeda 1983).

Lockhead (1992) reviews an impressive range of studies showing how brightness judgments are subject to successive, simultaneous, and other contextual variables, raising doubts about a psychophysical law for brightness. According to Davidoff (1992; 1991) perceiving hue, lightness, and saturation requires elaborate training to separate their interactions with any success: “for the naive [subjects] lightness and saturation are not independent” (Davidoff 1974, p. 79; cf. Burns & Shepp 1988). Being *integral* (that is unified as a whole in vision), it is unsurprising that hue and lightness, hue and saturation, and lightness and saturation cannot easily be separated (Pokorny et al. 1991; Saunders 1992).

Further problems arise with the psychological difference between black/white, dark/light, and dull/bright. For example chromatic surface colours may appear equally greyish, although their lightnesses (or brightnesses) differ (Beck 1972; Derefeldt 1991; Hering 1964; Katz 1935). Heggelund (1992; 1993) reminds us that Hering had already noted the bi-dimensionality of grey colours. No consensus exists about whether the ordinary words “black” and “white” refer to colour, brightness, or something else. There is difficulty in imagining a context-independent scale for brightness or whiteness comparable to say, length (Smith & Shera 1992). It has been suggested that brain grey, *Eigengrau*, is “the intrinsic basal sensation associated with the equilibrium condition of the entire visual system” (Hurvich 1985, p. 66). This would suggest that the natural zero for brightness is not black or absolute darkness, but brain grey. Psychophysically the status of (induced) blackness is unclear (Lee at al. 1989; Volbrecht et al. 1990). A white surface may be defined as a surface that equally reflects at every wavelength across the spectrum. But that offers no help for the psychological differences here under review and leaves out for example the luminous-whitish properties seen on white light sources (Heggelund 1993).

5.3. The primacy of hue. At the cross-cultural level things are no better. At various times a dominance of brightness (as distinct from hue) classification in tropical areas has been proposed (Simon 1951; Van Wijk 1959). It has been suggested that people in those areas have different macula and lens pigments (Bornstein 1973c; Rivers 1901). But this hypothesis has no physiological basis. Recently MacLaury (1992) has revived brightness classification as an early evolutionary stage, though he disclaims physiological implications.

People in the Euro-American world are trained to distinguish hue. Cross-cultural research reveals the distinction to be contingent–hue, we must conclude, not being naturally salient. This is supported by numerous translation problems. (Note however that current translational choice is limited to hue, brightness, and saturation, these being the only parameters recognized under the Munsell scheme.) For example, hue/brightness problems arise in translating Sanskrit (Hopkins 1883) and Arabic (Fischer 1965, Gätje 1967). Cases such as Sudanese and Arabic green or blue skin (Bender 1983) must be considered metaphors. Homeric Greek, discussed in numerous publications, presents intractable problems for the hue/brightness distinction (Hickerson 1983, Irwin 1974, Maxwell-Stuart 1981). Skard (1946) gives more than 50 sources discussing these problems in pre-1940 literature, and Maxwell-Stuart (1981) needs 200 pages to discuss the uses of *glaukos*. Ancient Greek colour terms are problematic because they have more to do with brilliance and lustre than with hue. Sensitivity to, and the importance of gloss and glitter descriptions in the Homeric poems should alert any reader to dimensions of temporality and movement as distinct from *stasis* in the use of these terms (and the same would seem to apply to Sanskrit: Hopkins 1883). This goes much further than saying the ancient Greeks were more interested in brightness than hue.

Hence the *inherent* independence and/or salience of hue (or brightness) does not seem a well-supported conclusion in the cross-cultural data. This brings us to the more general issue of the autonomy of the colour domain.

6. The autonomy of colour

6.1. Cross-modal neurophysiology. In this section we briefly review neurophysiological evidence. Our conclusion is that it fails to support the autonomy of colour. As noted in section 3.4, most ganglion cells (in the retina and the parvocellular layers of the LGN) are connected to both L and M cones. These L/M-cells cannot distinguish between a large coloured spot and a small white spot. Such cells can switch instantly to nonopponent processing, for example, if an object is presented only briefly. They show a surprising functional plasticity: in a way, they adapt to the purposes of looking. It has been realized that if in the simple opponent-process model both summing and differencing signals (both brightness and chromaticity) must go through a single neural pathway then “[t]he absence of a distinct ‘achromatic’ pathway is the most troublesome physiological finding” (Lennie & D’Zmura 1988, p. 372). This is especially so if one expects chromaticity and brightness to be neatly separated and in one-to-one correspondence at the psychophysical and neurophysiological level (see also Derrington et al. 1984; Estévez & Dijkhuis 1983; Finkelstein 1988; Gouras 1984; Gouras & Eggers 1984; Hood & Finkelstein 1983; Tansley et al. 1983; Zrenner 1983).

Recently, more emphasis has been placed on two distinct visual pathways from retina through LGN to visual cortex (see Hubel & Livingstone 1990; Lennie & D’Zmura 1988; Livingstone & Hubel 1984; 1988; Mollon 1989a; Zeki 1985). According to Shapley (1990), achromatic vision is identified with the *M pathway* (via the Magnocellular LGN) and chromatic vision with the *P pathway* (via the

Parvocellular LGN). There are however, a number of reasons why this is implausible (see in particular Mollon 1989). As the PLGN is much bigger than the MLGN, it is doubtful that it would only be implicated in wavelength detection; evidence exists that it also responds to motion and acuity (texture, fine pattern, and fine stereoscopy; see Albright 1991; Boynton 1988, p. 81; Gouras 1991; Hubel & Livingstone 1990; 1991; Ingling & Grigsby 1990; 1991; Ingling & Martinez 1985; Kooi & De Valois 1992; Krauskopf & Farell 1990; Merrigan 1989; Mollon 1989; 1990; Ohmura 1988; Schiller et al. 1990). Even Shapley (1990), a strong advocate of a version of chromatic/achromatic dualism and parallelism, admits that "(c)ooperative and suppressive interactions . . . demonstrate that these pathways may start in parallel but . . . converge." Hubel and Livingstone, often quoted for their claim that colour and form are processed separately in the visual cortex, do not deny that "colour information" may be used to "detect borders" and that "some opportunities for cross-talk exist" between the P- and M-pathways (Hubel & Livingstone 1990, p. 2223). Also, too little is known about the significance of the massive back-projections from all areas of the cortex to the thalamic nuclei (see Barlow 1990; DeYoe & Van Essen 1988; Steriade & Deschenes 1985; Zeki & Shipp 1988).

Psychophysical evidence suggests that space and wavelength are intricately linked, an issue related to the existence of nonlinearities in all three psychophysical channels (see sect. 3.3). There seems to be consensus that the chromatic channels contribute to brightness, at least above threshold levels (Ingling & Martinez 1985; Cole et al. 1990; Yaguchi & Ikeda 1983). Inputs originating from wavelength differences may go to the "motion channels" (cf. discussion on M- and P-pathways above), chromatic channels also displaying "orientation sensitivity" (see Bradley et al. 1988; Dobkins & Albright 1993; Flanagan et al. 1990; Javadna & Ruddock 1988; Shapley 1990). In addition there are rod-cone interactions (Ingling 1977; Montag & Boynton 1987; Zrenner 1983).

There is strong evidence that between retina and cortex, processing of wavelength is intricately mixed with luminosity, form, texture, movement response, and other environmental change. It is sometimes suggested that the value of colour vision is to pick up survival information from the environment. But why pick up *colour*? Answers could be: because colour contributes to object recognition, or: it contributes to identifying edible fruits, and so on. However, to arrive at the conclusion that the fruit is ripe, or over there, there is no unique need for colour (cf. Akins & Lamping 1992). For that we would need an antecedent argument for pre-ordained cosmic harmony (cf. Saunders 1995b; Shepard 1991)

At the level of individual cells there is no evidence for anything that might be called, even metaphorically, a "colour-coded" cell. Of the many cells that seem to contribute to colour vision, double-opponent cells in area V1 of the visual cortex are most sensitive to simultaneous presentation of stimuli of two different wavelengths, one covering the centre of the cell's visual field, the other illuminating the surround. What are called green off centre cells give a maximum response to a red spot surrounded by a green annulus. Such a cell is not influenced by a large homogeneous (chromatic or achromatic) light spot covering the entire receptive field. So double-opponent cells are claimed to be sensitive to wavelength *differences* only. But it would be premature to conclude that we have now found the locus of "colour-coded" cells.

First, there is a "bewildering variety of colour-coded cell types" (Livingstone & Hubel 1984, p. 348; cf. Lennie & D'Zmura 1988) and little is known about the organization of the receptive field of double-opponent cells, or about how they connect to cells in the LGN (Billock 1991; Zeki 1985). Experimental evidence is as disputable as it is difficult to isolate centre from surround (Shapley 1990, p. 647). This may explain why there is no agreement on the spatial properties of colour-opponent cells (Lennie & D'Zmura 1988, p. 376) or on the number of such cells in different areas of the visual cortex (Michael 1985; Zeki 1985). In general, wavelength sensitivity varies with the particular conditions under which the measurement is made.

Second, not only has the existence of double-opponent cells in area V1 been disputed (Lennie & D'Zmura 1988; Ts'o & Gilbert 1988), but there are also single-opponent cells, cells with single spectral sensitivity curves, and cells that only respond to wavelength in conjunction with lines or edges of particular orientations. Some researchers report columns or blobs in the visual cortex that respond solely to colour stimuli and not to white light; others dispute their existence (see Boynton 1988, p. 92; Livingston & Hubel 1984; 1988; Michael 1983; 1988); Tanaka et al. 1983; Zeki 1985).

Third, Zeki (1984; 1985) doubts that the double-opponent cells in V1 "code for colour." He believes double-opponent cells merely respond to a predominance of a signal from one cone type. Only in area V4 would we find the first truly "colour-coded cells" (i.e., cells that respond not merely to wavelength, but to context-effects and such). In this respect he has suggested that his account converges with Land's retinex theory (Land 1986). But Zeki's conclusion that neurons in V4 are chromatically more selective than those at lower levels is disputed by many (for critical discussion of Zeki's work, see Andersen et al. 1983; Boynton 1988, p. 93; Desimone et al. 1985; Gouras 1991, p. 189; Krüger & Fischer 1983; Lennie & D'Zmura 1988, p. 390; Schein et al. 1982). Moreover, Livingstone and Hubel (1988, p. 744) refer to a "dozen or so areas north of the striate cortex" of which the (colour) vision properties still need to be investigated (cf. Lennie et al. 1990).

By what criteria then should a cell be qualified as "colour-coded"? This already poses a problem on a purely technical level (Krüger & Fischer 1983, p. 295; Lennie & D'Zmura 1988, p. 382). For example, it is a common fallacy to say that a cell has a special coding role for a stimulus that makes it fire faster (see Estévez & Dijkhuis 1983; Hardin 1988, p. 56; Martin 1988, p. 383; Teller 1984), as we cannot exclude the possibility that the cell might respond similarly or even better to a stimulus for which it was not tested. Furthermore, neurophysiological support for theories of human colour vision has drawn primarily on experiments with monkeys. These monkeys tend to be anaesthetized, meaning that in addition to feeling no pain they are *seeing* nothing at all (Boynton 1988, p. 93, cf. Haenny et al. 1988, p. 245). This means that "the extra-retinal inputs to the prelunate cortex . . . would be reduced or eliminated and the whole state of control of the excitability of this cortical area might be different, leaving the cells with their pure afferent sensory inputs that originate from the retina" (Krüger & Fischer 1983, p. 293). This touches on the more fundamental problem of how to make a *principled* division between active *colour see-ers* and *passive wavelength responders*.

6.2. The disintegration of the colour concept. Examples from non-western languages where hue plays a less prominent role than it does in English have already been given. The imperative of a clear-cut colour category disintegrates further when aspects of hue are referred to in ways that are vaguely chromatically individuated *in some contexts*. Examples in English are gold, silver, rosy, and blond. Similarly there are many languages that seem to refer mainly or solely to colour change (cf. Zaidi & Halevy 1993), or to indicate something of the interrelationship of growth and maturity, or to the interaction of colour with evaluation.

Consider the languages of African pastoral cultures, whose colour vocabularies are claimed to have a large number of BCTs (see Berlin & Kay 1969 – for the Bedauye, p. 83; Masai, p. 85; Bari, p. 87; Dinka, p. 93; Nandi, p. 98). Magnus (1880) had already noted that Xhosa people distinguished twenty six cattle colours, though there were no words for blue and green. Many subsequent studies have addressed the difficulty of separating colour and cattle idiom. See Evans-Pritchard (1933–35) on Ngok Dinka; Evans-Pritchard (1940) on Nuer; Lienhardt (1970) on Dinka; Fukui (1979) on Bodi; Tornay (1973; 1978c) on Nyangatom; Turton (1980) on Mursi. Obvious questions are whether a colour idiom is applied to cattle or a cattle idiom to colour, and whether it makes sense to enforce a distinction between colour and pattern vocabulary. Similar complexities arise where horses are important (see Centlivres-Demont & Centlivres 1978 for Uzbek; Hamayon 1978 for Mongol; Hess 1920 for Bedouin Arabic; Laude-Cirtautas 1961 for Turkish; Radloff 1871 for Kirgiz).

Second, common confusions arise when it is unclear whether a word is about colour appearance or aspects of growth. For example in Lokono (Arawak) there is *imoroto* unripe, immature, green, pale yellow, *koreto* ripe, mature, red, orange, deep yellow, and *bunaroto* overripe, overdone, brown, buff, tan, purple. Attempts to conclude that either colour is metaphorically extended to growth or vice versa, fail. Similar problems arise in many other languages (see Bulmer 1968 for Karam; Conklin 1955 for Hanunóo; Geddes 1946 for Fijian [Bauan]; Hickerson 1953, 1988 for Lokono; Juillerat 1978 for Amanab).

Third, consider Bellona colour words. Kuschel and Monberg (1974) began their research (based on dictionary data) with the assumption that Bellona had seven BCTs. But at the time of investigation the language was in a process of rapid change due to globalizing forces. Carrying out fieldwork in 1971/72, it was nonetheless possible to draw on pre-contact memories. Though for the younger generation the BCTs had the approximate meanings of white, black, red, yellow, blue, green, and brown/violet, for the older generation the situation was different. None of these words (nor the great variety of other Bellona colour words) could be used independently of context. They were not like secondary colour words (lemon yellow, poppy red, etc.), which can be used to name a colour on any object, but were more like words such as blond (applicable to hair, wood, or beer). In Bellona, such words were used for a range of properties, objects, situations or events (of varying degrees of abstractness or particularity) that to Western understanding appear disparate and unconnected. For example, one of the many words for blackness or darkness *lalangi* could be used of a dark night, black tattoos, flying foxes, and Melanesians from the West Solomons, but not of hair, whales, or fish.

Often Bellona words did not refer to colour as the property of an object, but to a process of change, such words being also frequently evaluative (like Dani *mili* and *mola*).[12] It is therefore unsurprising that most terms could not be mapped on the Munsell colour chart. The old uses have now disappeared, being replaced by Western ones. In this Bellona perfectly illustrates what Tornay (1978a, p. xxxi) described as the mistake of confusing the cultural evolution of mankind with the history of the progressive domination of Western practices.

7. Conclusion

The results of the preceding sections can be summarized as follows:

1. Neither structural nor functional opponency is denied. But neither neurophysiology nor psychophysics supports exactly two pairs of opponent hues or three pairs of opponent colours.

2. The history of western art and science and a cursory cross-cultural glance reveal that to rely on folk perception to establish four primitive hues, eleven basic colour categories, or any other perceptual categorization of colour held to be universal (due to salience, innateness, or whatever) is an unreliable procedure. There is no convincing evidence that particular colour primitives exist at any kind of prelinguistic, phenomenal, or biological level.

3. Psychologically there is nothing natural about either the combination or separation of hue, brightness, and saturation. There are no linking propositions between say, luminance and brightness.

4. There is little if any concrete evidence for a one-to-one correspondence between physiological pathways and psychophysical channels. Different functional channels may be (partly) embodied in the same set (pattern) of anatomic cells and different contexts may require different sets of functional channels. But there is no convincing neurophysiological evidence for an autonomous colour pathway.

5. There is no strong evidence for specific mechanisms corresponding to Hering's four unique hues.

We conclude that the evidence supporting the four hypotheses reviewed in this target article is at best inconclusive, at worst flawed.

The analogy between colour science and works of art in our introduction was not meant to suggest that colour research should be abandoned any more than painting and literature were in the face of ekphrasis. The allusion is intended as a reminder of the normative dimensions of colour research and of the deep problems of colour ontology, as an indicator of the slippage between precept and practice. It would be presumptuous to answer the question: Whither colour research? Our aim has been to address the plausibility of the four hypotheses. We do not conclude that research must cease because of messy data or lack of certainty. On the contrary, we suggest that colour scientists may wish to consider what consequences the implausibility of the four hypotheses may have for their own research.

ACKNOWLEDGMENT

This target article was originally submitted in 1993. Literature appearing since then has not been included systematically. We are grateful to the referees for their thoughtful and constructive criticisms. A special debt is owed to P. Whittle, whose intervention prevented a number of misunderstandings.

NOTES

1. See Saunders 1993 for an account of the history of the concept of "basic" in Basic Colour Term.

2. For summaries of the various criticisms, evaluation of the Berlin and Kay theory, and further references, see Saunders 1992; 1993; 1995a; 1996; Saunders & van Brakel 1988; 1995; 1996; van Brakel 1993.

3. The reviews of Conklin 1973 and Sahlins 1976, although critical, were more sympathetic.

4. Summarising the Rosch Heider experiments (1971, p. 453; 1972b, pp. 15–19), Saunders (1992, pp. 76–77) concludes that it is difficult to see what Rosch means by there being some evidence that the first four Berlin and Kay colours receive some statistical support.

5. Kay (1975) ignores Rosch's assertions and claims that *mola* and *mili* are WARM/COOL categories.

6. These experiments also raise the issue of the propriety of transposing laboratory methods into the field and what the status of the subjects is. For example Rosch Heider (1972a, p. 16) says "the testing was so arranged that only E's hands were visible to the S during testing."

7. Compare Collier (1973), Lucy and Shweder (1979), and Garro (1986) on the relation between foci and saturation. Collier et al.'s (1976) claims need to be reevaluated.

8. Though it could be said that there are as many psychophysical models as there are colour scientists, the account we present tends to be the one taken over by adjacent disciplines.

9. For three-colour cosmologies in African languages, see Jacobson-Widding (1979), Turner (1967), Whitely (1973), and Zahan (1974). For four-colour cosmologies common in South India, see Beck (1969). For five-colour cosmologies, see Baxter (1983), Beffa (1978), and Gernet (1957) for Chinese; Pritsak (1954) for Altaic; and Riley (1963) and von Kállay (1939) for Amerindian languages. For the eight-colour cosmology of Khmer, see Lewitz (1974) and Nepote and Khing (1978).

10. Some of the languages on the Northwest Pacific Coast mentioned by Kinkade (1988) could be interpreted thus (for example Bella Coola, N. Lushootseed, N. Straits Samish, Songish). In particular his reconstructed Proto-Salish would support the interpretation. It dovetails with Boas (1891), who collected "light blue" terms in the area (see Saunders & van Brakel 1996). Other possible examples of 2 BCTs for blue include Russian (Corbett & Morgan 1988; Morgan & Corbett 1989; Moss et al. 1990), Nepali (Bolton et al. 1980), some Indian languages (Furrell 1885; Rivers 1905), Urban Thai (Wierzbicka 1990), and ancient Greek (Irwin 1974, pp. 79–110; Maxwell-Stewart 1981, vol. 2).

11. Older sources referring to possible yellow-with-green or yellow-with-green-with-blue categories include Connolly (1897, p. 138) on Fanti; Spencer and Gillen (1927, p. 552) on Arunta; Bright (1952) on Karok (Karuk); Almquist (1883) on Chukchi. In recent literature, Forge (1970, p. 283) mentions a pale green powder used by the Abelam of New Guinea that is called yellow or blue; Senft (1987, pp. 329–30) mentions the Kilivila term *di-gadegila* – yellow, green, blue – which he claims shows a "pattern of confusion" (see also references in Berlin et al. 1991; MacLaury 1991a; van Brakel 1994a).

12. Compare also Monberg (1971) on Tikopia, Snow (1971) on Samoa, and further references on Polynesian languages by Branstetter (1977).

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Constraining color categories: The problem of the baby and the bath water

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Abstract: No crucial experiment demonstrates that four hue categories are needed to describe color appearance. Instead, converging lines of evidence suggest that the terms red, yellow, green, and blue are sufficient and precise enough for deriving color discrimination functions and for a useful model constraining relations between color appearance and neuronal responses. Such a model need not be based on linguistic universals. Until something better is available, this "standard model" holds.

We agree with much, perhaps most, of what is said in Saunders & van Brakel's (S&vB's) target article. And yet we disagree profoundly with the paper as a whole: It seems to be throwing out the baby with the dirty bath water. Practitioners on one side of the color fence (e.g., physiologists) are said to be aware of problems and hidden assumptions in their work but to accept the canonical versions offered by those on the other side (e.g., linguists), with the result that both sides accept a deficient structure. Although this may be true, we protest that not only are we aware of the skeletons hidden in our closets, but that we can also make educated guesses about what is amiss with the other side's work (e.g., see Abramov & Gordon 1994). Nonetheless, poking holes in everything about color vision has some value: it reminds us to question the "givens" of our fields. The value is greatly reduced, however, when, as in this case, no useful replacement is offered.

The criticisms are so scatter-shot that the target article's thrust is unclear. As we read it, it deals primarily with color appearance – is this "red," or "pink," or "teal" – rather than with color vision as a discriminative sense – is "this" different from "that," regardless of whether the difference is one of brightness, or hue, or anything else. The fundamental problems for color appearance are (1) the number and nature of basic color sensations, and (2) their linkage to specific neuronal mechanisms. These issues need not be intermixed, as they often are by S&vB. For example, it is argued that the number of color categories cannot be small because the distributions of spectral null points of spectrally opponent LGN cells are very broad and so it is not possible for each cell type to delineate a precisely bounded color category. First, more recent and precisely controlled studies find acceptably narrow distributions (Derrington et al. 1984; incidentally, the earlier paper by De Valois et al. [1966] was not part of some Berkeley zeitgeist. The work was done at, and published by Indiana University). Second, and more important, this confuses color channels (neurons whose responses preserve some information about the spectral content of a stimulus – usually, spectrally opponent neurons) and entities we have termed "hue mechanisms," by which we mean neurons, or collections of neurons, whose responses are directly linked with sensations. Indeed, color terms are often used to refer to the responses of ganglion, LGN, and cortical neurons. But it is also now widely acknowledged that this is at best loose terminology and none of these neurons is a hue mechanism. Each violates the linking proposition that we experience as specific hue when a specific neuron responds and that we experience the hue whenever it responds. But all the recorded neurons respond, at least to some degree, to white light, which is an achromatic sensation (Abramov & Gordon 1994; De Valois & De Valois 1993; Jacobs 1993). The responses of all these neurons are not epiphenomena, however; color vision requires the responses of spectrally opponent neurons: when a group of such neurons in the LGN is ablated, the corresponding region of the visual field is not totally blind, but color discrimination is lost (Merigan et al. 1991).

For psychophysiological models of color (especially hue) we must agree on a set of color sensations. We, and others, have argued for a limited set of color categories: color probably evolved to highlight and signal the properties of species-important objects (e.g., ripeness of a fruit); as such, it would be useful if objects continued to fall within the same categories despite normal variations in illumination, size, and position of the retinal image (Jacobs 1993; Mollon 1989a). In addition, each retinal region must be capable of dealing with images – color vision is well-represented across much of the retina, even though its size scale increases with eccentricity from the fovea (Abramov et al. 1991).

We accept the four canonical fuzzy sets of red, yellow, green, and blue. There is no essential set of deductions or crucial experiment, but using this set of terms as orthogonal, bipolar axes of color appearance space (e.g., +R-G vs. +Y-B, with arbitrary assignment of signs) yields a "good" model in that it relates many seemingly disparate observations and is a viable framework until something better replaces it. Some of the lines of evidence that converge on our conclusion include: (1) Spectral discrimination functions can be derived from direct estimation of sensory magnitudes using these linguistic categories (e.g., Abramov et al. 1990; Chan et al 1991); (2) even though many color terms have precise spectral denotations, these categories are sufficient for a full description of color appearance (e.g., Sternheim & Boynton 1966, a very important paper given short shrift by S&vB); (3) the hues of wavelengths between these category boundaries can vary considerably with stimulus conditions (size, intensity, etc.), but the wavelengths of the boundaries themselves (unique hues) are quite robust even when stimulus purity is greatly reduced (Abramov et al. 1996). Using other hues as the cardinal points of hue space is possible in principle (e.g., +Plum-Chartreuse vs. +Orange-Teal), but neither we nor our psychophysical observers can conceive how to apply such terms to scaling our sensations. True, our conclusions are based on English, but if these terms allow us to examine color sensations precisely and guide us in relating sensations to neuronal responses, then we accept them as useful terms. It would be nice if they also denoted linguistic universals, but that is not essential. Although we are persuaded that there is a very great commonality in the denotations of their lexical equivalents across languages, we add the minor datum that bilingual subjects who did not speak English during their formative years use these four terms with the same precision and spectral congruence as native English speakers (Gordon et al. 1994).

Selective vision

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Abstract: The physics of color and the psychology of color naming are not isomorphic. Physically, the spectrum is *continuous* with regard to wavelength – one point in the spectrum differs from another only by the amount of wavelength difference. Psychologically, hue is *categorical* – colors change qualitatively from one wavelength region to another. The psychological characterization of hue that characterizes color vision has been revealed in a series of modern psychophysical studies with human adults and infants and with various infrahuman species, including vertebrates and invertebrates. These biopsychological data supplant an older psycholinguistic and anthropological literature that posited that language and culture alone influence perceptual processes; language and culture may modify color naming beyond basic categorizations.

1. Hue categorization. Physical attributes and properties in the world are complex and constantly in flux. However, our perceptions are in some measure organized so that psychological discreteness, coherence, and stability cope with physical continuity, variety, and instability. For example, a narrow band of the continuous radiant spectrum of electromagnetic energy – between 400 and 700 nanometers – is visible: we call it "light." For normal observers, an impressive quality of visible light is, as Sir Isaac Newton (1761–1772) observed long ago, its color. Viewing the spectrum today, we are impressed (as was Newton) that ranges of the spectrum are characteristically dominated by different colors, *categories of hue,* even though it is apparent that these categories contain mixtures of two (or more) hues (e.g., the short-wave end of the green category is bluish; the long-wave end is yellowish).

Boynton and Gordon (1965) asked native English-speaking adult observers to apply four basic color terms – blue, green, yellow, and red – singly or in pairs to describe wavelengths presented to them from across the visible spectrum. The results of their color-naming experiment are displayed in Figure 1(E). These data lend quantitative support to the foregoing central observation about the qualitative appearance of the chromatic spectrum. Boynton and Gordon's observers were satisfied to name all the visible wavelengths using one or two of the four basic color terms the experiment permitted. The authors reported a high degree of inter-observer reliability in this judgment task. Boynton showed in other experiments that when secondary color terms – such as orange or violet – are permitted, observers use them less

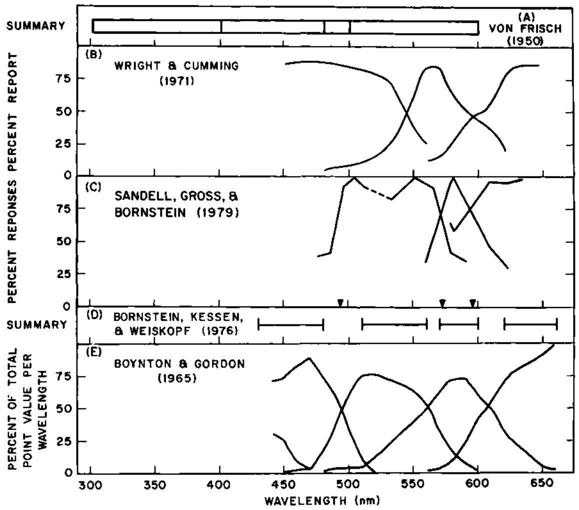

Figure 1 (Bornstein). Hue categorization in relation to wavelength for several species. (A) Categorization by landings on novel-color sugar dishes in the *honeybee.* (B) Categorization by matching probe wavelengths to training wavelengths in the *pigeon.* (C) Categorization by generalization to novel wavelengths in extinction in the *monkey.* (The dashed line is an interpolation. Arrows indicate cross-over points of adult human color-naming functions from the same study.) (D) Categorization by generalization of habituation to novel wavelengths in the *human infant.* (E) Categorization by color naming in the *human adult.* (Left to right: blue, green, yellow, and red. The increasing function at very short wavelengths is for red.)

reliably, and all the wavelengths that observers describe could easily be analyzed into four basic categories (Boynton et al. 1964; Sternheim & Boynton 1966). Results such as those of Boynton and Gordon (1965) shows clearly that a reasonable orderliness obtains in basic color naming of the spectrum.

Similarly, long before the acquisition of language or the inculcation of the rudiments of culture, human infants, as well as various infrahuman species without language or culture, regularly partition the spectral continuum into categories of hue. The principal infant hue categorization data derive from a study conducted by Bornstein et al. (1976). Figure 1(D) summarizes 4 basic ranges of wavelengths that 4-month-old babies categorize, in the sense of treating them as similar. As is clear, human adults and infants partition the spectral continuum into corresponding visual categories of hue, positioning boundaries between hue categories in similar spectral locations. The results of the infant study reveal that the visible spectrum is organized into basic psychological categories well before experience, language acquisition, or formal tuition could operate extensively on cognition.

Infrahuman species that see color likewise partition the spectral continuum into categories of hue. One study conducted by Sandell et al. (1979) analyzes hue categorization in the monkey, *Macaca fascicularis* and *M. mulatta.* As can be seen in Figure 1(C), monkeys readily generalize responses across certain wavelengths but not others, indicating systematic hue categorization. The ranges of wavelengths monkeys categorize and the points of transition between categories match those of human adults and children. Numerous psychophysical studies confirm that these monkeys and human beings have highly similar trichromatic color vision (e.g., De Valois & De Valois 1975), and natural color categorizations articulate with the standard model of color vision. Figures 1(B) and 1(A) show hue categorization data from another vertebrate and from an invertebrate species, respectively. Wright and Cumming's (1971) data on the pigeon and von Frisch's (1950) work on the European honeybee converge to confirm that visual categorization of hue is phylogenetically common in species that possess color vision.

It is important to note that the ranges of the radiant spectrum that are visible to different species differ; the bee sees ultraviolet but not red, as human beings do. Different species partition the spectrum at different locations; the interhue boundary positions for the pigeon differ from those of the monkey. The number of basic hue categories differs across species; the pigeon has three and the bee, four.

The demonstration of basic hue categories in human beings does not imply that between infancy and maturity hue categorization is fixed; categorizations may change and are plastic to experience. Indeed, the seemingly insurmountable problems of linguistic and cultural variation in color categorization that S&vB present (sect. 4.3) are hardly mystifying from a human developmental perspective. Figure 1 clearly shows, however, that for many species studied in many laboratories in many ways spanning many years, *to see color is to categorize the spectrum into hues.*

2. Some residual issues. However helpful it might be, it would be indecorous to deconstruct the contentious, self-contradicting, derivative, factually incorrect, and retrogressive aspects of the S&vB's target article. They pass over data inconvenient to their arguments, like the foregoing on hue categorization. The same corrective analysis could be brought to bear, for example, on the psychological salience of color exemplars (prototypes): suffice it to say that many empirical studies of the infant's perception of color exemplars support the idea that, even near the beginning of life, human beings see and perceive select colors in the spectrum as affectively and cognitively special; and perceptual studies with children and psychophysical studies with adults (even from different cultures) support the developmental continuity of the specialty of these perceptions.

Nonetheless, some errors of record demand correction. (1) S&vB misrepresent the infancy literature in preference: infancy studies show the primacy of naturally saturated colors (blue and red) as opposed to less saturated ones (yellow). As the authors themselves state in section 2.2, "Given a particular hue category it would seem self-evident that the best example is the most saturated because 'most saturated' means 'having most colour'." Of course, preferences, like categorization, may change in development. (2) In section 5.3, the authors recount that "it has been suggested that people . . . have different cone pigments." Rivers (1901) and Bornstein (1973a) actually referred to macula and lens, not cone, pigmentation. (3) Space is lacking to pursue numerous other misattributions (S&vB deny findings in "infant psychology" with uninformed, sweeping generalizations) and misreadings (there are identifiable principles in basic human development by which infant perceptions are transformed into mature cognitions).

Semantics versus pragmatics in colour categorization

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Abstract: We argue that the confusing pattern of evidence concerning colour categorization reported by Saunders & van Brakel is unsurprising. On a perspectival view, categorization may follow semantic or pragmatic attributes. Colour lacks clear semantic attributes; as a result categorization is necessarily pragmatic and context-sensitive. This view of colour categorization helps explain the developmental delay in colour naming.

Saunders and van Brakel (S&vB) present a compelling argument that much received wisdom concerning colour categorization requires reinterpretation. We fail to find anything surprising, however, about their suggestion that neurophysiology, psychophysics, or basic colour vocabulary do not provide adequate constraints on colour categorization. A perspectival view of categorization (Franks & Braisby 1996) suggests that S&vB's argument merely indicates that colour categorization is constrained more pragmatically than semantically.

S&vB's quotation from Newton is potentially misleading. Philosophers have argued that colour terms are observational in that their application is determined by observation alone (under normal conditions): "If one can tell at all what colour something is, one can tell just by looking at it"; and "the look of an object decides its colour, . . . the information of one or more senses is decisive of the applicability of an observational concept" (Wright 1975, p. 338). The intransitivity of non-discriminability then presents difficulties for observational concepts, exemplified by the Sorites paradox (e.g., where removal of one grain of sand does not change a heap into a non-heap, but removal of some finite number of grains does). The paradox has been taken to indicate the intransitivity of *modus ponens* (Parikh 1983), or that colour terms are not entirely observational (Bosch 1983); Wright concludes that colour terms are essentially incoherent, and fail to refer.

The incoherence claim may be too strong, though it is quite consistent with S&vB's review: their denial of the evolutionary value of colour resonates with the claim (though it would be hard to reconcile with the systematic use of colour in the plant and animal kingdoms). However, the fact that colour categorization appears to give rise to paradoxical implications, inviting the conclusion of incoherence, may suggest that foundational assumptions concerning categorization should be re-examined.

Braisby & Franks (1996) and Franks & Braisby (1996) present a view of categorization as pragmatic or perspectival. We build on Donnellan's (1966) distinction between attributive and referential uses of terms. Categorization falls between two extremes: one in which semantic attributes alone are used (attributive), and one in which only pragmatically appropriate, nonsemantic attributes are used (referential). These extremes come from a reflective mode, using central or essence-type attributes, and a "rough and ready" mode, using appearance-based attributes (cf. Smith & Sloman 1994). If the observational nature of colour terms does not allow the identification of semantic attributes, then the only kind of categorization available will be pragmatic, or referential, in character. Referential categorization reflects the use of colour terms simply to "pick out" some colour relative to a set of alternatives (even perhaps alternatives that are only "in mind"). For example, given black, green, blue, and orange chips, the use of the name "red" will probably suffice if the intention is to pick out the orange chip. The perspectival approach indicates the range of factors to which this kind of categorization is sensitive, including the speaker's intention to use words referentially, the constituents of the immediate context (e.g., the alternative colour chips present), the previous and current discourse, and the stock of shared beliefs of speaker and hearer (perhaps whether knowledge of typical "reds" can be assumed to be mutual). S&vB report that "when presented with chips people get confused and give inconsistent answers" (sect. 4.3). This is consistent with the possibility that people adopt different perspectives and that the naming task (which presupposes semantic or attributive categorisation) fails to fix the appropriate perspectival factors. Note the contrast with the categorization of other entities, say natural kinds: although referential categorization may be obtained, the speaker also has the option of intending to categorize entities attributively, in which case their categorization will be constrained by semantic attributes.

Although the appearance of incoherence is explicable on the perspectival approach, the philosophical literature nonetheless highlights the fact that colour categories overlap and lack clear semantic attributes. Indeed, this helps to explain the developmental delay in colour naming. Braisby and Dockrell (1996) report that adults judge one colour to be an instance of another: "A red is a kind of brown." Given such overlap, it would be surprising if children did not encounter difficulties in identifying the boundaries of colour terms. Thus, in naming, children are inevitably faced with choosing which colour term from a set of possibilities is the most appropriate to produce under the circumstances. Braisby & Dockrell's data suggest that young children tend to base their choices on word frequency, higher frequency colour terms being preferred. These findings are consistent with the perspectival view, assuming that the development of colour categorization must proceed in tandem with the development of pragmatic abilities, and that younger children may not have the latter. In the absence of such abilities, children's naming may "scaffold" on any available and potentially relevant linguistic properties, such as word frequency.

On the perspectival view, the confusing pattern reported by S&vB for colour is entirely predictable, even though other domains may appear more systematic. Only when empirical investigations take into account pragmatic factors, rather than solely pursuing constraints from neurophysiology, psychophysics, or basic colour vocabulary, is colour categorization likely to reveal its true systematic structure.

When science fails, can technology enforce color categories?

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Abstract: This commentary expresses basic agreement with Saunders & van Brakel, gives explanations for some of the frailties they see in color science, and suggests that the conditioning forces of modern technology may render color categories increasingly useful even if not initially "real."

I agree with the arguments Saunders & van Brakel (S&vB) advance against the notion of universal color categories. These authors have also shown how much of color science depends on this notion – for example, unique hues, opponent colors, and many physiological models. Fortunately, some edifices of color science may be left standing – for example, color matching, color-difference thresholds, and color constancy (based on matching).

I am not impressed, however, by S&vB's arguments that color science (and even the concept of color itself, as in sect. 6.2) refers to ideals that do not exist (ekphrasis). This could be said about *any* scientific theory, for theory is an imperfect description of the world. I am also not ready to ascribe all the ills of color science to a "grand illusion."

In this commentary, I explain why I think some areas of color science have the frailties discussed by S&vB and why color categories may still be useful, if not "real."

1. Opponent colors and unique hues. The apparent fragility of the opponent-color framework may be the result of insufficient coupling between models of spatiotemporal vision and models of color vision. There is a natural tendency to associate color with the physical properties of a single object or stimulus, although the surrounding stimuli in space and time exert powerful influences on the perception. Even semantic context can have an effect (although this effect may be on higher perception and not on basic sensation). For example, a uniform display screen with D65 chromaticity looks white to me, but when some of that same screen is replaced by a map of dark green continents, I see the ocean between them as bluish.

Complicated though color vision is, any model must be a simple picture. Opponent colors are pictured in three-dimensional space with special perceptual coordinates that are tied directly to the tristimulus values of a single stimulus (or at most of two stimuli, the test and an "equivalent surround"). As S&vB show, this picture is too simple. Ultimately, what is needed is a space of N dimensions instead of three (N being the number of receptors in the retina), wherein a dynamical law carries physical stimuli to percepts. Even this kind of theory stops short of semantic dependencies.

2. A parable. S&vB are not alone in their uneasiness with color-vision models. In introducing such models, Wyszecki and Stiles (1982) offer a warning that includes the following:

> Among the various kinds of model that philosophers of science consider . . . are so-called "floating models" (a term coined by H. R. Post) which we seem to encounter quite frequently in the field of color vision. Essentially, a floating model is disconnected from the basic theory and the empirical facts; that is, it has no theoretical or empirical support and, quite obviously, its usefulness is in question. According to Redhead (1980), H. R. Post has parodied the case of a floating model with an example referred to as the *Farm-Gate Contraction.* (Wyszecki & Stiles 1982, pp. 584–85)

In the Farm-Gate parable, a farmer investigates the relation between the diagonal length (l) and the length and width (x and y) of a farm gate. Not knowing the law of Pythagoras, he invents a linear law ($l = x + y$) that is empirically true for $x = 0$ or $y = 0$, but not otherwise. To correct the law, the farmer writes $l = a(x + y)$, where the correction factor a is called the Farm-Gate contraction. He maintains the reality of the linear law, which becomes the sort of ekphrasis referred to by S&vB.

Of all the models Wyszecki and Stiles describe in their book, none bears explicitly the stigma of the Farm-Gate, yet all seem implicit candidates for the dubious honor. I also felt uncomfortable with the Farm-Gate parody: Would I feel superior to the farmer if I had not learned the Pythagorean law? Because it floats freely above the field of models, the Farm-Gate contraction is both amusing and disconcerting. It conveys the uneasiness with which even Wyszecki and Stiles viewed the models with which they were so conversant.

3. Engineering expedients. A true theory of vision in N dimensions is a tall order. Nonetheless, tractable engineering models of color exist and have been useful in the design of lighting, object colors, displays, and so forth. Such models underpin color-order systems (such as the Munsell). A color-order system can be defined as a human-engineered, physically exemplified database of color specifications. To be useful, any such system of color samples must be arranged so the samples can be searched and interpreted easily. It helps if colors are uniformly spaced and if they are labeled according to cardinal axes that are in some sense familiar. The positing of cardinal axes is surely reinforced by training in the use of such atlases. The use of a color atlas may reinforce perceptual universals, or it may reinforce other conventions that eventually *appear* to be universals. Artists, for example, demand an additional dimension (besides perceptual) from a color atlas: the means of production of a color. Standardization of inks in color presses and printers has made it useful to organize an atlas according to increments in the color densities of the particular specified inks; hence the atlas can function in the context of "active vision," rather than just passive color recognition.

All this suggests an interaction between the technological tools and modes of expression. Can one be exposed to the controls of a color television and not have a notion of color in the Western sense, and indeed in the NTSC (National Television Standards Committee) sense? Consider the analogy of the QWERTY keyboard: Although designed to *reduce* typing speed, QWERTY is second nature to us now. It is nearly an invariant of the Roman-alphabet world. This could be an example of a learned "universal" order, and color could be another.

4. Conclusion. Although a unified theory of vision (including color) does not exist, the engineering models may provide hope, if only because they affect the reality they purport to describe. Color engineering exerts normative influences on color categories, especially within communities that use color-order systems explicitly. These influences will surely expand as a variety of cultures are "Westernized." Therefore, even if color categories started out as an ekphrasis, the "hypothesis" becomes more substantial and real as the normative influence spreads. Perhaps metamerism is an appropriate metaphor for the coalescence.

Could we take lime, purple, orange, and teal as unique hues?

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Abstract: Saunders and van Brakel question whether the special status of red, green, yellow, and blue in our perceptual organization is anything more than a shadow cast by the English language. I suggest that it is more than this. We can hardly imagine treating lime, purple, orange, and teal as unique hues, and the reason does not lie in special training. To settle the issue, I suggest some lines for psychological experiment and anthropological investigation.

First, where we agree: Saunders & van Brakel (S&vB) are right that there was a neat standard model, it was attractive and widely held, and it simply cannot be correct. There was a phenomenological story of two pairs of unique hues, standing in opponent relation to each other; there was a physiological story of two pairs of colour-opponent cell types in the lateral geniculate nucleus (LGN); and the model claimed that the first story was explained by the second. (For now I shall say nothing about the further claim, that this also explained some universals of human colour naming.) The evidence against a crude linkage of the two domains was available long ago: The LGN cell responses simply did not correspond adequately to the patterns of colour naming and appearance. (Approaching the short-wave end of the visible spectrum, for example, there is an increase in the component of apparent redness, but no increase in firing of +R −G cells.) But as S&vB say, it is only recently that the failure of the correspondence has been widely acknowledged.

So far, this is a correction concerning the physiology and its implications. But S&vB have bigger fish to fry. They wish to question the phenomenal story itself, and this is where we part company. I shall describe a little more fully the main ingredients of the phenomenal story, and then consider how far it withstands their attack.

The basic story is this. There are shades of red, yellow, green, and blue that contain no hint of any other colour. These unique hues, moreover, can be used (with only a little stretching) to characterize all colours. For example, pinks can be seen as pale reds, browns (at a pinch) as dark reds or orange. The most significant support for this comes from Jameson's and Hurvich's cancellation experiments (e.g., Jameson & Hurvich 1955), supplemented by other work on the forced naming of colour samples with a restricted vocabulary. There is no obvious connection between the roles just mentioned and the claim that unique hues come in complementary pairs. (There is no *a priori* reason why what cancels the redness in a mixture should itself be unique green.) But the claim is not far from the truth, though the complement of unique red may be bluish rather than unique green.

There is nothing in this view of the unique hues to disparage or exclude the existence of colour classifications that are not easily mapped in terms of the unique colours, nor classifications based more on brightness or saturation than on hue, nor even classifications (like *blond*) that are restricted to particular domains.

For S&vB, this view of the unique hues should be considered no less "rhetorical" than Newton's claims to segment the spectrum into seven bands (sect. 4.1, last para.) They mention the "unnatural" viewing conditions involved in the cancellation experiments (sect. 3.3, para. 5). But their "crucial question" is this: Even if there are interesting things we can do with red, yellow, green, and blue, "why start with the four unique hues in the first place?" (sect. 3.3, para. 5) The worry, I think, is that the features of the chosen colours may be no more than a reflection of accidental features of 20th-century American English (cf. sect. 4.2, para. 1).

The accusation needs to be taken seriously; taken seriously, it emerges, I think, as implausible. Sticking entirely to the level of phenomenology and perceptual organization, we should ask: Is the choice of red, green, yellow, and blue as unique and basic terms of classification an arbitrary one? Could we have done just as well, for example, with lime, purple, orange, and teal?

Suppose we imagine a colour classification system based on those four. What would be involved? Some shade of orange would need to strike us as unique and pure – containing no hint of red or yellow, or any other colour. Similarly, teal would have to seem free of any hint of blue or green. What we now see as unique blue we would have to see as containing purple and teal. We would need to be able to understand instructions like this: "Take this yellow patch of light, cancel the orangeness in it with as much teal as it takes, until you are finally left with the pure lime that is its other component."

It is, I think, no mere accident of the 20th-century American English that we find it hard to imagine using these four hues in the way described. We might write the specification of a language for talking of colour in this way, but is it a language we could learn to speak? It might well be beyond our capacities. This is a speculation that is hardly to be proved or disproved in a few paragraphs. But there are points to be made in favour of it, and a request for empirical information that would shed light on the matter.

Wittgenstein, Quine, and Chomsky, in very different ways, have all pointed out how learning and mastery rest on natural endowment: given a limited amount of training with a term, a successful learner will acquire a capacity to apply the term in an indefinitely large range of new cases. But that the learner goes on (beyond the cases that appeared in the training) in this way rather than that, however, is ultimately a matter of natural endowment.

Consider some of the uses to which we put red, yellow, green, and blue. We easily see a red component in central shades of orange; even with effort, however, I cannot see an orange component in central shades of red. It seems quite possible that we were given no teaching directly on this; and training in the ordinary application of the terms would give no instruction on what we ought to say here. And yet there is wide agreement. Given a fairly narrow training in the use of "red," we happily go on to talk of the "redness" in an orange thing, though it is not in any ordinary sense *red;* by contrast, trained in the use of "orange" we do not go on to talk of the "orangeness" present in red things. This seems a matter in which our application of the terms depends on "how we find it natural to go on," not on any specific training imparted in the course of learning English.

The issue is not an a priori one. There are two empirical investigations that would help decide the issue. First, anthropologists can search for speakers of other languages who use radically different sets of unique hues. Second, psychologists nearer home can see how easily different vocabularies can be mastered by people like us. Can they get subjects to perform cancellation experiments successfully with lime, purple, orange, and teal? Can subjects use such terms (with only a little stretching) to characterize all colours?

We might look to the World Color Survey, that huge investigation from which preliminary reports are now appearing, for information on the first issue. Unfortunately, I suspect that the help it gives will be insufficient. It is important to know about the extension and focus of colour terms, which are the core of the WCS study. There are other factors, however, that are just as important: the qualifiers and modifiers (like our "light," "dark," "reddish") and comparatives (like "redder than," and "darker than"). And finally, we need to know about the use of phrases like "there is some x in y" – as when we talk of the red in a certain pink. We need something like a whole grammar of colour. A mapping of extensions alone, for English, tells us the extent of "red" and of "orange," but says nothing about whether there is red in orange or orange in red. Yet it is that kind of judgment that is ultimately the most interesting, I think, in clarifying how we use something like a coordinate system with axes marked by unique hues for mapping out colour space. The next time the anthropologists collect data on the kinds of colour language used, they might add this to the wish list for the folks back home.

The fears of Saunders & van Brakel will be justified if people prove to do as well with lime, purple, orange, and teal as we do with red, yellow, green, and blue. But we should not underestimate the fact that such an outcome at the moment seems almost unimaginable.

Unique hues

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Abstract: Saunders & van Brakel argue, inter alia, that there is "little evidence" for the claim that there are four unique hues (red, green, blue, and yellow), and that there are two corresponding opponent processes. We argue that this is quite mistaken.

The Commission Internationale de l'Eclairage (CIE) defines a unique hue to be a "perceived hue that cannot be further described by the use of hue names other than its own" (CIE 1987, quoted in the target article, sect. 1, para. 4). Thus as Saunders & van Brakel (S&vB) note, a unique hue is defined by the CIE in linguistic terms. However, color scientists themselves generally do not think of unique hues in this way. Boynton, for example, says that "[t]he explanation of such a conception requires an appeal that is wholly to subjective experience," and explains a unique (or, as he says, "pure") yellow to be one that "cannot coexist with blue" and "is neither reddish nor greenish" (Boynton 1979, p. 31). Hurvich says that a "unique point" in the hue circle is the "place where a yellow occurs in which we cannot detect any red or even any green. It is a transition point where we experience a hue that is uniquely yellow (Hurvich 1981, p. 3). None of these explanations concerns language at all. Hurvich does remark on the striking fact that all colors can be described as combinations of the four unique hues together with black and white, but it is perfectly clear that this is not intended to be part of any definition.[1]

The claim that red, yellow, blue, and green are the four unique hues is best understood as a collection of generalizations about human color experience: For example, all human beings with normal color vision can see a yellow that appears to them to be neither reddish nor greenish, and (at least under normal conditions) no yellow appears to be bluish. We will abbreviate the conjunction of these generalizations as "4UH" (four unique hues).

What is the connection between 4UH and linguistic data of the sort reported by Berlin and Kay (1969)? It is reasonable to think that color appearances and color terminology are not entirely independent. We might expect, for example, that monolexemic terms for unique hues appear in languages more frequently than monolexemic terms for binary hues, and that no language has a monolexemic term meaning *yellowish-blue.* But, in the absence of any theory of how, exactly, the unique/binary distinction among color appearances would influence color semantics, the links between the linguistic data and 4UH must be regarded as no more than suggestive. Similar remarks go for Rosch's work on color categorization (see, for example, Rosch 1973). *Thus the link between 4UH and such research is highly indirect.* S&vB are therefore quite wrong to suggest that if 4UH were true, this would be clearly reflected in color terminology (see especially sect. 4.3).

The connection between 4UH and subjects' judgments of color appearance is, in contrast, very close. Of course, it is not claimed that the distinction between the unique and binary hues will invariably suggest itself to all adults with normal color vision who reflect on their color experience. Rather, the claim is that the distinction will be found fairly natural *after it is pointed out.* So S&vB's addition to 4UH of "phenomenal self-evidence" (sect. 3.2, para. 4) is entirely gratuitous, and the surrounding argument is directed at a straw man.

Sternheim and Boynton (1966) found, inter alia, that English speakers were unable to describe the color of a yellow light without using "yellow," but were able to describe an orange light as "red-yellow." According to S&vB, citing this study "misses the point," because "the issue is whether asking people to point out the unique hues reveals anything more than their command of English" (sect. 4.2, para. 1). This is confused (not least because the Sternheim and Boynton subjects were not asked to "point out the unique hues"). According to 4UH, orange visually appears to all normal humans to be reddish and yellowish. It can hardly "miss the point" to ask subjects to describe orange lights without using the term "orange." Naturally, there may be rival explanations of the data. One might argue, for example, (1) that "red-yellow" is synonymous with "orange," and (2) that this fact has nothing to do with the ways colors appear visually to all normal humans. But S&vB do not supply any such argument.[2]

Opponent-process theory (OPT), at least in conjunction with "linking propositions" connecting the activity of the opponent channels to color appearances, predicts 4UH. Therefore, any evidence for OPT is evidence for 4UH. (And conversely: 4UH is a weak kind of evidence for OPT.) So why do S&vB think there is no good evidence for OPT?

Setting aside S&vB's mistaken view that OPT puts severe constraints on human color vocabulary, two of their main reasons for doubt are the following:

First, OPT claims that "there can . . . be no red-green experience," but "the results of Crane and Piantanida (1983) suggest that whether or not this is so is an empirical matter" (sect. 3.2, para. 2). Because one of Crane and Piantanida's results was that some subjects reported seeing a reddish-green, we take it that S&vB intended to say that "the results of Crane and Piantanida suggest that OPT is false." But Teller (1984), whom S&vB quote apparently to support their case, in fact makes it clear that these data do not raise any particular difficulty. OPT is not committed to the assumption that *all* color information in the visual system is opponent-coded.

Second, S&vB seem to assume that, if OPT were true, there would be single cells whose response properties mimic those of the psychophysical opponent channels (sect. 3.4). They then take the fact that no such cells have been found to be a serious objection to OPT. But the assumption is unmotivated: the physiological opponent coding of color, like the coding of most perceptual properties, may be accomplished not by individual cells but by populations of them.

OPT is a simple theory with no close competitors that explains a wide range of psychophysical phenomena, including hue cancellation, color naming, wavelength discrimination, temporal resolution for flickering stimuli, mixture thresholds for color stimuli, color blindness, and successive contrast.[3] That is why it is reasonable to believe that it is at least approximately correct. But we do not know how S&vB would object, for most of the phenomena just listed are missing from their discussion.

NOTES

1. The target article is not clear about the status of the CIE definition. S&vB appear to adopt it officially (sect. 1, para. 3), but this is plainly at odds with their discussion of nonlinguistic evidence that would be irrelevant if the definition were accurate. (See also note 2 below.)

2. In this part of the target article S&vB seem to have completely forgotten their earlier apparent endorsement of the CIE definition. Adopting it would imply that the existence of the unique hues would be *completely settled* by the verbal reactions of competent speakers of English, with the result that the Sternheim and Boynton study would not "miss the point" at all.

3. For a recent experiment on the effects of spatial pattern on color appearance, which supports OPT, see Wandell (1993). The experimental procedure here did not require subjects to name colors.

"Colour science" and the autonomy of colour

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Abstract: At the close of their searching critique, Saunders & van Brakel raise, but do not address, the question: "Whither colour research?" There are two distinct traditions of colour research, one based on disembodied coloured lights and another on surface colour. The coherence and integrity of both these traditions are challenged by the nonautonomy of colour.

Saunders & van Brakel's (S&vB's) discussion of the most central theoretical tenets of modern colour science complement Thompson's (1995) equally searching examination of its fundamental ontological and epistemological assumptions. [See also Thompson et al: "Ways of Coloring." *BBS* 15(1) 1992.] Such radical reappraisal is rare indeed. Yet S&vB seem curiously unwilling to cause really serious trouble. Thompson rests largely content with the existing theoretical and research approaches. S&vB raise, but decline to answer, the obvious question remaining after their potentially devastating critique: "Whither colour research?"

Colour research has been polarized since at least the time of Goethe (Kantor 1950). If this polarization has been somewhat concealed in recent years, it is because of the ascendancy of psychophysical and physiological studies based on isolated, disembodied spectral colours. Yet a countertradition persists, pioneered by the classic research of David Katz:

> Where do we encounter colours? First of all they are certainly to be observed in objects. A paper is white, a leaf is green, coal is black. . . . Then further: The sky is grey, the water has a green shimmer, and the air is full of beams of light. Such judgments, too, have to do with colour, and they seem to be perfectly commonplace. The attitude which dominates such judgments of colour, we shall term the "natural," and, because of its significance for everyday life, the "biological" attitude. . . . Experiences of colour in their natural unbroken meaningfulness arise out of the need for a practical orientation towards the colour-qualities of the surrounding world. . . . It would be a kind of psychological perversion . . . to cast these cases aside, and, instead, begin [our] study with colour-phenomena which the colour specialist has been able to produce only under the highly artificial conditions of the laboratory. Most people depart from this world without ever having had a chance to look into an expensive spectroscope, and without ever having observed an after-image as anything other than something momentarily wrong with the eye." (Katz 1935, pp. 3–4.)

The topics of research in the countertradition include colour constancy and the perception of transparency, gloss, and shadow. Now, both S&vB (sect. 5.1) and Thompson (1995) would appear to place themselves securely within this tradition in their insistence that colour is not a disembodied quality or sensation but an intrinsic property of things: "This red would literally not be the same red if it were not the woolly red of a carpet" (Merleau-Ponty 1962, pp. 4–5; cited in Thompson 1995, p. 285).

Yet, as I see it, the important new work of S&vB and also of Thompson must call into question not only the mainstream but also the countertradition of colour research. First of all, many studies of "surface colour" themselves involve highly abstracted displays, where colourfulness – or gloss, fluorescence, and so on – are as extraneous to the target object as are the colours of standardized colour chips (S&vB, sect. 5.1). More fundamentally, our "experiences of colour in their natural unbroken meaningfulness" (Katz 1935, p. 4) are simply not about colour *per se*. Colour, in and of itself, makes no biological sense, any more than movement, shape, or size do. Colour could not, then, afford the coherent, ultimate, isolable subject of a "colour science."

Empirical evidence for constraints on colour categorisation

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Abstract: The question of whether colour categorisation is determined by nontrivial constraints (i.e., universal neurophysiological properties of visual neurons) is an empirical issue concerning the organisation of the internal colour space. Rosch has provided psychological evidence that categories are organised around focal colours and that the organisation is universal; this commentary reconsiders that evidence.

Saunders & van Brakel (S&vB) raise some important concerns about colour categorisation from a neo-Whorfian position. Many of these are worth considering, but we should decide whether they are justified primarily from psychological rather than neurophysiological data. We therefore begin by asking what is meant by colour. Perhaps the ease with which others understand us when we use colour names pardons the omission of definition from S&vB's target article; yet a definition of colour is nonobvious and repays consideration.

Colour names are adjectives and therefore always qualify a noun. We talk of a red table or a magenta jumper, not of a red or a magenta. So colour names must refer in some way to the appearance of something else, either to the surface of an object or, in the case of light sources, an extension in space. When we describe a surface appearance using colour names, these appearances usually bear some reliable connection to the wavelength information coming from the surface of the object of course, we cannot put into words what it feels like to have the experiences. Equally obvious is that some appearances of surfaces may not be related to wavelength information. Indeed, in most cases without an obvious connection to wavelength, we would not call the surfaces coloured. However, even when they are connected, the relationship between the appearance and wavelength may be complex. A given wavelength does not always produce the same (coloured) appearance.

If we were constrained to the use of only one word, many different appearances might be classified as red. If we are allowed the use of a further qualifier, the words transparent, fluorescent, shiny, dull, light, deep, and so on may be needed to give a better description of the different appearances. So what does it mean to say that these different types of stimuli can all be classified as red? To answer this question one needs to address the question of colour categorisation. It may be that categorisation is determined by the action of wavelength-coded cells, but this is not proven even for red or green and seems unlikely for purple or brown.

Although the appearance of a surface is directly perceived, the placing of that appearance in a category depends on memory, in particular, the organisation of the memory for surface appearances that Davidoff (1991) called the internal colour space. We need to question the structure of this type of memory. Naturally, one question concerns the similarity of different surface appearances to each other. Another question concerns the constraints on surface appearance that prevent stimuli from being classified together. As part of the latter question, it is worthwhile to ask whether there are constraints imposed on categorisation from the neurophysiology that produces surface appearances. Clearly there are constraints on appearance but S&vB doubt their impact on categorisation.

The organisation of the internal colour space is the critical issue. Rosch (Rosch Heider 1972; Heider & Oliver 1972) has provided evidence that categories are organised around focal colours and the organisation is universal. Rosch argued that focal colours will be more salient; the consequences should be (1) that focal colours should be easier to remember and (2) it will be easier to form associations to these colours. If these psychological consequences apply only to the four primary foci (red, yellow, green, and blue), then we might suspect that some neurophysiological derivative of the unique hues was driving the organisation of the internal colour space. It would not be proof of a connection, but it would be a strange coincidence if there were no connection.

So it is an analysis of Rosch's and other similar data that is critical. Let us first examine Rosch's claim that focal colours are easier to remember whatever one's colour name vocabulary. A close inspection of these important empirical data does give some concern. The Dani achieved only 2.05/8 correct for their memory of focal colours, although this very low success rate compared well to their even lower success rate for nonfocal colours. Indeed, the evidence that Dani colour memory closely resembled that of U.S. subjects was only gained from a visual impression, not a statistical analysis of the data. This is not good evidence for universality. The Dani clearly found the tasks difficult. It could also be that the salience of the high saturation colours in the ordered array biased the observer's choice toward focal colours, although this has been disputed (Garro 1986).

The second key finding is that the Dani found it easier to learn associates paired to focal colours. These data may have faults, but that the Dani were not asked to use nonsense words is not one of them. The use of such artificial stimuli would be difficult for the Dani and, in any case, likely to contain phonemic combinations unacceptable in their language. In fact, Rosch provides here some of the best evidence for universality. The only crumbs of comfort for S&vB are that the data do not give any support for the Berlin & Kay (1969) evolutionary order of acquisition and that the Dani were not prepared to select the best example from arrays.

A reanalysis of Rosch's data suggests some doubt about the pivotal role for focal colours. However, the argument is one that could only be settled by further experimentation that ruled out existing artifacts. There is a further method that could be used. It has been shown that the discrimination of colour appearances is affected by category membership; within-category discriminations are harder than across-category discriminations. The phenomenon is general and applies to other visual categories and to other modalities (Harnad 1987). The neurophysiological origin of the phenomenon would seem unlikely to derive from properties of opponent cells. It would be interesting to know the extent to which the phenomenon applies to the organisation of the internal colour space. Our own unpublished studies have certainly shown that the phenomenon applies to colour categories other than those based around the four unique hues.

Colour-cognition is more universal than colour-language

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Abstract: We acknowledge that empirical support for universal colour categories in colour cognition is insufficient: it relies too heavily on Rosch-Heider's work with the Dani. We offer new evidence supporting universal perceptual-cognitive colour categories. The same data also support language modulating colour-cognition: Universal structures are fine-tuned by language.

The epidemiology of belief in colour universals reveals that this condition is prevalent among psychologists but less so among anthropologists and sociologists. Psychologists are more comfortable with the notion of biology shaping and constraining behaviour and cognition. The acceptance of colour universals in psychology has a narrow empirical base. It rests largely on Rosch-Heider's work with the Dani of Indonesia (Heider 1972; Heider & Olivier 1972). Accounts of her results are now the standard fare of textbooks: "The Dani . . . whose language has only two basic color terms, perceive colour variations in exactly the same way as people whose language has all 11." (Atkinson et al. 1990, p. 327). Important as her work is, the inferential load it is required to support is too heavy. In addition, as Saunders & van Brakel (S&vB) point out (sect. 2.2), her results are consistent with relativism: the Dani's low scores on the recognition task could reflect the relative unavailability of apt descriptors. We (Corbett, Davidoff, and Davies) are carrying out a replication and extension of her work in Africa, the Caucasus, and Papua New Guinea to test the generality of her results.

In the meantime, we have some recent results that may be placed along side Rosch-Heider's in support of the universalist position, although these data also suggest that there is some scope for language (culture) to influence colour-cognition. We give just one example here (see also Davies et al. 1997). Davies and Corbett (1997) report a study comparing speakers of English, Russian, and Setswana (a Bantu language spoken in Botswana) using a colour-grouping task. Setswana has four colour terms that are used widely and frequently: *bontsho* ("black"), *bosweu* ("white"), *bohibidu* ("red"), and *botala* ("green or blue") (Davies et al. 1992). There are a number of other less common terms for yellow and brown, but there are no terms equivalent to "pink" or "orange," and the term for "purple" is known by only a minority of the speakers.

We asked monolingual rural informants to sort a set of 65 colours (a representative sample of "colour-space") into groups based on their similarity. We were careful about the wording of the instructions; we consulted native Setswana speakers about the appropriate wording and about the acceptability and appropriateness of the task. It was a task that all our informants enjoyed and appeared to find intriguing. What was perhaps the most striking aspect of these data were the obvious similarities across languages in the groups formed. Most notably, many Motswana (the Botswana people) produced groups that would be called *blue, green, purple,* and *pink* in English, just as speakers of English and Russian did. We used a variety of more formal statistical techniques to assess the similarities and differences in the pattern of colour grouping amongst the three languages. Multi-dimensional scaling, factor analysis, and cluster analysis all supported the informal impression of strong cross-language similarities. Nevertheless, there were also significant differences between the Botswana and the English and Russians that were consistent with language influencing performance. The clearest instance was that the Botswana were more likely than either English or Russian speakers to group blue and green colours together. These data are consistent with common underlying perceptual-cognitive structures being modulated by language or culture.[1]

Our stimuli were uniform patches of colour drawn from a standard system varying on hue, lightness, and saturation. The stimuli did not vary on the set of possible attributes such as shape, size, glitter, and so on given by S&vB (sect. 5.1). Ratner 1989 also worries about the use of such "sanitised stimulus materials," p. 364. In our case, they might be less concerned about the nature of the stimuli, because there was evidence for relativism despite the nature of the stimuli. We agree with S&vB that the use of such stimuli to establish how a language describes surface appearance puts one at risk of missing important terms. Setswana has a number of sets of descriptors for restricted domains. The most prevalent are descriptors of "cattle-appearance." Most such terms are "mixed-colour" terms such as "black and white." But there are some single colour terms such as "red of cattle." No Botswana would normally use "red of cattle" to describe an equivalent red tile.[2] However, using such sanitised materials in conjunction with natural objects, available artifacts, and consulting native speakers seems to us to be both practical and sensible.

In summary, we accept that the empirical base for claims for colour universals is weak. Further work is needed. We do have evidence consistent with relativism that emerged despite using sanitised materials.

NOTES

[1] With the help of a friend's bilingual daughter, who was about 7 at the time, I have used versions of the colour grouping task on mono-lingual, Setswana-speaking children, aged from 3 to 10 years. The children also form separate blue and green groups, and they tell you that both groups are *botala,* "green or blue." They also form purple and pink groups, and will tell you that they do not have a name for these groups.

[2] If asked whether tile-colour is "red of cattle," they either treat it as a joke or are embarrassed for you.

Colour categorization and the space between perception and language

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Abstract: We need to reconsider and reconceive the path that will take us from innate perceptual saliencies to basic (and perhaps other) colour language. There is a space between the perceptual and the linguistic levels that needs to be filled by an account of the rules that people use to generate relatively stable reference classes in a social context.

Here are two claims that are important to the universalist tradition in colour naming: (1) Some colours are psychologically salient, and that salience is not a function of language or culture. (2) Some colour categories are psychologically salient, and that salience is not a function of language or culture. I take it that Saunders & van Brakel (S&vB) accept these two claims as being empirically supported. As they note with respect to (1), certain of Eleanor Rosch's (1972a) experiments with the Dani of New Guinea "*did* show that the Dani remembered focal colours better than nonfocal ones, as did Americans" (sect. 2.2, para. 5). With respect to (2), S&vB devote section 2.3 to the relevant work. Though they suggest that "caution" (sect. 2.3, para. 1) is required when it comes to Bornstein et al.'s (1976) claim that 4-month-old infants naturally partition the spectrum into 4 colour categories – in much the same way that Western adults do in hue-naming tasks – they present no evidence that there are any problems with Bornstein's hue-categorization claims.

These results specify innate sensitivities to prototypical colours and to hue categories. The Dani had no language for many of the focal colours. Bornstein's infants had neither language nor culture. So there is something right about Berlin & Kay's (1969) original claims. There are psychologically salient basic colour categories with an internal structure dominated by focal (prototypical) colour samples. Berlin & Kay may have been wrong about many things (e.g., the number and nature of basic categories) and their original work did not establish these claims. But the claims are pretty much established and the proper foundation for one kind of investigation of human colour language.

Where to go from this minimal toehold? S&vB strive (rightly) to show that there is no determined path from the perceptual saliencies mentioned above to the colour vocabularies found in real languages. They are (rightly) critical of a scientific practice that would like to see – or imagines there to be – tight correlations percolating up from the neurophysiological to the psychophysical to the perceptual and, ultimately, the linguistic. This is currently a no-go. The "Hering code" of psychophysics cannot, at the present time, be represented at the physiological level (Abramov & Gordon 1994). Although this may be merely an epistemic gap, the same is not likely to be the case when we look at the relations between perceptually salient colours/categories and colour language. It is a standard assumption of the universalist tradition that languages with restricted basic colour vocabularies have composite terms (sect. 2.1, para. 2). Within their reference classes, these terms include at least two of the original basic colour categories and may include two or even three of the unique hues. There is no evidence that these categories – historically primary by the universalist's own account – have any correlates at the perceptual/psychophysical/physiological levels. Vision science has nothing to say of them and would, I suspect, have to be very different from what it is to do so. Composite basic colour categorizations are "salient" only in the linguistic domain. (These points can all be made from within the universalist framework. One does not need to reject it all – and much of vision science! – to recognize these problems.)

We need to reconsider and reconceive the path that will take us from innate perceptual saliencies to basic (and perhaps other) colour language. The universalist tradition has proposed no cognitive account of how people come to have and use basic colour terms. It has limited itself to a correlation game, an attempt to show that colour categories and colour prototypes that have a nonlinguistic salience are represented in the languages of the world. When the universalist does not get the fit – between the perceptual and the linguistic – there is very little to say; there is nothing but (supposed) correlations to fall back on. The S&vB make much of putative failures of fit, and their evaluation of the universalist tradition is almost wholly negative. What more positive proposal can we make that is not simply a recapitulation of the standing universalist view? I think we should recognize that there is a space between the perceptual and the linguistic that needs to be filled by an account of the rules that people use to generate relatively stable reference classes in a social context. These rules must be stated with some precision and yet be flexible enough to account for the kinds of variation in colour languages that we find.

This is a tricky (possibly circular) project, but it is not improbable (Harrison 1973). It takes the idea of nonlinguistic saliencies seriously and asks how such saliencies may be exploited by colour language users for essentially social purposes.

As S&vB point out (sect. 2.3, para. 2), children do have some difficulty in mastering colour language. This suggests that the rules they must learn are relatively complex and difficult to internalize even in Western basic-colour-using contexts, where there is explicit, rote learning of abstract colour categories. I think we ought to concentrate on these languages first. How do their speakers conceptualize colour? This may give us some clue as to how the speakers of rather different languages operate. We may find that categorization based on hue is common to Western languages, but it is brightness that is crucial to others. We may find languages that do not fully integrate hue and brightness, and we may find languages that do not fit a hue-saturation-brightness (HSB) model of colour ordering. It may be possible therefore to classify languages in terms that are different from Berlin and Kay's evolutionary order. Such classification can only be "nontrivial," however, if there is more to colour categorization than social idiosyncrasy. But even Saunders and van Brakel say (sect. 1, para. 6) that they are not arguing for that view.

Cultural beliefs as nontrivial constraints on categorization: Evidence from colors and odors

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Abstract: The following provides further arguments for the nonuniversality of color as an autonomous dimension. Research on odors suggests that there are cultural constraints on the abstraction of dimensions for objects. Color vision analysis leads to an overemphasis on the role of perceptual processes in categorization. The study of odors points to human activities as a more important principle of categorization that drives the perceptual processing and suggests a reconsideration of vision itself.

Our current research on categorization and lexicalization in vision confirms the scepticism of Saunders & van Brakel (S&vB) (Dubois 1991). Our data on olfactory categories have led us to reconsider the theoretical framework in which the experimental paradigms on colors have generally been conceived.

1. Color and colors. Research on colors relies on the implicit presupposition that color is ontologically grounded, without evaluating whether any culture or language has indeed abstracted color (singular) as a dimension (Dubois et al. 1997b). Anthropological and linguistic research shows that some cultures talk about "color" as something not independent of the object supporting it. In Ngbaka-ma'bo (Central Africa), there is no specific term for color itself, independent of other parameters of the colored objects (Thomas 1989). In Fon (Benin), there is no word for the color dimension itself; color experience is never experienced and named *per se*, but always in association with the identity of the speaker, circumstances, or types of places (Guedou & Coninckx 1986). In this culture, color can be expressed by two words: the first one "*si-mé*" (literally "*water-in*") refers to the color of the liquid from which the object gains its unified colored aspect; the second, "*hweka*" ("*stripe*" or "*pattern*") is used for nonuniform colors (which are more intrinsic properties of the object).

The scene changes when we shift from colors to odors, within our own culture as well as in cross-cultural comparisons (Dubois & al. 1997). Whereas color has been set in our culture as an objective dimension of the world, as something standing "out there," odor remains a sensation that one "experiences" subjectively. Furthermore, in our languages (at least English and French), there are no names for odors. Naming of odors is commonly done by naming the source, for example, "*lavender*" meaning "*smell of lavender.*" In some African cultures and languages, however, odors are cognitively constructed as "objective realities" and have names (even nouns): for example, about 11 "basic terms" for odors have been identified in Li-Wanzi (Gabon) (Hombert & al. 1994).

2. The analytic mind. The analysis of constraints on categorization deals mainly with perceptual constraints, where perception is conceived as a "contemplative," "bottom-up" process of "extracting" relevant properties of the real world, among them, colors. The previous examples of colors (and odors) from African languages suggest that it is the diversity of human activities and interactions (e.g., hunting, cooking, domestic life, and corporal odors) that grounds categorization and naming, rather than common and therefore "trivial" perceptual constraints.

In our culture (at least the French one), the highly skilled expertise in perfumes provides further support for the role of human activities in the orientation of perceptual categorization: perfume creators are able to discriminate and identify the components of a perfume or to predict the global effect of a special combination of odorants. Experiments on the categorization of odors (Dubois & Rouby 1996) have led us to conclude that whereas "natural" odors – which are highly complex chemicals – are processed globally, as something given and experienced, perfumes may be processed as artefacts, in a more analytical manner. This difference between ordinary odors and perfumes is also morphologically marked in French where we say "parfum *de* fleurs" (the perfume of flowers) as well as "parfum *aux* fleurs" (perfume made out of flowers), and "odeur *de* fleurs" (smell of flowers) but where "odeur *aux* fleurs" (smell made out of flowers) is not accepted by native speakers (David & al. 1997). From these data we argue that the analytic outlook of categorical perception relies mainly on the development of a specific technology and of a specific social activity, rather than on the generic development of cognition as seen by "analytic philosophers, scientists, and bureaucrats" (sect. 4.3).

The perception of an odor or a perfume is related to the orientation and the purpose of the task: detection or identification can rely on a holistic processing of the odor as a cue to the source (e.g., animal, state of a food, presence of someone), whereas the choice or the discrimination between perfumes requires analytically oriented processing. Thus, perception is not a unique bottom-up process, but a complex, knowledge driven skill honed by cultural practices, such as expertise.

3. Conclusion. The work on color categorization has been misleading in relying on a physical theory of light. The reduction of color to physical parameters ignores its semantics, grounded in practice and expertise such as dyeing or painting (hunting or perfume creation in the case of odors). The theory of color(s) that derives its authority from the physical sciences may not be the only one relevant. What we get from the experiments reviewed here is either, in our culture, the "correct" answers from subjects trained according to a commonly shared knowledge of color, or, in other cultural groups, the distance from this "common" and "official" knowledge. In any case, it is a social answer to a socially oriented question, a negotiation of word meaning rather than a "raw," naive, primitive perception of "natural" categories of platonic forms on which words map as labels (Dubois & Resche-Rigon 1995). This is perhaps why, as noted by Saunders & van Brakel, we have arrived at the fact that "innate categories always coincide with 20th-century American English" (sect. 4.3).

Mad about hue

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Abstract: Despite the heat of their attack, Saunders & van Brakel do illuminate various shortcomings of color research in the tradition of Berlin & Kay. Berlin and Kay elicit a pan-cultural pattern in color language, but the pattern does not provide much insight into the human mind.

One cannot ignore the fire of Saunders & van Brakel (S&vB). In section 2.1 they accuse Berlin & Kay of pushing their "a prior belief" on the scientific community "as an empirical discovery." Berlin & Kay's "most solid result," namely "the clustering of foci" is explained away as an artifact of "the progressive domination of the West." S&vB charge that Berlin & Kay's "goal was to refute relativism," so by implication S&vB's goal is to defend it. S&vB are thus allied, perhaps inadvertently, with poststructuralists who view science as socially sanctioned propaganda used by the power-elite to dominate the weak. To respond in kind would but confirm poststructuralist views of science. Better to attend to the light S&vB do generate, if only as an effect of fire. It would be madness to get too heated about hue.

Most importantly, S&vB point out the minimal size of some subject samples in Berlin and Kay's earliest studies, and their reliance on bilingual subjects. To Berlin and Kay's credit, they and their fellows have labored for improvements. By presenting subjects with a set of 330 standard color samples in random order and simply recording whatever names the subjects voice, the ongoing World Color Survey of Berlin and Kay and coworkers also mitigates any artifactual effect of fixed color arrays and color terms. The looser structure makes it more difficult to detect the patterns described in their earlier work, but they still do emerge. If biologists are allowed to find the common patterns defining any species, despite such diversities as those between Great Dane and Chihuahua, we must concede that Berlin and Kay and their camp have discerned a pancultural pattern in color languages.

But what does it all mean? Nature is full of robust patterns that are nevertheless superficial. Since ancient times a robust pattern was noted between eclipses and extreme tides, but ruminations about how the darkening of the sun or moon were related to the height of the sea revealed nothing about the causes of the tides. Instead Newton, responding to a challenge by Halley to show that an eliptical orbit would result from a force inversely proportional to the square of the distance, provided the requisite insights into the physics of Earth and the heavens, revealing the coincidence between eclipses and tides to be just that, a mere side-effect of deeper realities. Likewise, the striped pattern of the zebra is robust, and no doubt the effect of its genotype and thereby of its evolutionary history, but no one expects the pattern to provide much insight into the workings of the zebra. Do we have any reason to think that patterns in human color languages provide insight into the workings of the human mind?

Here is an argument that they do not. Human color languages are examples of primitive classificatory schemes. A sophisticated instance of such primitive schemes was the four elements: earth, air, fire, and water. Each element was an external referent defining the focus of a number of extremes. Fire was the extreme of the hot, dry, bright, soft, and light, and so opposed to water as cold and wet, to air as dark, and to earth as hard and heavy. Among them, the four elements spanned all of terrestrial phenomenological space. Any terrestrial article could be located in this space, and any process explained in terms of various blends of the elements. As it ripened an apple absorbed fire from the sun, becoming brighter in color and taste. As late as two centuries ago, Priestley, who is often said to be the discoverer of oxygen, thought of it as dephlogisticated air – not as a gas, but as the elemental air of the ancients from which any admixture of elemental fire had been removed. Further progress required Lavoisier, Dalton, and abandonment of the four elements.

In retrospect, two things are clear. First, the system of four elements made for shallow physics precisely because it attempted to work in terms of human phenomenological space. Going beyond the way things appear to us towards what they are in themselves is the goal of science, whether what is abandoned is Ptolemy's stationary observer at the center of the universe or Newton's absolute time and three-dimensional space. Second, the extremes that are identified as spanning the phenomenological space are quite accidental. Other elements are identified in other cultures (e.g., the ancient Chinese included wood and substituted wind for air). This is quite understandable, because any three noncolinear points will define a plane, and any additional point outside the plane will define a third dimension, and so on. That fire is so often identified as an element, thus constituting a sort of pancultural constant, is not deeply revelatory of human psychology. After all, the world might never have contained fire, but given that it did, it is neither surprising nor important that human beings noticed how hot and bright it is.

Color languages map a subdistrict of human phenomenological space. Like other such systems, they require external referents, such as snow, grass, or Munsell color chips. There is abundant and decisive evidence that color does not reveal much of physical interest: a given blend of radiations may appear as quite different hues, and quite different blends of radiation may appear as identical hues. So red, green, blue, and yellow do not cut any closer to physical reality than do fire, water, air, and earth. the choice of extremes in color is probably just as accidental as the choice of elemental extremes. That red is the sole pancultural focus of hues other than black or white is quite on a par with fire as a pancultural element: an accident that reveals little about the human mind, a mere byproduct of deeper principles.

Cultural practice and perception

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Abstract: In adult humans, conscious visual experience – including that of colour – is shaped by particular cultural practices, as evidenced in the cross-cultural literature. In addition, the practices of our own culture already inform attempts to assess the "natural" experience of newborns or other animals.

Saunders & van Brakel (S&vB) have done a considerable service by providing such a thorough and scholarly critique of the notions that there are nontrivial biological constraints on colour categorisation and colour experience and that the phenomenology and psychophysics of colour are adequately explained by neurophysiologically identified channels. This commentary will have nothing to say regarding the second of these points. With regard to the first, and in relation to the phenomenology of colour, I am generally in agreement with S&vB's conclusions (see Gellatly 1995), although some of their specific arguments are so compressed as to be difficult to evaluate (e.g., final paragraph of sect. 2.2, penultimate paragraph of sect. 4.2, fourth paragraph of sect. 6.1).

It goes without saying that visual experience is dependent on a neurobiological substrate and that the nature of this substrate – for example, dichromacy versus trichromacy – delimits the range of sensory response on the basis of which that experience arises. The question at issue is whether conscious visual experience provides the ground on which linguistic and other social practices are subsequently erected or whether these practices are somehow constitutive of conscious visual experience itself. Posed in this way, the problem can appear seductively simple. Evolutionary and developmental perspectives may tend to suggest a straightforward answer. If newborns of our own species and members of other species have conscious experience, then this must necessarily be free of linguistic and cultural determinants: it follows that if their experience can be shown to be ordered similarly to that of adult members of our own species – and, particularly, of our own culture – then the primacy and universality of conscious visual experience is demonstrated. In research on colour, hue has been taken to be the relevant dimension of experience, and this logic has driven claims that both infants (Bornstein et al. 1976) and members of other species (see Bornstein 1975) naturally partition the spectrum into bands corresponding to red, green, yellow, and blue.

The problem with this general approach is that it fails to recognise the mediating role of the prevailing theory and technology of colour. To determine how the subjects of an experiment partition hues/wavelengths, the experimenters very properly control out orthogonal variables such as brightness and saturation (intensity and purity). Hue is then the only dimension along which discrimination between stimuli is possible. The best the experiment can hope to show is that, in these conditions and at some level, subjects can be forced to discriminate along the hue dimension, and that they then exhibit a partitioning similar to that of adults in our culture. What the experiments cannot show is that the subjects would attend to differences of hue in other circumstances, rather than to differences of brightness, scintillation, and so on. In other words, conscious visual experience may only come to be organised around differences in hue when it is mediated by appropriate cultural practices. As S&vB point out, if hue were inherently and universally salient, children would not exhibit such problems in acquiring supposedly basic colour terms.

That adult experience of colour is not universally organised around the so-called focal colours and their boundaries was shown by Luria (1976). As is well known, his nonliterate subjects preferred to categorise skeins of wool according to brightness rather than hue, just as some of MacLaury's (1992) informants divided his colour chart into horizontal rather than vertical bands. S&vB question whether perception and linguistic description in terms of hues is in any sense more analytical than perception and description in terms of brightness. Similar arguments can be made against Luria's suggestion that the former terminolgy is in some way more abstract than the latter (Gellatly 1995), and against his view that perception and categorisation in terms of brightness is natural and unmediated, whereas that in terms of hue is culturally mediated. Arguments of this kind have a longstanding currency within the literature but, other than for privileging the dominant practices within our own culture relative to other sets of practices, they serve no purpose at all.

Issues relating to culture and perception on the one hand, and to biology and perception on the other hand, are invariably rooted in some such question as: Do they see what we see? Such a question has little meaning until the relevant sense of "see" has been explicated. "Seeing" as a biological function is not the same as the "seeing as," which we consciously experience. In other terminology, this is the sensation/perception distinction or the early/late distinction. The links between the two halves (or levels) of these distinctions remain poorly understood, and our understanding is not aided by claims of simple reduction.

Color-order systems: A guide for the perplexed

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Abstract: If, as Saunders & van Brakel assert, hue, lightness, and saturation characterize artificial color spaces and not the colors of everyday life, one would expect those color spaces to have limited relevance to our understanding of color phenomena and to be of little practical application. This is not the case. Although people perceive these and equivalent color dimensions holistically rather than analytically, they are able to use such triples to categorize the colors of their environment.

For those who are puzzled by Saunders and van Brakel's (S&vB's) critique of color-order systems in their section 5.1, here are a few observations:

(1) What seems to bother Saunders and van Brakel the most about color-order systems is that the samples are decontextualized, abstracted from a host of appearance variables. But others would see this as an advantage. Replacing real systems with ideal ones (and then discovering how to complicate the ideal systems in a controlled fashion to approximate the real ones ever more closely) is a standard and effective technique of scientific inquiry. For example, it was only by abstracting mass from size and weight and chemical composition that mechanics could make real progress. In color science, the use of abstracted variables has likewise led to substantial advances in our understanding. A practiced eye going down the list of features that S&vB say are eliminated by the use of color chips will be struck by how much we have learned by using them in controlled environments (e.g., the effects of surrounds on perceived colors), as well as how many of them can be explicitly defined in terms of the parameters of color-order systems (e.g., colorfulness and nuance in the Natural Color System [NCS]), and how many of them are matters of how the chips are presented (e.g., angular size, gloss, duration).

(2) S&vB assert that "the chips representing the colour space eliminate all aspects of the location of objects, their surfaces, and their relations in the world – the difference between related and unrelated colours" (sect. 5.1). But this is not how the distinction is marked in color science. Aperture colors are unrelated colors; surface colors are related colors. The colors of Munsell chips can be either of these, depending on the circumstances in which they are viewed. The study of such chips will of course not by itself be sufficient to develop a complete theory of color appearance in the full natural environment, but this does not mean that it will not contribute to that theory in important ways, or that we must invoke additional basic color appearance variables.

(3) It is true that "none of the existing systems of colour classification achieves the goal of uniform perceptual intervals" (sect. 5.1), and that when one tries to determine a global metric for a color space by adding together just-noticeable differences, the resulting space turns out to be non-Euclidean. Furthermore, if one tries to achieve the curious goal of a single color space that will represent color relationships for deviant as well as normal observers (Chang & Carroll 1980), one will require more than three dimensions. But why should we thereby conclude that the standard color-order systems are deficient in practice? In fact, they have generally good visual spacing, are Euclidean to a high level of approximation, and have wide-ranging real-world applications. Graphic artists could hardly be without the Pantone system (which maps nicely to the NCS). Munsell chips, a stock-in-trade of art instruction, are used in a variety of tasks from paint formulations to soil-sample analysis. For two decades the Swedish National Building Research Council has subsidized color research in which the NCS has been the chief tool. Color printing and color television do rather nicely with the three dimensions reviled by S&vB. Are all these uses only subserving "particular technical purposes"? Or does the fact that the same basic ideas are successfully used wherever perceptual appraisal of color is important afford some evidence of their validity?

(4) "What experimental support is there for the assumption that colour in daily life consists of three psychologically salient components?" (sect. 5.1). S&vB do not tell us what they mean by "salience" here, but what they appear to have in mind is whether or not hue, brightness, and saturation are psychologically independent. Immediately after asking this question they cite Burns and Shepp (1988), who provide good evidence that the three variables are not psychologically independent and then ask whether or not it makes sense to base a metric on them. (Burns and Shepp remark that the three variables "structure and order perception in a

consistent way," so theirs is not a critique of the use of three variables in standard color-order systems.) But S&vB make no use of these important insights, being content to say that Burns and Shepp "review evidence that physical attributes of colour do not independently affect the psychological dimensions." Actually, Burns and Shepp do very little of this, because their topic is not the relationship between physical and psychological variables, but whether the psychological variables are *psychologically independent of each other.*

Curiously, when S&vB do turn their attention to the issue of psychological independence, it is by saying, "because of the Bezold-Brücke and Abney effects there seems little doubt that hue, brightness, and saturation are interdependent" (sect. 5.2). Now what the Bezold-Brücke and Abney effects actually show is that changes in a single psychophysical parameter, such as dominant wavelength or purity, will affect more than a single perceptual variable – a common theme in psychophysics. This has nothing to do with whether observers have difficulty distinguishing, say, changes in saturation from changes in lightness, which is the sort of thing that counts as evidence for the psychological interdependence of those variables.

To say that these variables are not psychologically independent is to say that subjects respond to colors not analytically, as conjunctions of the variables of lightness, saturation, and hue, but rather holistically, as being similar to, or different from, other colors with respect to their lightness, saturation, or hue. Interestingly enough, Hering appreciated this nonindependence, and color samples in the NCS are scaled according to their degree of resemblance to the six elementary colors – red, yellow, green, blue, black, and white. The NCS estimation procedure, which does not require reference to an array of colored samples and may be used in the natural environment, calls for the subject first to estimate the blackness of the sample (its degree of resemblance to an ideal black), then its hue (its degree of resemblance to one or two of the four chromatic primaries), and finally its chromaticness (its degree of resemblance to the ideal example of that hue). With brief training, people prove to be rather good at this task. The basic human ability on which this performance rests, to exploit what Davidoff (1991) calls "the internal color space," is a central portion of the cognitive foundation of Berlin & Kay's project (1969).

Logic, physics, physiology, and topology of color

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Abstract: This commentary starts with a simplified Cartesian vector space of the tristimulus theory of color. This vector space is then further simplified so that bitstrings are used to represent the vector space. The Commission Internationale de l'Eclairage (CIE) diagram is shown to follow directly and simply from this vector space. The Berlin & Kay results are shown to agree quite well with the vector space and the two-dimensional version of it, especially if the dimensions are normalized to take into account the sensitivity of the eye to the different wavelengths comprising color. There is asymmetry with respect to the colors and a similar asymmetry in vocalic phonemes; these effects can be explained in terms of physiology. The (in)famous problem of the color channels is given a unified treatment at various levels. An eight-valued color algebra is created, with addition and multiplication corresponding to additive and subtractive blending of colors. Finally, it is shown that the discrete Hamming metric for colors has a natural toroidal topology.

1. Introduction. In this target article, Saunders & van Brakel (S&vB) do an exhaustive survey of the literature on color. They seem to go too far, however, in that they are almost claiming that nothing is known about color or that there seems to be no order in it. Most of the article itself seems to be based on nuances of natural languages while at the same time attacking practices such as the use of the term "basic color term" (BCT), which is hazily defined. Specifically, claims (i)–(iv) in the abstract are incorrect or exaggerated. First, the target article itself tries hard to evade the accurate and precise discussions of color by avoiding the mathematical and physical discussions of it, specifically the tristimulus theory, the CIE chromaticity diagram, and the art/science of color production via the additive (RGB[W]) or subtractive (multiplicative) primaries (CYM[K]). In this commentary, using much simplified graphs of the above, plus a color-algebra, we can demonstrate that each of S&vB's conclusions is exaggerated to the point of being incorrect. If S&vB's purpose was to point to the still hazy and incorrect views of color propagated or hypothesized over the centuries, views that might still be found in some books, then the article makes more sense.

First, the standard tristimulus theory is based on distributions or spectra of wavelengths or frequencies. Idealizing the distributions by making them very narrow and highly peaked we show the color space simply as a 3-D Cartesian vector space spanned by the three (additive) primaries (this can be seen in Judd & Wyszecki 1975). The basic ideas, however, can be shown even with binary strings (vectors) as illustrated in Figure 1A. The diagram on the right (Figure 1B) is a 2-D projection of the 3-D space in which the perspective is looking down so that white(111) and black(000) shrink to a point. The 3-D space is distorted so that the subtractive primaries are then shown between the additive primaries. One can see the essentials of the CIE color diagram in this figure. In the CIE diagram the middle point is white, whereas in Figure 1B the middle point represents white, black, and all grays. It is not surprising that the colors at the nodes of this simple 3-D space are all colors that occur earliest in almost all cultural color naming systems, as noted by Berlin and Kay (1969). Hence there is already a correlation between what we expect in terms of the physics and physiology of color and its linguistic expression in various languages.

The one thing we do notice is that the classification systems of humans (after starting off with black/white) seem to be asymmetric in that the BCTs are concentrated toward the red/green side.

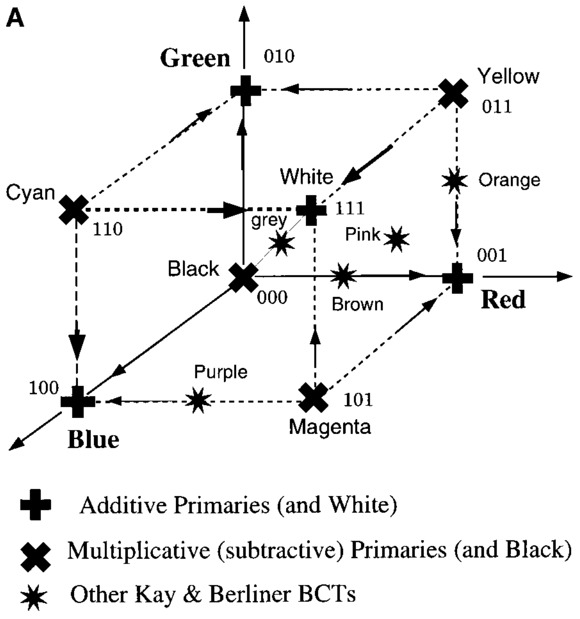

Figure 1A (Hubey). Colors in 3-dimensional vector space. Most of the basic colors can be shown as vertices. To represent the others we need to make use of the concept of a discrete or continuous vector space.

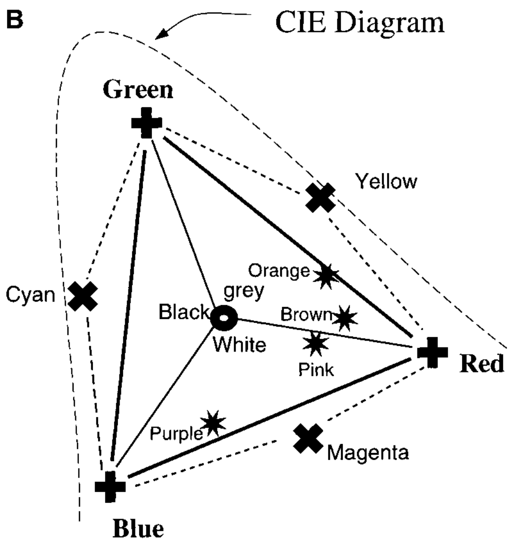

Figure 1B (Hubey). Colors in simplified CIE-like 2-D space. A view of the 3-D space looking down the white-to-black line with a diagram suggestive of the standard CIE chart.

For example, brown is somewhat red/black, pink is red/white, orange is red/yellow, and grey is a black/white combination. The color perception space is more sensitive to the longer wavelengths (red and green); therefore, for the 3-D system (Fig. 1A) and the derived CIE system to be constructed along the lines of sensitivity we would have to stretch the red and green axes. Evidently, the perceptual space is filled in according to two rules: (1) The space is named in the order that makes the colors as distinct as possible (i.e., as far apart as possible), and (2) this distance is modified by the sensitivity to the various wavelengths.

For example, white and black are farthest apart because they are on opposite corners (distance 3; i.e., 3 edges between the nodes). After this, the next choice has to come from one of the additive primaries because the subtractive primaries are all distance 1 from white and are not "as distinct" as the additive primaries. At this point the longest wavelength (i.e., red) dominates because it is far from both white and black. Then the choice should be between green and blue (according to the primaries being selected first); however, apparently because of the favoring of long wavelengths, green (or yellow) gets selected. The selection of yellow can be the result of the relative abundance of this color in nature (i.e., leaves in autumn) as compared to blue. Then, as expected, a blue makes an appearance.

After these primaries we note that the volume that is largest in perceptual terms (i.e., the red region) gets filled in; the only exceptions are grey (which is in the middle) and purple, which is near blue. For whatever reason, there seems to be a similar asymmetry in phonemes of language (see Fig. 2). The most common phonological systems seem to have /aeiuo/, which, in addition to the Truebetzkoy triangle /iua/, contains /o/ and /e/. A more thorough 3-D space of phonemes can be seen in Hubey (1994). The human hearing system seems to be better tuned for the "supervowel" /i/. In the case of colors, the sensitivity of the cones to red and green explains why there seem to be more BCTs favoring the red-green side of the color space.

2. Color channels and power spectra. Treating the 3-D color space vectors as bitstrings (as in Fig. 1A) allows us to assign the bitstrings to the additive primaries in which every primary has a single 1 and 2 zeros. Then the complements (which are the subtractive primaries) are obviously generated by using the bitwise or of the additive primaries. White is represented as 111, which means that it is the sum of all three primaries, and black is 000, meaning that it is the absence of all three primaries. Similarly, multiplying (bitwise and) any two of the subtractive primaries produces the color value of the corresponding additive primary. In the rest of this commentary the word "multiplicative" will be used instead of "subtractive." There are good reasons, aside from the fact that the color algebra works out, for using multiplication. One reason is that subtraction means filtering out a specific range of wavelengths. Using two such filters accordingly refers to subtracting out both sets of wavelengths; this really corresponds to a logical and, which is multiplication. Furthermore, in serial circuits, as in serial subtraction/cancellation, we also represent the action by a logical and.

The opponency can be explained as a simple averaging effect; this is also related to the fuzzy logical xor. For example, a simple average of red (say 650) with green (say 510) gives an average of 580, which is about yellow. Similarly, averaging green and blue gives (510 + 440)/2 = 475 (which is green). We should note that these wavelengths are not the standard colors; however, they do fall in the correct color range. There are two other auxiliary explanations that go with the above. The first has to do with the alleged problem of why, if we average red and blue as above, we do not obtain a similar effect for magenta (because magenta looks like a combination of red and blue rather than a "unique hue" like yellow). The second, related explanation has to do with why the single multiplicative primary (yellow) stands out like a unique hue, whereas the other two multiplicative primaries (cyan and magenta) perceptually resemble some kind of combination of the primaries. All of this is also related to fuzzy-logical xor, which is an

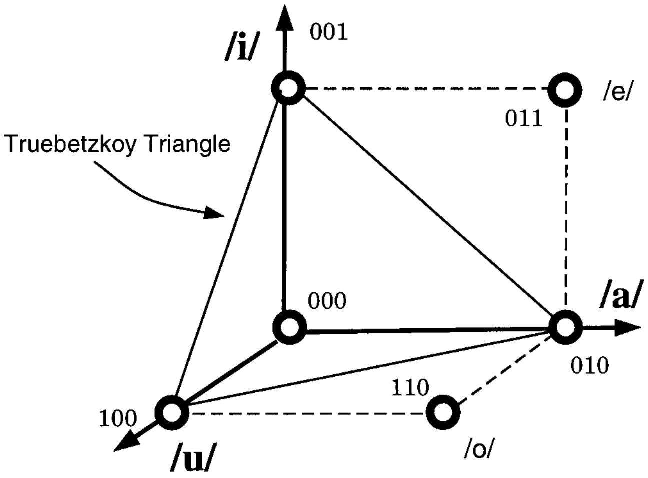

Figure 2 (Hubey). The phoneme vector space. The most common phonemes of languages are not symmetric about the Truebetzkoy triangle but are weighted toward the /e/ and /o/ part of the 3-D vector space.

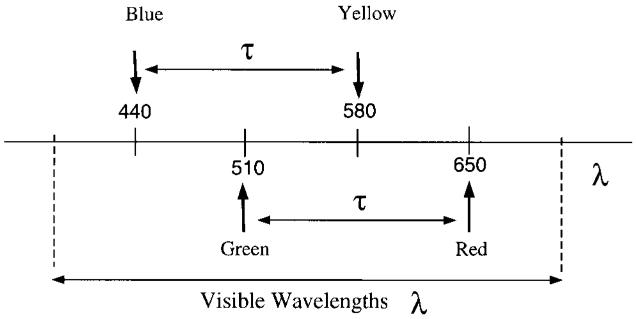

Figure 3 (Hubey). A one-dimensional view of the color spectrum. A highly suggestive representation of the color channel effect in which it is shown that there can only be two such color channels.

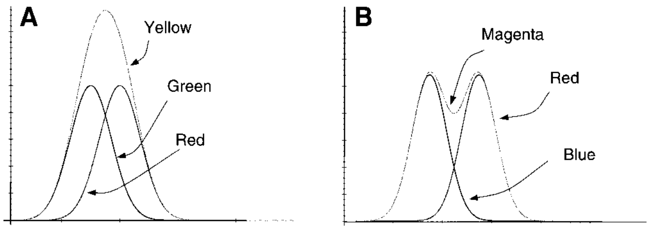

Figure 4 (Hubey). **A:** Yellow from green and red; **B:** Magenta from red and blue.

averaging of sorts (although without division because of the prenormalization of the variables).

These figures show that if the two constituent color densities are close together so that the sum density is unimodal, then the mixture is perceived as a unique hue, on par with the primaries. If the constituents are far apart, then the mixture resembles both colors because the distribution is not unimodal.

If we move the two arrows (i.e., red-green channel) to the right in Figure 3, so that the left arrow is at red (keeping the distance between the arrows constant), then the right arrow extends into the invisible region. Similarly, if we move the right arrow into blue, then the left arrow is off into the invisible region. Hence there seem to be only two opponent channels possible because the response is evidently a function of the differences among the frequencies or wavelengths. Suppose the discrimination capacity, Ψ, is a function of two frequencies, that is, $\Psi(\lambda_1,\lambda_2) = \sigma(\lambda_1)\sigma(\lambda_2)$ (where $\sigma()$ is the sensitivity of a single channel). Defining $\lambda_2 = \lambda_1 + \tau$, we then have $\Psi(\lambda_1 - \lambda_2) = \Psi(\tau) = \sigma(\lambda_1)\sigma(\lambda_1 + \tau)$. Evidently, we have $\tau \approx 140$ nm according to Figure 3. We can see that the averaging process also works out, because the four colors (blue, green, yellow, red) seem to be separated by about 70 nm.

There is yet another way we can understand what is happening. When two unimodal spectral distributions or densities are close together, their sum produces a unimodal density (Fig. 4A), and bimodal one if they are farther apart (Fig. 4B). In a sense, this sum density is an averaged one, or a color halfway between the two. Now, for green and red, the "halfway" color happens to be yellow, and because of unimodality we see a unique hue. If the two distributions are close to each other, they merge to produce a single unimodal distribution whose peak is somewhere around the center. This is the case for the red-green and yellow-blue channels. For red and blue, magenta is still "halfway," but because the two (red and blue) are so far apart, the bimodal distribution allows us to see that magenta is a mixture of the two. A fuzzy-logical `xor`, which is a saddle, can also be used to explain this behavior.

$\times$

	K	R	G	Y	B	M	C	W
K	K	K	K	K	K	K	K	K
R	K	R	K	R	K	R	K	R
G	K	K	G	G	K	K	G	G
Y	K	R	G	Y	K	R	G	Y
B	K	K	K	K	B	B	B	B
M	K	R	K	R	B	M	B	M
C	K	K	G	G	B	B	C	C
W	K	R	G	Y	B	M	C	W

$+$

	K	R	G	Y	B	M	C	W
K	K	R	G	Y	B	M	C	W
R	R	R	Y	Y	M	M	W	W
G	G	Y	G	Y	C	W	C	W
Y	Y	Y	Y	Y	W	W	W	W
B	B	M	C	W	B	M	C	W
M	M	M	W	W	M	M	W	W
C	C	W	C	W	C	W	C	W
W	W	W	W	W	W	W	W	W

Figure 5 (Hubey). Color logic (the eight-fold way). The multiplication (conjunction) and addition tables are shown for the eight basic colors demonstrating that color production can be treated as an eight-valued logic.

3. Algebra of color. We note that identifying addition (bitwise `or`) with the mixing of the additive primaries and multiplication (bitwise `and`) with subtractive blending works very well to explain the behavior in the color space. What we see is that our vision implements an octal logic. The rules for addition and multiplication of the basic eight colors can be seen in Figure 5. DeMorgan's laws are valid, as can be proven by trying every possibility as in a truth table. For example, complementing $R = MY$ we obtain $R' = C = M' + Y' = G + B$. The multiplication table is exact because of the subtractive (`and`) nature of the filters. For example, if we have already filtered out all the wavelengths except blue and red (which combine to form magenta), if we try to filter out all other colors except red (blue) we cannot filter out what is not there.

The addition also works out for the primaries. For example, $Y = R + G$. If we complement both sides, we obtain $Y' = B = R'G' = CM$ as expected. The addition table is, unfortunately, only approximately true in this sense; if we add together a primary, say red, and another color that already contains red, say yellow (green and red), we will obtain a reddish yellow. The table, however, is based on an arithmetic using `or`. So adding 011 and 010 results in 011 (which is yellow) instead of 021 (which could be interpreted to mean 2 parts/intensity red and 1 part green), which would be a reddish yellow. To make the color algebra exact, we either have to specify a kind of *intensity normalization* for addition (which occurs naturally in multiplicative [filtered] blending of colors) or we have to think of the color space as one in which any color that is not a very pure (additive) primary is assumed to be a combination color. This translates into a distortion of the triangle (which is already an approximate CIE diagram) so that small areas are allotted to the primaries. For example, cyan plus red (which is $R + G + B$ but with R intensity greater than the others) would be counted as (reddish) white, whereas in reality it would be pink, which would normally be considered closer to red than white by the average observer. We could, of course, use the arithmetic alluded to earlier in that we can demonstrate that the reality is 112 (for cyan + red) so that the digits assigned to the numbers represent intensities. In this case, when two different classes of primaries are added, the colors obtained will be represented by numbers that also represent the relative ratios of the components.

4. Distance metrics. There is a distance metric among the basic colors that makes use of the Hamming distance (a simplified version of the Cartesian distance): the number of bits by which two bitstrings differ, which translates in Figure 1A and 1B to the smallest number of edges between any two nodes. Clearly all adjacent nodes differ by one bit, and diagonals (complements) differ by 3 bits so that black and white have distance 3, as do magenta-green, and yellow-cyan. The primaries in either class are distance 2 apart from each other.

5. Topology of color. There is yet another way to examine the various dichotomies of the BCTs. We can draw a table (Karnaugh map) comprising the primary colors, RGB, in binary. A zero entry means the complement of the color and a 1, the presence of that then create a torus so that the corners are "neighbors." Any color color. The labeling of the table entries is done according to Gray

B \ RG	0000	0001	0011	0010	0110	0111	0101	0100	1100	1101	1111	1110	1010	1011	1001	1000
00	K		G						R		Y					
01							Gr_1									
11	B		C						M		W					
10													Gr_2			

Figure 6 (Hubey). Two-bit colors. If two bits are dedicated to each of the primary colors then the number of colors representable is 64. In addition to the basic colors that include the additive and multiplicative primaries, black (K) and white (W), the chart also shows several grays (Gr).

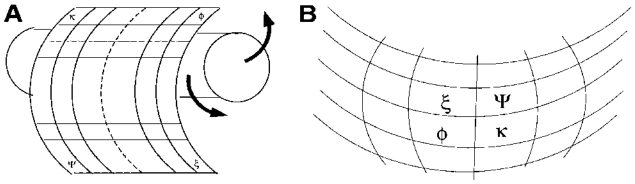

Figure 7 (Hubey). **A:** Any size color map; **B:** Any size color map corners. The sketches show how one can wrap the color map (where any number of bits may be allocated to each primary) on a cylinder and represented on the surface of the torus is distance 1-bit from any of its neighbors.

coding. Now because of the (Hamming) distance metric each entry in the table is a neighbor of (i.e., distance 1) from the one in the same column or row adjacent to it. In addition, the corners are neighbors. In general, we can expand this scheme to represent composite colors with bitstrings as shown in Figure 6. In that figure we are using two bits for each primary so that we can represent 2^6 or 64 colors, including tints, shades, and tones, because white and black are also in the scheme of things. We wrap the Karnaugh map (Fig. 6) around a cylinder (Figure 7A), bringing the corners together as shown in Figure 7B. This results in a torus, so that the corners are "neighbors" as they should be. The Hamming distance still applies to this space with the obvious caveat that the shortest distance between the two points is taken (along the torus surface) as similar to the shortest distance on a sphere. It is obvious that we can dedicate more bits to the primaries RGB to represent more colors and wrap the map torus. The distances on the torus are more than three-dimensional because a torus cannot be topologically transformed into a sphere.

It would probably be best to try to interpret the results of color naming and psychological aspects of colors in terms of the colors on this torus. In this scheme of things, whether the "opponent hues" are "intrinsic" is moot because this only concerns the meaning of the word "intrinsic." The primaries, and those that are composed of them (black/white) are certainly quite "intrinsic" to the whole concept of color: perception, creation, recognition, and naming. The same conclusion can be reached about whether color is "autonomous." There are certainly plenty of rules in color perception and creation, as I have tried to illustrate.

Constraints on the definitions of "unique hues" and "opponent channels"

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Abstract: Zone theories of color vision transform cone sensitivities to channel sensitivities before transmitting these signals to the brain. The concepts of "unique hue" and "color opponency" are fundamental to an understanding of this transformation. Saunders & van Brakel question the objectivity of these concepts. Statements in their target article indicate that the reason for this questioning stems from a failure to appreciate the constraints inherent in the definitions of these concepts.

Saunders & van Brakel (S&vB) argue that the observations underlying conventional zone theories of color vision are weak. In their view, current formulations are largely arbitrary, and may reflect little more than cultural, linguistic, and theoretical biases rather than the true structure of the visual system. On the contrary, zone or opponent-color theories of color vision rest solidly on objective facts, and a consistent, rigorous, and objective theory has been (and is being) constructed to explain these facts. The observations and theories I discuss here are based on psychophysics and deal with Hypotheses 3 and 4 of S&vB's Introduction. I have nothing to say about surveys of different cultures or single-unit recordings.

The two most important color vision theories are the Young-Helmholtz hypothesis (e.g., Wyszecki & Stiles 1982, pp. 582ff), which explains trichromacy, and Hering's theory of opponent colors (ibid, pp. 451ff), which explains the appearance of the spectrum. Zone models incorporate both of these ideas. The observations supporting these models are not open to a host of alternate and equally plausible interpretations – they are not "unconstrained." The characterization of both these stages by psychophysics is convincing. Why are S&vB not convinced? Apparently because they do not understand color opponency.

The spectrum (including the line of purples) or color circle has four unique points, or unique hues. As an example of a unique point, let us choose unique yellow, which lies between 570 and 580 nm for most observers. Over the wavelength range, roughly between 520 and 570 nm, the observer will notice no marked transition in hue. The colors seen are various shades of yellow-green. For wavelengths longer than, say, 590 nm, although yellowness persists, there is no green. Upon crossing a wavelength around 575 nm, something has happened. The color changes from greenish to reddish as a point called unique yellow is crossed. It is sheer obstinacy to deny that this transition is not qualitatively different from crossing a wavelength of, say, 550 nm. It matters not that medieval Christians or Serbo-Croatians used the same word for red and green. Red is not green. What we need here is experimental control. Starve these observers to 80% of their ad lib. weight, put food under the red cup, and see whether they persist in not making this distinction before jumping to Whorfian conclusions about the effect of language on seeing colors. There are objective transition points in the spectrum that have properties not dependent on language. The fact that discontinuities can be described using language does not mean that language causes discontinuities. If we include the line of purples in the spectrum, there are two loci that are neither reddish nor greenish, and two loci that are neither yellowish nor bluish. Hues are discontinuous across these loci.

What is the procedure for objectively specifying colors that are opponent? Continuing with the example using unique yellow, at the hue transition point near 575 nm, which was objectively defined by properties that arbitrary spectral positions lack, the observer is required to identify the hues that seem mixed with yellow on each side of the red-to-green transition point. These are the opponent hues. S&vB seem to believe that there could be an infinite number of opponent hues, inasmuch as every color on the color circle has a complement. Therein lies the root of one of their problems. Opponent hues are defined by their association with the obvious hue transitions in the spectrum. They are not arbitrary, and are not obtained by introspective musings. They are found by experimentally locating hue transition points, or discontinuities. The hues that are canceled or nulled at the transition point but flank each side of the point (say, green on one side and red on the other) are the opponent colors. S&vB repeatedly show evidence that they do not grasp the significance of this simple rule. "Yet *if* the unique red/green, blue/yellow opponencies really were the timeless primitives of colour vision, why was so careful an investigator as Goethe unable to introspect them correctly?" (sect. 3.1). Because, like S&vB, he did not analyze color opponency correctly. Introspection is simply not how you get opponents. You get them by noting the hues on either side of obvious transition points or discontinuities in the spectrum. These, and no others, are the hues that cancel at the transition point.

"Why start with the four unique hues in the first place?" (sect. 3.3). Because there is a simple theory of why these hues, and only these hues, are unique, and why they behave the way they do in cancellation experiments. Jameson and Hurvich's (1955) cancellation experiment is referred to, but its significance seems to have been missed. Jameson and Hurvich's paper is a landmark in the history of opponent theory. It is based on the indisputable fact that discontinuities in the hue perceived occur at certain positions in the spectrum, and it explains them with a simple, parsimonious theory.

S&vB make much of reports of nonlinearities in opponent-colors models. It would be remarkable were there no nonlinearities. Reports of nonlinearity should not be interpreted as attacks on the soundness of the cone-to-channel transformation; they are intended as refinements of a widely accepted theory. None of the points mentioned in a litany ranging from the effects of field size and duration to interactions between channels is a serious challenge. It is strange to see them cited as evidence for weakness of a theory they were intended to strengthen and generalize.

Regarding correlations with electrophysiology: Although certainly desirable, it is not necessary for a psychophysical opponent mechanism to have electrophysiological confirmation, any more than Young's theory of trichromacy required single-cone microspectrophotometry to be a viable theory. Psychophysical opponent-colors theory stands by itself.

Finally, citing contradictory literature proves nothing. In this case it merely shows that S&vB cannot winnow the wheat from the chaff. They accord all facts an equal footing, and submit the resulting contradictions and discrepancies as proof that the field is a can of worms. As much weight is given to Goethe's ineptness as to Jameson and Hurvich's clear, objective measurement of opponent spectral sensitivities.

In conclusion, a parsimonious color-opponent theory based on objective features of spectral appearance exists. Central to this theory are the concepts of unique hues and opponent pairs. S&vB's challenge of the validity of these concepts stems from a failure to appreciate the constraints imposed on the selection of the unique hues.

What Saunders and van Brakel chose to ignore in color and cognition research

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Abstract: Saunders & van Brakel set out to review color science research and to topple the belief that color-vision neurophysiology sets strong deterministic constraints on the cognitive processing of color. Although their skeptism and mission are worthwhile, they fail to give proper treatment to (1) findings that dramatically support some positions they aim to tear down, (2) existing research that anticipates criticisms presented in their target article, and (3) the progress made in the area toward understanding the phenomenon. At the very minimum these oversights weaken the credibility of their arguments and leave the reader to wonder why their discussion ignores what is clearly omitted.

Saunders & van Brakel (hereafter S&vB) are perplexed that there is no simple linkage between color vision neurophysiology and color sensation; they propose that the literature unjustly puts forth a simple explanation of that linkage. The view they present is unbalanced, often citing work superseded by later research, and using extreme claims from the literature. Although they correctly deem incomplete the present understanding of color neurophysiology as it relates to color sensations, they mistakenly imply that the enterprise has made no progress. Below I focus on some issues S&vB chose to overlook to demonstrate how their analysis is wrong.

1. S&vB ignore color-appearance similarity judgments. There is a body of similarity scaling research, ignored by S&vB, indicating that hue, saturation, and brightness (hereafter HSB) are real psychological constructs. The work suggests that the polar structure of dimensions HSB has psychological reality in color-appearance similarity judgments. HSB accounts for much of the variance found in judgments of similarities among visible color space appearances, implying that these are psychologically "natural," cognitively relevant dimensions. This is typically found even when similarity criteria are not explicitly suggested by an experimenter. These data from color-appearance similarity judgments also conform systematically to the relational structure of HSB.

Research by Indow and colleagues (cited in Indow 1988) shows that although there are no logical reasons that colors should be embedded in a metric space according to their mutual relationships, similarities of surface colors (Munsell colors) can nevertheless be embedded in a 3-D manifold with locally Euclidean metric. These multidimensional scaling (MDS) results suggest that individual subjects exhibit a "sweet spot" embedded in the 3-D solid of their similarity judgment scaling (Indow & Aoki 1983; personal communication). That is, for colors in a hollow cylindrical-solid region (corresponding approximately to Munsell Value 3-8 and Chroma 4-6) similarity judgments for stimuli are highly consistent, resulting in a uniform metric, whereas judgments for stimuli on the extremum of the dimensions tend not to be as regular and yield a weaker metric. Although the observed cognitive metric is not uniform over the entire visible color-space, this core region of high regularity suggests an appropriateness of the dimensional constructs HSB.

Moreover, Indow and colleagues' (1991; 1992) assessment of discrimination thresholds for surface versus aperture color appearances found remarkably similar ellipsoid tolerances for the two stimulus formats, similar to those estimated by MacAdam (1942). One could interpret these results as implying that the three dimensions of HSB appropriately model individual color-appearance matches. With respect to these MDS studies of hue, saturation, and brightness dimensions Indow concludes that "the global system is perceptually real" (1980, p. 6).

These similarity judgments, analogous to color judgments made in everyday settings, are obtained under a variety of empirical tasks, stimulus modes, and scaling methods, and over numerous tests converge on the conclusions presented above. Indow's findings are corroborated by less systematic work of Shepard (1978; 1994) and others. Dunn (1983) reviews the scaling literature and concludes that the *only* dimensions that yield dissimilarity in terms of a Euclidean metric are HSB. Furthermore, Pruzansky et al. (1982) found that surface color space is one of the few cognitive domains that can be appropriately represented in a 3-dimensional Euclidean space.

Although the discussion remains open on some parameters of the metric space (Chang & Carroll 1980) and on whether HSB are integral or separable, the fact remains that these "dimensional interaction(s) are mirrored in the patterns of scaling data" (Burns & Shepp 1988, p. 505). S&vB mention none of this, yet clearly this counts toward a validation of the psychological importance of HSB. S&vB should put aside the attribution of HSB to only color-ordered systems. Even if Munsell never put ink to paper-swatches we could still obtain similarity distances using colors in the world and the resulting scalings would still reflect the relational structure of HSB described above.

2. S&vB ignore HSB as practical cognitive constructs. If one chose to ignore the above empirical work, one would still need to account for the uses of HSB in everyday practice. Designers of user-interfaces make use of hue and brightness cues (and, to a lesser extent, saturation explicitly) to convey information to display operators. Instances range from some air traffic control systems to naval tactical data symbologies. Systems based on the general principle that there is a systematic relationship between the intensity of a signal light and the perceived brightness of that signal have been in operation since the introduction of automated information representation, and are “to a first approximation . . . valid” (Travis 1991, p. 55). The fact that financial considerations largely determine whether a system is maintained or discarded underscores the practical value of this principle.

Jameson, Kaiwi, and Bamber (in preparation) show that observers can utilize both a luminance/brightness code and a gradient hue code effectively. Moreover, when those two dimensions are combined into a single code that simultaneously varies along one dimension of brightness (ranging from dark to light) and another color dimension (a gradient of green-yellow-red), naive subjects can parse this code efficiently on the two described dimensions independently (making both correct detections of signals and correct classifications of the information content conveyed by the chromatic code).

Gegenfurtner and Kiper (1992) provide psychophysical evidence for two independent and equally efficient mechanisms tuned to a luminance axis or a chromatic axis, and imply that detection performance is limited by putative luminance and chromatic mechanisms, but involves mechanisms that combine luminance and chromatic information (p. 1885). This speaks to the psychophysical relevance of these dimensions, and the robustness of the dimensions in practical domains suggests a psychological “naturalness” contradicting S&vB’s Conclusion 3.

3. S&vB ignore certain experts. S&vB’s Abstract (iii) and Conclusions (1), (3), and (5) posit the nonexistence of linking propositions and neurophysiological opponent mechanisms tuned to the Hering fundamentals. Neurophysiologists and psychophysicists have indicated frequently in print that these linking propositions are yet to be completely understood (see Burns et al. 1984; Krantz 1975; 1989; Krauskopf et al. 1982; Lennie & D’Zmura 1988; all cited Jameson & D’Andrade, in press). Mollon (1995) states again: “There is no physiological evidence in the visual system for cells that secrete the sensations of yellowness and blueness or redness and greenness. The two subsystems found in the retina and visual pathway simply do not correspond to Hering’s two axes” (p. 144). One could cite additional examples.

Thus, on this issue S&vB say nothing new. Moreover, although they acknowledge a few dissenting voices, they fail to give proper consideration to experts who are critical of the dominant theories and who recognize that much needs to be done before there is a “correct” theory. As in all active areas of science, the field is still incrementally evolving. In particular, S&vB overlook the fact that constructs like hue and brightness are still understood as works-in-progress by almost everyone in the area. Although only one facet of S&vB’s analysis is considered here, similar and, in some cases, more damaging arguments can be made against their remaining critique.

Science ≠ imperialism: There are nontrivial constraints on color naming

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Abstract: Saunders & van Brakel’s claim that Berlin and Kay (1969) *assumed* a language/vision correlation in the area of color categorization and disguised this assumption as a finding is shown to be false. The methodology of the World Color Survey, now nearing completion, is discussed and the possibility of an additional language/vision correlation in color categorization is suggested.

***1. Introduction: Observation and inference in* Basic Color Terms.** In 1969, we published a short monograph (Berlin & Kay 1969; hereafter *Terms*) advancing two broad hypotheses: (1) There are semantic constraints on the basic color lexicons of the world’s languages; and (2) basic color lexicons grow in a constrained fashion. At the time, we guessed that both sets of constraints must have something to do with universals of color vision, independent of language, although we had no idea what those might be. We expressed this view on the penultimate page of that monograph:

> A fundamental problem which remains unsolved is the explanation for the particular ordering found. Given that cultural evolutionary factors may explain the gross numerical growth in the size of basic color vocabulary, why are terms added in a partially fixed order and why in this particular order? Our essentially linguistic investigations have led, seemingly inescapably, to the conclusion that the eleven basic color categories are pan-human perceptual universals. But we can offer no physical or psysiological explanation for the apparently greater perceptual salience of these particular eleven color stimuli, nor can we explain in any satisfying way the relative ordering among them (*Terms*, Berlin & Kay 1969, p. 109).

Below, we will suggest that since 1969 modest progress has been made in relating cross-language universals of color naming to properties of the visual system. Before undertaking that task, we pause briefly to reject the Saunders and van Brakel (S&vB) claim that our inference actually went in the direction opposite from the one we reported. We wrote that cross-linguistic investigation had revealed universal constraints on the semantics of color, from which we inferred that these constraints might well be grounded in vision. S&vB claim that the cross-linguistic universals we presented as an empirical finding were assumed *a priori* and somehow built into the experimental procedure, so that the results were artifactually constrained to turn out the way they did. (We have numbered the sentences in the following paragraph for ready reference.)

> (1) Berlin and Kay assumed that the perceptuolinguistic basic colour system is innate, biologically constrained, and (semi-) automatic. (2) In the absence of any reason to suspect members of other speech communities having different automatisms, they felt justified in taking the American English colour lexicon as a standard. (3) Experiments were set up in such a way that performance could be transposed into competence through a generating or translation rule. (4) This revealed that at the meta-level, as in American English, there were exactly 11 BCTs. (5) Although it is suggested that BCTs were the *result* of cross-cultural empirical research, this lexicon was in fact derived from the most popular American-English colour terms in Thorndike’s *Teacher’s handbook* (via Brown & Lenneberg [1954]) (S&vB, sect. 2.1, para. 5. Emphasis in original).

Sentence (1) is false. *Terms* contained no assumption, explicit or otherwise, regarding an innate “perceptuolinguistic” system. S&vB offer no evidence for this assertion.

Sentence (2) is false. American English color words were not used as a standard. Again, no supporting evidence is presented. English color words were used in *Terms* to *gloss* universal categories. These categories showed up in the close clusterings of the responses of speakers of 20 languages who were asked to name in their own languages color stimuli identified in the Munsell system of color notation. Writing in English, it would have been perverse to gloss such a cross-language response cluster with *rojo* (Spanish), *krasny* (Russian), *nchi* (Western Apache), or *kula* (Tongan), rather than *red* (English), or to render it with a list of Munsell notations opaque to most readers.

Sentence (3) does not readily yield a straightforward interpretation. Presumably, the words “competence” and “performance” refer to the distinction introduced by Chomsky (e.g., 1965, pp. 3ff). Although we are familiar with the competence/performance distinction, the sentence, “experiments were set up in such a way that performance could be transposed into competence through a generating or translation rule,” is opaque to us.[1]

Sentence (4) appears to be saying that the (illegitimate) method described in sentence (3) led to the (improper) conclusion that there exist 11 universal basic color categories. If this is indeed the intended assertion of sentence (4), it is unsupported – although often repeated – by S&vB.

Sentence (5) is false. There is no reference in *Terms* to “Thorndike’s *Teacher’s handbook*” or to Brown and Lenneberg (1954, hereafter BL).[2] As best we can recall, neither of us has ever mentioned “Thorndike’s *Teacher’s handbook*” in print or in person to anyone, including each other, in this or any connection.[3]

The *only* evidence S&vB present for the claim that Berlin & Kay assumed a universal language/vision correlation and coerced the cross-language data to support it is the following: “We find we can only understand this work [i.e., *Terms*] on the assumption that Berlin and Kay had a strong *a priori* belief that just as ‘biological foundations of . . . language . . . must exist for syntax and phonology’ so ‘basic color lexicons suggest such connections are also . . . found . . . in the realm of semantics’ (Berlin & Kay 1969, pp. 109f)” (S&vB, sect. 2.1, para. 4.) The passage quoted by S&vB is drawn from the last two sentences of *Terms.* Berlin & Kay present this parallel as a tentative conclusion, not as an assumption.

2. Empirical procedures. On our way to suggesting some tentative recent advances in correlating universals of color semantics with properties of the visual system, we will have occasion to review our empirical methods, both in the experiments of the 1960s and in more recent investigations. Readers may judge for themselves whether these methods are biased toward finding Western-like semantic structures in non-Western languages. In light of S&vB’s claim that Berlin and Kay – and, by implication, others working in the same tradition – have artifactually built a false finding of semantic universals into their method of investigating color naming cross-linguistically, it is worth noticing that both the stimuli and almost all of the procedures used by Berlin and Kay and their associates were taken directly from the classic study of Lenneberg and Roberts (1956, hereafter LR; see also BL). Both the LR and BL studies were conducted in search of effects confirming the Whorf hypothesis of radical linguistic relativity. Both LR and *Terms,* as well as several intervening investigations in this tradition, have used an array consisting of 40 equally spaced Munsell hues at each of 8 equally spaced levels of lightness (Munsell “Value”) at the maximum saturation (Munsell “Chroma”) currently available for each hue/lightness point.[4] In addition, Berlin and Kay and their associates have presented speakers with a series of neutral hues varying in equal steps of lightness. This is not the place to evaluate the psychological validity of the Munsell coordinate system. It suffices here to note that no reason has been proposed by S&vB or anyone else to suppose that choosing this coordinate system for colors, rather than another, tends to impose Western categories on non-Westerners.[5] This same array of color stimuli has been used repeatedly by researchers looking for relativistic effects. Just as the stimuli used in *Terms* were the same as those used in the original cross-language study of Lenneberg and Roberts, so were the experimental procedures: color names were first obtained without the stimulus array and then each speaker was asked to indicate for each color term investigated (a) all the colors denoted by the term and (b) the color(s) most aptly denoted by the term.

If these were the procedures used in LR, what were the results?

> A comparison of the responses of monolingual Zunis with the responses of English speakers reveals that with only one striking exception most of the color-categories of one language have an equivalent category in the other. The exception, however, is interesting. In English yellow and orange are very sharply defined, separate categories whereas in Zuni (as spoken by monolinguals) there is only one category encompassing both orange and yellow. Even more interesting is the following comparison of the overall structure of the entire color space in the two languages (Lenneberg & Roberts 1956, p. 30)

In their search for relativistic effects, Lenneberg and Roberts go on to characterize some rather subtle statistical differences in their aggregate pictures for their English, Zuni monolingual, and Zuni bilingual groups, indicating three respects in which the bilingual group might be considered transitional between the two monolingual groups. They concentrate on the differences, having acknowledged their primary finding to be that the English and Zuni color term systems are on the whole very similar, Zuni simply lacking a separate term for orange and including orange and yellow colors under a single term. We rehearse this ancient history only to make the point that Lenneberg and Roberts were looking for Whorfian effects in their Zuni-English comparison (as were Brown & Lenneberg in their English-internal study). Berlin and Kay borrowed both the stimuli and the elicitation procedures from investigators who were looking for relativistic effects.[6] The fact that our method was closely modeled on that of LR was reported in several places in *Terms* (p. 3, pp. 103–4, note 3) and so was known to S&vB. S&vB’s claim that the findings of *Terms* are an artifact of Western-biased methodology is not only unsupported: given the source of Berlin and Kay’s empirical procedures, it is *prima facie* implausible.

The Berlin and Kay Munsell stimulus array is shown in Figure 1. Row A is comprised of 41 pure white chips (Munsell neutral Hue, Value 10). Row J has 41 pure black chips (Munsell neutral Hue, Value 1). Column 0 contains 10 neutral colors ranging from pure white (A0), through mid-brightness gray (E0, F0), to pure black (J0). Columns 1 through 40 represent equally spaced Munsell hues, from Munsell red 2.5 in column 1 to Munsell red-purple 10 in column 40. Rows A through J represent equal, descending steps of Munsell value (lightness) from 10 through 1. Each of the 320 nonneutral chips (that is, those in rows B-I and columns 1–40) is at the maximum saturation available for that hue/value combination. The hues range from yellow-reds (starting in column 1), through yellows, greens, blues, purples to purple-reds (column 40). A color print approximating the original array may be found in the 1991 paperback reprinting of *Terms* and in Kay et al. (1991a).

3. The World Color Survey (WCS). Since *Terms* appeared in 1969 further work on cross-language color naming has been done by numerous researchers in many different languages (see Maffi 1991 and additional items cited by S&vB). Perhaps the most wide-ranging new research carried out since 1969 is that of the World Color Survey (WCS),[7] a large-scale comparative color naming project whose initial data acquisition stage was completed in the early 1980s (see Berlin et al. 1985; Hardin & Maffi 1997; Kay et al. 1991; Kay et al., in press; Kay et al., in preparation; MacLaury 1992; MacLaury, in press; Maffi 1991).[8] We provide a brief description of the WCS project, its methods, and some of its basic findings, to make two points: (1) The methodology is not biased

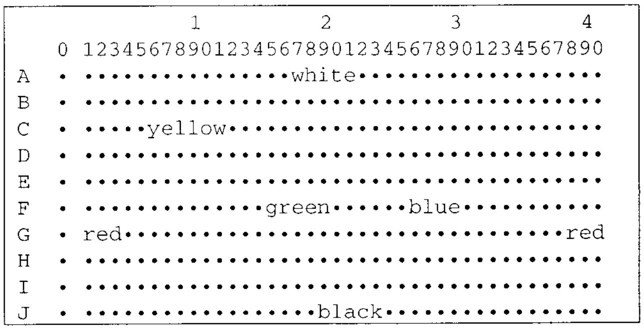


Figure 1 (Kay & Berlin). Diagrammatic representation of the B & K Munsell color array with English words loosely suggesting the location of the Hering primaries.

toward imposing Western color categories on non-Western languages, and (2) the linguistic data do reveal (a) universal constraints on color naming and (b) associations between constraints on color naming and apparent properties of color vision.

The field research for the WCS was conducted by trained field linguists of the Summer Institute of Linguistics, who collected color naming data using a stimulus array substantially the same as that of Berlin and Kay. Comparable data on naming ranges and focal choices for basic color terms were collected *in situ* on 110 languages representing a wide range of language families. In most cases 25 speakers were interviewed per language. Monolingual speakers were sought insofar as possible. A methodological departure of the WCS from the method used in *Terms* was that chip-naming judgments were obtained on 330 individual chip presentations rather than on the fixed array of 330 color chips. (Kuschel & Monberg 1974 used a similar procedure). The individual chips were encased in 35mm photographic slide covers, arranged in a standardized random order, placed in metal slide boxes and sent to the field investigators. An identifying numeral (1–330) was printed on the back of each slide for ease in recording. In the naming task, each speaker was shown the chips one by one and asked to name the color. The responses were written in coding booklets that were eventually computerized for analysis.

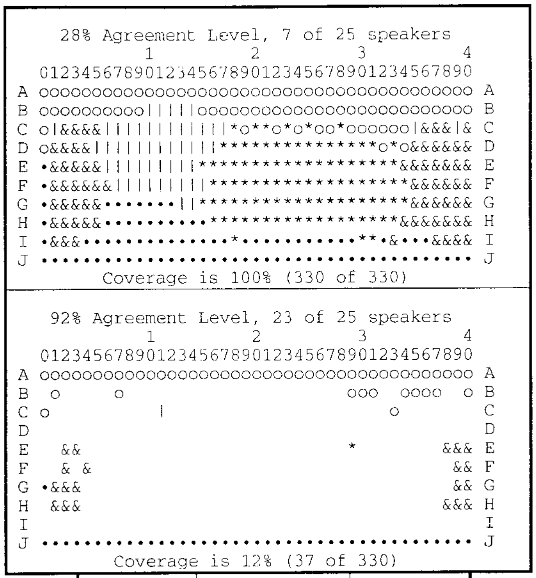
The coded data of the WCS are represented in several different types of display over the grid of 410 Munsell chips shown in Figure 1.[9] An *individual naming array* shows for a single speaker the naming response given to each of the 330 stimuli in the form of a symbol that is keyed to the native language term by which that chip was named. An *aggregate naming array* shows for a given language the response most often given for each chip, provided the response reaches a specified frequency, called the *level of agreement*. Thus, each aggregate naming array has a specific level of agreement attached: The x% level of agreement shows the responses for all and only those chips whose most popular response was given by at least x% of the responding speakers. A *term map* presents for a given term a summary of its denotation. Term maps reveal the internal structure of (gradient) color categories; how they are constructed is described below. Examples of each of these types of arrays can be seen in Figures 2–4.

Figure 2 presents two aggregate naming arrays for Sirionó, a Tupian language spoken by approximately 500 people in the lowlands of Bolivia. Figure 2, with the accompanying key, shows that Sirionó has five basic color terms that can be glossed as black (including brown) *erondeì,* white *eshĩ,* red *eĩrẽĩ,* yellow (including orange) *echo,* and "grue"[10] *erubi.* Each of these five terms is represented in the aggregate naming array corresponding to the 92% level of agreement, and at the 28% level of agreement, the complete stimulus array is covered by these and only these terms.

Sirionó term maps for these five basic color terms are presented in Figure 3. There is a separate map for each term. In the term map for a given term t, each chip c receives a typographical symbol (including blank) of visual "density" intuitively commensurate with the degree of consensus among speakers regarding the use of t to name c, specifically, commensurate with the proportion of the speakers naming any chip with t who name c with t.[11]

A term map depicts the internal structure of the category denoted by a single color term. Because a term map is based on the naming responses of all cooperating speakers who use the term, it maps the corresponding category at the interpersonal level. High-agreement symbols tend to occur in the interior of categories and lower agreement symbols at the edges. Term maps also give an accurate summary of the degree of consensus among speakers regarding the denotation of a term.

Finally, individual naming arrays for two Sirionó speakers are given in Figure 4. Generally speaking, individual arrays illustrate significant individual variation, the two shown in Figure 4 being unusually similar, although this variation does not obscure the patterns that emerge from the aggregate naming arrays and the term maps.



```
   28% Agreement Level, 7 of 25 speakers
            1         2         3         4
  01234567890123456789012345678901234567890
A ooooooooooooooooooooooooooooooooooooooooo A
B oooooooooo|||||oooooooooooooooooooooooooo B
C o|&&&&||||||||||||*o**o*o*oo*oooooo|&&&|& C
D o&&&&||||||||||||***************o*o&&&&&& D
E •&&&&&|||||||||*******************&&&&&&& E
F •&&&&&&|||||||||*******************&&&&&& F
G •&&&&&•••••••||********************&&&&&& G
H •&&&&&••••••••••******************&&&&&&& H
I •&&&••••••••••••••*•••••••••••**•&•••&&&& I
J ••••••••••••••••••••••••••••••••••••••••• J
       Coverage is 100% (330 of 330)

   92% Agreement Level, 23 of 25 speakers
            1         2         3         4
  01234567890123456789012345678901234567890
A ooooooooooooooooooooooooooooooooooooooooo A
B  o     o                     ooo  oooo  o B
C o          |                     o        C
D                                           D
E   &&                          *        &&& E
F   & &                                   && F
G •&&&                                    && G
H  &&&                                   &&& H
I                                           I
J ••••••••••••••••••••••••••••••••••••••••• J
        Coverage is 12% (37 of 330)
```

Symbol	Term	Gloss
•	*erondeì*	black
○	*eshĩ*	white
&	*eĩrẽĩ*	red
|	*echo*	yellow
*	*erubi*	grue

Figure 2 (Kay & Berlin). Two Sirionó aggregate naming arrays (25 speakers, 12 F, 13 M).

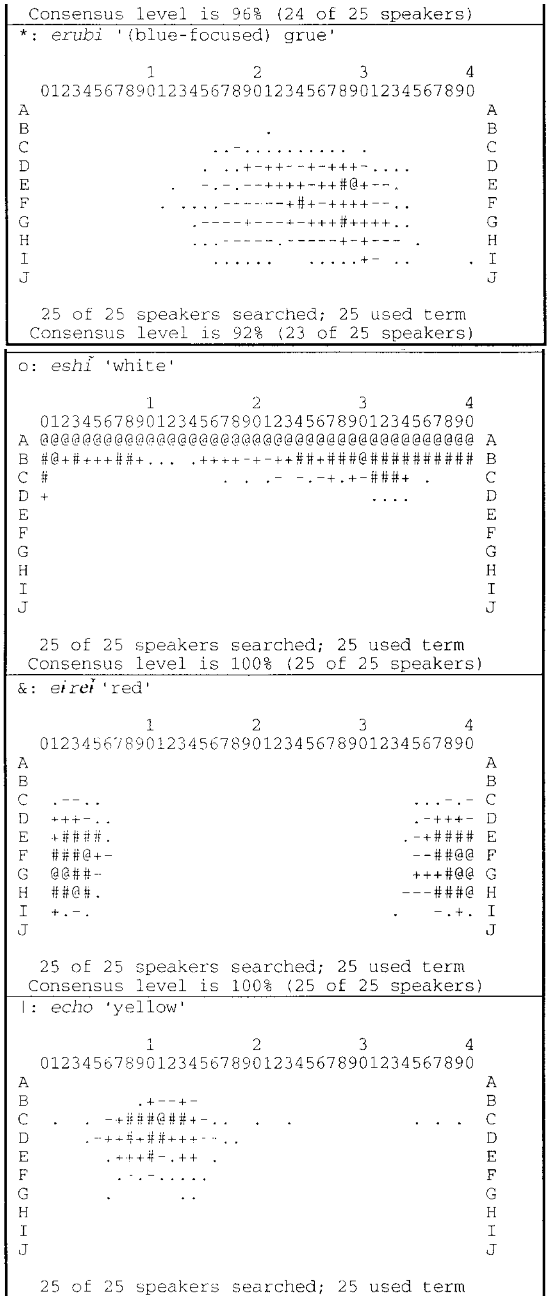
4. Cross-language constraints on color naming. The WCS data allow us to observe several cross-language generalizations in the color naming behavior of speakers from the 110 languages in our sample. Although a full exposition of these interlanguage generalizations awaits a monographic treatment (see Kay et al., in preparation), preliminary analysis of the WCS data controverts the two substantive points regarding color naming that are advanced by S&vB.

Methodological considerations aside, S&vB make one substantive claim and one empirically investigable theoretical suggestion in the domain of color naming. The substantive claim is that "Linguistic evidence provides no grounds for the universality of basic colour categories" (S&vB, abstract). S&vB's theoretical suggestion presupposes the falsity of this claim. S&vB cite approvingly Tornay's (1978a, p. xxxi) suggestion that it is Western colonialism, rather than the biology of color vision, that accounts for the universals proposed by Berlin and Kay in the semantics of color terms.

> Alternative explanations of tendencies to basic colour categories across languages were not considered [in *Terms*]. For example, Tornay (1978a, p. xxxi) proposed the history of the progressive domination of the West and its values accounts for apparent universality. This seems a plausible suggestion with respect to what is often quoted as Berlin and Kay's most solid result (S&vB, sect. 2.1, para. 9).

According to S&vB, there are no cross-linguistic constraints in the semantics of color and it is "the progressive domination of the West and its values" that accounts for them!

The WCS data refute each of these claims.[12] The data reported below support the hypotheses that (1) there are universal semantic constraints in color naming, and (2) not *all* cross-language similarities can be explained by processes of diffusion from one language (e.g., a colonial language) to another (e.g., a local language).[13] Specifically, while all the politically dominant European and Asian languages have basic terms distinguishing red



```
 Consensus level is 96% (24 of 25 speakers)
*: erubi '(blue-focused) grue'

            1         2         3         4
  01234567890123456789012345678901234567890
A                                           A
B                      .                    B
C                 ..-.......... .           C
D                . ..+-++--+-+++-....       D
E             .  -.-.--++++-++#@+--.        E
F            . ....------+#+-++++--..       F
G               .----+---+-+++#++++..       G
H               ...-----.-----+-+--- .      H
I                 ......   .....+- ..     . I
J                                           J

  25 of 25 speakers searched; 25 used term
 Consensus level is 92% (23 of 25 speakers)
o: eshĩ 'white'

            1         2         3         4
  01234567890123456789012345678901234567890
A @@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@@ A
B #@+#+++##+... .++++-+-++##+###@########## B
C #                . . .- -.-+.+-###+ .     C
D +                              ....       D
E                                           E
F                                           F
G                                           G
H                                           H
I                                           I
J                                           J

  25 of 25 speakers searched; 25 used term
 Consensus level is 100% (25 of 25 speakers)
&: eìrẽĩ 'red'

            1         2         3         4
  01234567890123456789012345678901234567890
A                                           A
B                                           B
C  .--..                             ...-.- C
D  +++-..                            .-+++- D
E  +####.                           .-+#### E
F  ###@+-                            --##@@ F
G  @@##-                             +++#@@ G
H  ##@#.                            ---###@ H
I  +.-.                            .   -.+. I
J                                           J

  25 of 25 speakers searched; 25 used term
 Consensus level is 100% (25 of 25 speakers)
|: echo 'yellow'

            1         2         3         4
  01234567890123456789012345678901234567890
A                                           A
B          .+--+-                           B
C  .  . -+###@##+-..  .  .           . . .  C
D     .-++#+##+++--..                       D
E       .+++#-.++ .                         E
F        .-.-.....                          F
G       .      ..                           G
H                                           H
I                                           I
J                                           J

  25 of 25 speakers searched; 25 used term
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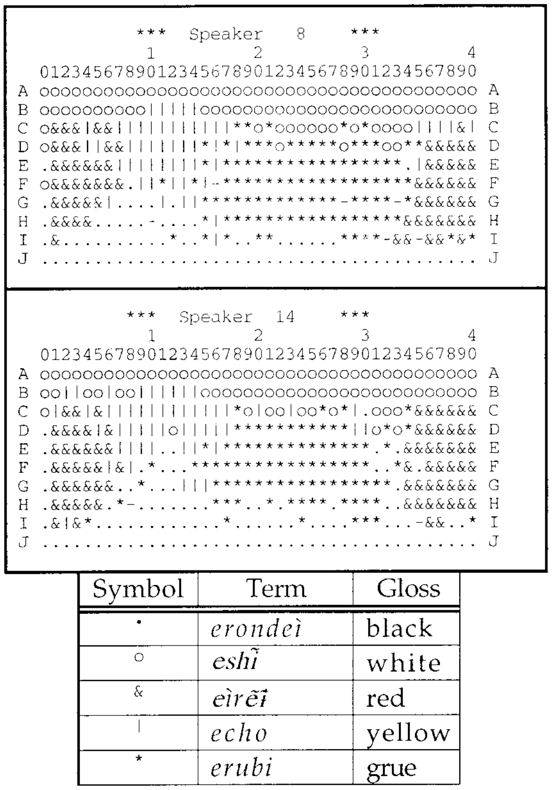
Figure 3 (Kay & Berlin). Sirionó term maps (# = 81–100%; + = 61–80%; − = 41–60%; . = 21–40%).



```
              ***  Speaker   8    ***
            1         2         3         4
  01234567890123456789012345678901234567890
A ooooooooooooooooooooooooooooooooooooooooo A
B oooooooooo|||||oooooooooooooooooooooooooo B
C o&&&|&&|||||||||||**o*oooooo*o*oooo||||&| C
D o&&&||&&|||||||*|*|***o*****o***oo**&&&&& D
E .&&&&&&||||||||*|*****************.|&&&&& E
F o&&&&&&&.||*||*|-******************&&&&&& F
G .&&&&&|....|.||*************_****-*&&&&&& G
H .&&&&.....-....*|*****************&&&&&&& H
I .&..........*..*|*..**......****-&&-&&*&* I
J ......................................... J

             ***  Speaker  14    ***
            1         2         3         4
  01234567890123456789012345678901234567890
A ooooooooooooooooooooooooooooooooooooooooo A
B oo||oo|oo||||||oooooooooooooooooooooooooo B
C o|&&|&||||||||||||*o|oo|oo*o*|.ooo*&&&&&& C
D .&&&&|&|||||o|||||***********||o*o*&&&&&& D
E .&&&&&&||||..||*|**************.*.&&&&&&& E
F .&&&&&|&|.*...*****************..*&.&&&&& F
G .&&&&&&..*...|||*****************.&&&&&&& G
H .&&&&&.*-.......***..*.****.****..&&&&&&& H
I .&|&*............*......*....***...-&&..* I
J ......................................... J
```

Symbol	Term	Gloss
•	*erondeì*	black
○	*eshĩ*	white
&	*eìrẽĩ*	red
|	*echo*	yellow
*	*erubi*	grue

Figure 4 (Kay & Berlin). Individual naming arrays for two Sirionó speakers.

from yellow and basic terms distinguishing green from blue, many unwritten languages (as well as documented earlier stages of the major written European and Asian languages) reveal the presence of the underlying universality of the red, yellow, green, and blue percepts by encoding in one basic term either a category that covers just what is covered by *red* and *yellow* in English (equivalently, by *mérah* and *kuning* in Bahasa Indonesia), or a category that covers just what is covered by *green* and *blue* in English (equivalently by *yaroq* and *kahol* in Hebrew), or both. There are also rare cases of yellow-or-green categories, but no cases of categories denoting just red-or-green or just yellow-or-blue, a result consistent with opponent theory.

The fact that languages that do not have separate basic terms for each of the six Hering primaries tend strongly to contain terms whose denotations cover sets of two or three perceptually adjacent Hering primaries supports the universal finding that basic color systems are based, with rare and partial exceptions, on the Hering primaries: red, yellow, green, blue, black, and white.[14] The existence of such composite categories, as they were termed by Kay and McDaniel (1978), shows that the constraints on color naming shared by colonial and local languages cannot all be caused by dissemination from the former to the latter because there is nothing in English, French, Spanish, German, or Dutch (the major languages of Western colonialism) that could induce blue-or-green or red-or-yellow categories, for example, in the languages of the colonized peoples. (Not to mention that [1] many local languages were reported to have composite categories at the time of contact with the West and [2] earlier stages of the now dominant European and Asian languages [e.g., Latin, Japanese] contained composite categories [e.g., green-or-blue: L. *viridis*, J. *ao*], and lacked terms for some of the Hering primaries [e.g., Latin "blue," probably Japanese "green"].)[15]

The array of composite categories in the WCS data reported in Kay et al. (1991) are all reducible to unions of Hering primaries. Kay and McDaniel (1978) proposed that the categories denoted by the basic color terms of the world's languages are of three types: (1) composites, the unions of two or more Hering primaries, for example, red-or-yellow, green-or-blue,

(2) Hering primaries, for example, red, green, and (3) intersections (mixtures) of Hering primaries, for example, orange (red-and-yellow), purple (red-and-blue). The WCS data confirm that all categories denoted by basic color terms tend strongly to fall into one of these types.[16] The fact that local languages often contain categories of the composite type, which are absent from the languages of the colonizers, shows that not all cross-language constraints on color naming can be caused by contact between colonial and local languages.

At the time of this writing, 63 of the 110 languages in the WCS sample have been fully analyzed.[17] Of these, 46 have a basic color term that translates well into English as *red* (or into Western Apache as *nchi* or into Tongan as *kula*, etc.). There are 41 that have a separate yellow term (including two slightly unclear cases) and 24 that have a separate green term. These 24 include 5 cases in which it is somewhat unclear whether the term marks a separate green category or a category focused in green but extended throughout grue. There are 19 languages that show a separate blue term, including 5 cases that are possibly blue-focused grue. In addition, two Spanish-influenced languages have adopted the *celeste* "light blue"/ *azul* "dark blue" distinction into their basic color vocabulary. Native words predominate for all categories, although there are borrowings from both European and non-European languages.

Given previous experimental examinations of cross-language color naming going back as far as LR, this degree of similarity between unwritten languages and familiar European ones is not surprising. More interesting because more exotic are the composite categories observed in this subsample of 63 unwritten languages.[18] A summary of the term maps for the 19 green-or-blue and the 9 red-or-yellow terms found in these 63 languages are presented below.

In BCT, only 2 of the 20 languages in our original sample exhibited grue categories. On the basis of our survey of the literature, we were able to document another 17 languages whose descriptions suggested to us that they too included a green-or-blue composite color category. The WCS data reveal an additional 19 experimentally investigated languages which show grue as a well established category. The denotative range of grue in these 19 languages follows a narrow pattern, as can be seen in Figure 5. Figure 5 demonstrates that the term maps for the grue terms in each of the 19 languages possessing grue terms are each quite similar to the term map for grue in Sirionó, shown in Figure 3, and hence are quite similar to each other. Ideally, we would display here each of the 19 grue term maps, but that is precluded by considerations of space. Figure 5 was constructed by outlining the non-blank area on each of the term maps for grue in the 19 relevant languages. (A few outlying chips were not included in the outline, an "outlier" for a given term map being defined as a chip that receives a symbol of the term map which is not adjacent to any other chip receiving a symbol on the term map. There were only a handful of these in all, most languages, like Sirionó, containing no grue outliers).

It is clear that a term covering just the percepts of green and blue is a popular choice for languages that do not have independent green and blue terms. The highest level of consensus is sometimes shaded toward green, sometimes toward blue. Often it is fairly evenly distributed. There is also some variation regarding the degree to which purple is included in a predominantly green-or-blue term.

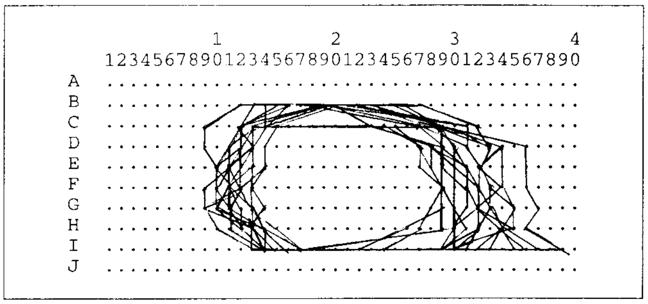


Figure 5 (Kay & Berlin). Green-or-blue in 19 languages.

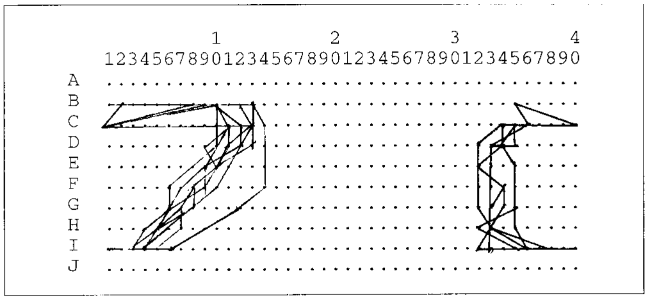


Figure 6 (Kay & Berlin). Red-or-yellow in nine languages.

Red-or-yellow terms are less frequent than green-or-blue terms in this sample, as may be predicted from the fact that separate red terms and yellow terms outnumber separate green terms and blue terms. (If a language has distinct basic terms for red and yellow it does not by definition contain a basic term for red-or-yellow. See *Terms* pp. 5-6.) Figure 6, employing the same procedure as used in Figure 5 for green-or-blue terms, shows the outline of the red-or-yellow term maps for the nine languages in our sample with red-or-yellow terms.

The data just reviewed make it clear that, contrary to the claims of S&vB (sect. 2.1), (1) linguistic evidence does provide "grounds for the universality of basic colour categories," and (2) a substantial fraction of this evidence cannot be explained by "the progressive domination of the West and its values."

5. Color naming universals and color vision. In this commentary we have discussed the red-or-yellow and green-or-blue composites to the exclusion of the other composite categories identified by Kay and McDaniel (1978) and integrated into a developmental model in Kay et al. (in press). One reason for this is that these two composites, based on cross-language color naming data, correspond precisely to the two channels of hue information at one stage of a recent 4-stage model of color perception that is based on a wide range of neurophysiological and psychophysical data (De Valois & De Valois 1993; 1996). Although there is much anatomical and physiological support for the earliest stage of the De Valois's model, especially involving cone types, frequencies of cone types, linkage of cones to horizontal and/or bipolar cells and the behavior of all these types of cells, De Valois and De Valois do *not* claim that the stage referred to here, their Stage 3, whose output consists of distinct red-or-yellow and green-or-blue channels, corresponds to a particular anatomical structure. Nevertheless, it is interesting that a multi-stage model of color perception based on neurophysiological data posits a stage with two channels of hue response, red-or-yellow and green-or-blue, whereas cross-linguistic color naming research independently establishes basic color terms denoting red-or-yellow and green-or-blue categories to be widespread in languages that do not have separate basic color terms for all of the Hering primaries.

In the De Valois model,[19] combined spectral and spatial opponency at the level of the midget bipolar cells, created by the complex connections among horizontal cells, individual cones, and bipolars, produces six kinds of cells at the second stage of the model: (1) L_o cells respond positively to L cones and negatively to the average of all cones weighted by their relative frequencies; (2) $-L_o$ cells respond negatively to L cones and positively to the same weighted average; (3) M_o cells respond positively to M cones and negatively to the weighted average; (4) $-M_o$ cells respond negatively to M cones and positively to the weighted average; (5) S_o cells respond positively to S cones and negatively to the weighted average; and (6) $-S_o$ cells respond negatively to S cones and

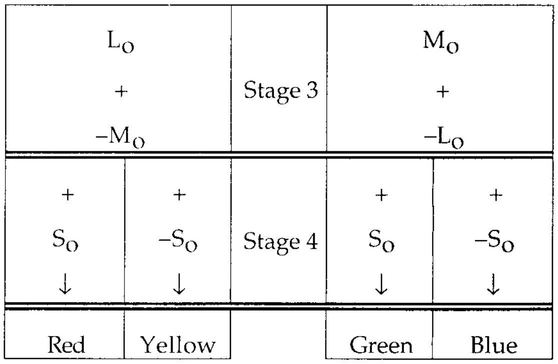


Figure 7 (Kay & Berlin). Stages 3 and 4 of the model of De Valois and De Valois (1993; 1996).

positively to the weighted average. At Stages 3 and 4, the L_o, $-L_o$, M_o, $-M_o$, S_o, and $-S_o$ signals are combined in two steps, as indicated in Figure 7. (Fig. 7 is an abridgment of Fig. 6 of De Valois & De Valois 1993, omitting the treatment of luminance information).

At Stage 3, there are two channels ($L_o + -M_o$) and ($M_o + -L_o$). At the fourth stage (1) the former is divided into red and yellow by the addition of S_o and $-S_o$, respectively and (2) the latter is similarly divided into green and blue by the addition S_o and $-S_o$, respectively. The two outputs of hypothetical Stage 3, *if some neurological structure should correspond to this stage,* would give us a neurological basis for the red-or-yellow and green-or-blue categories so often observed in the color vocabularies of local languages. We repeat that De Valois and De Valois make no claim for the italicized hypothesis. Still, this suggestive correspondence between higher-level outputs in a model based on statistical analysis of individual cell behavior on the one hand, and findings in cross-language color naming on the other, appears worthy of further investigation.

ACKNOWLEDGMENTS

We are indebted to Luisa Maffi for much useful advice regarding all aspects of this commentary and to C. L. Hardin for comments on an earlier draft.

NOTES

1. For Chomsky, linguistic competence is tacit (unconscious) knowledge of language. Linguistic performance consists in the actual, on-line production and interpretation of utterances. Linguistic performance, according to Chomsky, reflects many psychological abilities and disabilities apart from linguistic competence, such as limitations of memory, allocation of attention, perceptual and motor constraints, distractions, and so on. The relevance of this distinction to the empirical method of *Terms* is obscure. Also obscure are the intended meanings of the expressions “transposed” and “a generating or translation rule” in sentence (3). Readers of this journal who are not linguists should not suppose that sentence (3) uses linguistic terminology in a standard way.

2. A reference to BL appears in the supplementary Bibliography, prepared by Luisa Maffi (1991), which appears in the paperback reprinting of *Terms.*

3. Nor, for that matter, has either of us mentioned Thorndike and Lorge’s (1944) *The teacher’s word book of 30,000 words*, the only work authored by Thorndike to which Brown & Lenneberg (1954, p. 457) refer.

4. See, for example Landar et al. (1960). BL and Lantz and Stefflre (1964) used slightly smaller sets of Munsell colors, chosen on essentially the same principle: fair sampling of the Munsell space. All of these studies sought to establish relativistic effects in color categorization.

5. A “postmodernist” might argue that any coordinate system for color is necessarily Western-biased: because coordinate systems for color are elements of the scientific tradition and the scientific tradition is part of Western culture, no coordinate system for representing the denotata of color words can be legitimately used in the study of a non-Western language. Acceptance of this view would seem to remove the issue from the scientific arena. We do not know whether S&vB currently hold this view. However, in a volume entitled *Post-modernism and anthropology* (Geuijen et al. 1995), Saunders and van Brakel argue that Kay and McDaniel’s (1978) “reductionist argument . . . of six basic or atomic colour categories . . . to Fundamental Neural Response categories,” is invalidated by the prior epistemological principle that “there is no privileged discourse in which what is true is independent of our choices, hopes and fears” (S&vB 1995, p. 170). The fact that this argument does not appear in S&vB’s current paper may or may not indicate a moderation of their position toward one compatible with scientific discourse. It remains unclear whether S&vB currently hold that any uniform coordinate system used in color naming research necessarily imposes Western categories on non-Western languages.

6. But who were nonetheless punctilious in not exaggerating their successes in this endeavor. For a more recent study of Whorfian effects in the color domain, see Kay and Kempton (1984).

7. Robert E. MacLaury’s Middle-American Color Survey has also provided extensive new data. See MacLaury (1986, 1979, and references cited in the latter).

8. Saunders and van Brakel (1995) review the preliminary analyses of the WCS data made available in microfiche form as Berlin et al. (1991), making many of the same claims regarding color naming as in the paper under discussion.

9. The displays modeled on Figure 1 depict 410 chips, despite there being only 330 distinct stimuli, because the top and bottom rows of the display (rows A and J) each consist of 41 tokens of a single chip type. That is, row A contains 41 identical white chips and row J contains 41 identical black chips. The effect is something like a Mercator projection of the skin of the color solid, with extreme stretching at the poles.

10. “Grue” is an abbreviation we will use for “green or blue.”

11. If at least 81% of the speakers who name any chip with *t* name chip *c* with *t,* then *c* receives “#.” If 61–80% of the speakers who name any chip with *t* name *c* with *t*, then *c* receives “+.” If 41–60% of the speakers who name any chip with *t* name chip *c* with *t,* then *c* receives “−,” and so on, as indicated in the legend above the term maps. The chip(s) with the highest absolute level of consensus (independent of the group they fall into) are taken to represent the focus of *t,* and are marked by @. At the bottom of each term map is a sentence of the form: “Consensus level is X%, Y of Z speakers.” Here, X is the proportion indicated on the map by @; Y is, therefore, the number of speakers who name with *t* the chip(s) most often named with *t,* and Z is the number of speakers who name any chip with *t.*

12. Since the first claim is false and the second claim presupposes the first to be false, the second claim is coherent, but it is nonetheless false.

13. There is, of course, no question that interlanguage influence is a major factor in color term evolution. See, for example, the discussions of Javanese in *Terms* (pp. 87f), Siwi (pp. 89f), Lebanese Arabic (p. 91), Bahasa Indonesia (p. 91), Bulgarian (pp. 41, 92), Swahili (p. 40), Korean (p. 40) Malay (p. 97), and Tagalog (p. 100).

14. See Kay and McDaniel (1978), among others, as listed in Kay and McDaniel (1978, p. 620, note 5).

15. Kristol (1980) points out that several modern Italic dialects have never developed a basic term for blue, retaining a reflex of *viridis* to cover all of green-or-blue. (He also proposes that *caeruleum* was a basic term in classical Latin for blue, but his arguments for this are not persuasive and the weight of evidence in André’s comprehensive [1949] study of Latin color terms suggests that *caeruleum* was never a basic color term.)

16. There are infrequent exceptions (or possible exceptions). See, for example, the discussion of the peripheral red category in Kay et al. (in press).

17. All the papers cited above that reported on the WCS data have been based on preliminary and partial analyses. For example, term maps were not generally available, and the data contained errors, including coding errors, which have now been corrected.

18. The selection of this subsample from the full sample of WCS languages is, with haphazard exceptions, alphabetical. The selection of the WCS sample from the full population of the world’s unwritten languages was determined primarily by the presence or absence of a Summer Institute of Linguistics missionary linguist in the field area.

19. Actually, in one of two versions of the model. The difference between the two versions is irrelevant here.

Ethnographic evidence of unique hues and elemental colors

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Abstract: Contrary to argument that unique hues are undemonstrated, the World Color Survey shows that speakers of more than 100 minor and tribal languages focus color categories predominantly on 4 of the 40 hue columns of the ethnographic Munsell array. The pattern is not conditioned by saturation levels or other arbitrary structures among the color chips, nor is Western influence likely to be the cause. Moreover, all evidence suggests that color cognition is autonomous despite the connotations and polysemies of color terms.

Saunders and van Brakel (S&vB) place little faith in the evidence of four unique hues, citing the quandaries of vision research. But Kay et al.'s (1991b) World Color Survey (WCS) shows that speakers of minor and tribal languages focus color categories predominantly on 4 of the 40 hue columns of the ethnographic Munsell array. This pattern is not conditioned by saturation levels or other arbitrary structures among the color chips, nor does it result directly from Western influence.

The three-part Figure 1 elaborates these points. Part (a) represents the ethnographic Munsell array of 330 chips, the 1976 issue used in the WCS. Part (b) displays 15,186 color-term foci placed by 2,476 speakers of 107 of the 110 WCS languages. (Data from three languages pend a check of their preliminary processing.) Foci are included regardless of whether terms are basic or nonbasic or whether they name hue, saturation, brightness, or another quality of the light sense. This policy precludes distortion of the sample by the analyst, but the consequent noise decreases the percentage of terms that will be focused in reference to hues. The darkened cells mark the plurality peaks of foci in white (2,105), black (1,983), red (472), yellow (537), green (194), and blue (169), the 6 densest, noncontiguous clusters on single chips. Part (c) represents the frequency of foci distributed across the hue columns in the ethnographic array. There are four steep ascents to an apex in the single columns that intersect the purest examples of red, yellow, green, and blue (cf. MacLaury 1992, Fig. 1). The 4 favored columns contain 2,543 of the 10,644 chromatic foci, that is, 24%. Pure chance would amount to 10%. The numbers build up to the four columns in stepped fashion from troughs between. Apparently, speakers of 107 languages have responded to a perceived structure.

Comparing parts (a) and (b), we find an influence of the arbitrarily curtailed saturation levels: chip C9 manifests the highest saturation in yellow, /16, and it attracts the plurality peak, which is even higher than that of red at G1. However, the red, green, and blue peaks at, respectively, G1, F17, and F29 occur on chips of a saturation equal to that of their neighbors, yet the difference between highest and surrounding numbers is almost as pronounced as the difference seen in yellow. Although the unevenness of saturation in the ethnographic array is influential, it does not cause the frequencies shown in part (c). Probably, the distribution of foci as well as its stepped contour reveal a widely recurrent response to perceiving the closest approximations to unique hues attainable with chip pigments. Indeed, people everywhere perceive four unique hues and often include them in the meanings of color terms, allowing that red is never quite unique under other than laboratory conditions.

What explains the 76% of chromatic foci that do not fall on unique hues? Various cognitive processes occasion the deviation; These are inferred from correlations between focus placements and other quantifiable data. The model and evidence fill a book (MacLaury 1997). Elder speakers of languages, such as Australian Anbarra or Kuku Yalinji, appear to favor brightness when they name color (Jones & Meehan 1978, p. 27; Hargrave 1982, p. 208). Watkins (1969, pp. 1508 and 1517) reconstructs "to shine" as a gloss of major color terms in Proto-Indo-European. S&vB mention other possibly valid causes of disinterest in hue, but without ethnographic attestation. Because they produce no evidence that culture-specific connotations or polysemous meanings of color terms encourage people to focus the terms on colors other than the unique hues, their hypothesis of nonautonomous color naming is unsupported.

Western influence appears to have had little direct effect on the distribution of foci: The WCS shows color-term ranges that are patently exotic, although their foci are not (MacLaury 1992, Figs. 11–13). However, bombardment of any people with foreign information may push them to think analytically as a routine way of coping, and the world is becoming more complicated for everyone, including tribal peoples. Peralta (1980) argues that cognitive concentration directs more photons into the *fovea centralis,* where most hue receptors are lodged. Thus, globalization may indirectly occasion an increase of hue naming at the expense of other

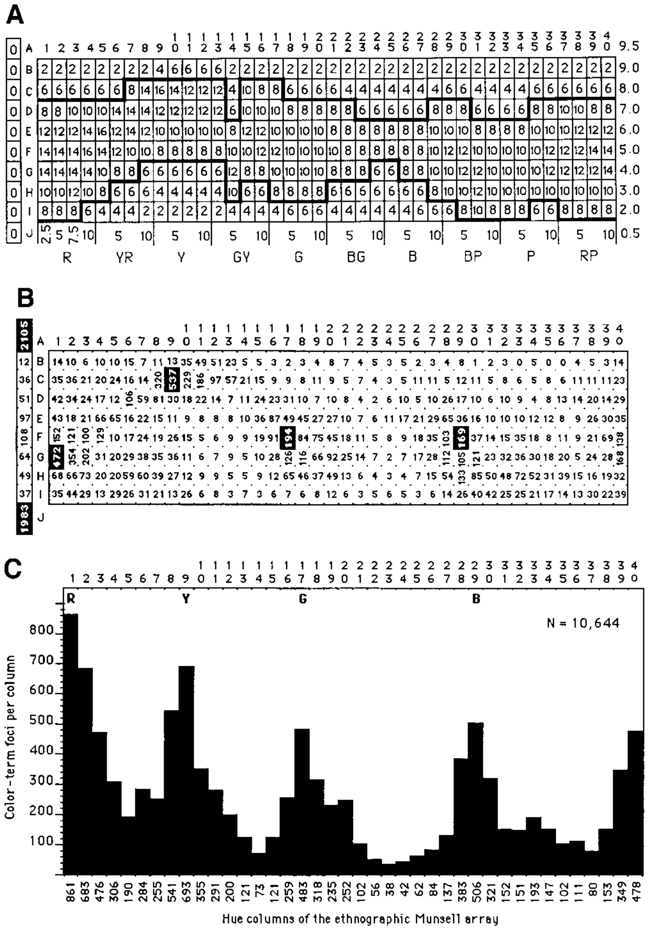

Figure 1 (MacLaury). **A:** Specifications of Munsell color chips constituting the ethnographic array, simplified at left and top or complete at bottom (hue), right (value), and within each cell (chroma). Heavy lines distinguish chroma at /6 and below from /8 and above. The left column represents achromatic white-grey-black. **B:** Foci (N = 15,186) of color terms collected by the WCS from 107 minor and tribal languages. Data include only the foci confined to one color chip, excluding foci covering adjacent chips. Each number totals the different individuals who focused a term on the corresponding chip. Plurality peaks on extremes of value and different hues are marked by dark background. **C:** Frequency distribution of 10,644 WCS color-term foci across the hue columns of the ethnographic Munsell array.

options. The question, then, is not only about what most people do nowadays, but also about the full range of demonstrable human potential at all times and places. Saunders & van Brakel rightly plead to keep this question open.

Why bother about opponency? Our theoretical ideas on elementary colour coding have changed our language of experience

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Abstract: There is no natural and pretheoretical classification of colour appearances into hue, saturation, brightness, unique hues, and so on. Rather, our theoretical insights into the coding of colour have reciprocally shaped the way we talk about colour appearances. Opponency is only one of many fundamental aspects of colour coding, and we are hardly justified in ascribing some theoretical prominance to it.

Saunders & van Brakel (S&vB) address an important question about the connection between two cognitive domains, each of which has a fixed and rich innate internal structure. They attack a tangle of (often only vaguely expressed) ideas that seem to be widely shared by colour scientists. They argue, quite rightly in my view, that there is no theoretical or empirical evidence to justify the view that linguistic colour categories were biologically fixed in a theoretically interesting way by properties of elementary colour coding. Unfortunately, however, the target article makes it all too easy for those who want to argue for the contrary, because it is burdened with many side issues that are completely irrelevant to their point (e.g., the problem of linearity of opponent codes). In addition, it often reflects fundamental misunderstandings about the principles and goals of investigations into the internal coding of colour (e.g., that "channels do not necessarily exist outside" of experimental conditions, sect. 3.3, and that experiments that use spectral lights are not representative of the chromatic world humans live in, sect. 4.2).

1. Colour attributes. Unlike spatial and geometrical attributes of visual perception, colour perception cannot be fruitfully studied in isolation because colour coding is, from the outset, intrinsically interlocked with these other aspects of visual perception. One cannot overemphazise the point stressed by Koffka (1936, p. 129) that "a general theory of colour must at the same time be a general theory of space and form."

If so, then how did we arrive at our vocabulary for describing abstract and decontextualized colour appearances? History (and, from a different perspective, developmental psychology) shows that the construction of a colour terminology is a cultural achievement that mirrors not only the significance of certain biologically important objects, but to an increasing extent the invention and cultural role of coloration techniques and dyeing processes. Our abstract colour terms derive mostly from either concrete objects or material properties, for example, shiny, glossy, speckled, dull, drab, and resplendent (see Hochegger 1884; Marty 1879; Waetzold 1909). Our conscious awareness is of objects and their material character, whereas colour appearances only seem to be a kind of medium we are reading through, as it were, in the visual system's attempts to attain functionally the biologically significant object. Because biologically and functionally crucial aspects of the perceptuo-motor system do not map in a natural and direct way to language, they have to be "reconstructed" by conscious cognitive activity, such as similarity classification and abstractive categorization.

Hence, it does not come as a surprise that scientific insights into the nature of internal colour coding have themselves, since the last century, reciprocally influenced the way we categorize and classify colours. The "basic colour attributes" of hue, saturation, and brightness are not a natural classification of colour appearances, but were, in the wake of Helmholtz, derived from the corresponding elementary physical operations. Many writers in the early literature were aware of the physicalistic trap of slicing the nature of colour perception along the joints of elementary physics. Hering (1920a) rejected the concept of saturation altogether as a mixing-up of perceptual and physical aspects. Stumpf also completely dismissed "saturation" as a colour attribute (Stumpf 1917, p. 86). He considered saturation to be a cognitive abstraction capturing the approximation of a colour to its ideal. In a similar vein the concept of saturation was rejected by many others, among them Katz (1911; 1929) and K. Bühler (1922). Von Kries was aware of such problems but he preferred to trade psychological arbitrariness for an apparent precision of colour concepts that results from their strong ties to physical operations. He remarked that a division of colour appearances in terms of hue, saturation, and brightness "does not claim to be a natural one; without much ado we can regard it as a completely arbitrary one" (von Kries 1882, p. 6). Rather than being linguistic universals, our concepts for classifying colour appearances arose from practical needs (e.g., norms for referring to colours) or for theoretical purposes of, say, explanatory simplification.

2. Opponency. Until now, attempts to formulate a coherent and empirically satisfying theory to account for our intuitions about opponency in colour coding have met with little success. The field is strewn with highly experiment-specific models of opponent coding. The theoretical picture does not gain in coherence when we turn to neurophysiology. Given the complexity now apparent both in psychophysical observations and in neurophysiological findings, we can but deplore the widespread willingness to call on ad hoc pseudo-explanations hastily for isolated psychophysical phenomena in terms of equally isolated neurophysiological findings. We certainly know much more about aspects of opponent coding than about many other, probably more fundamental aspects of colour coding. But why so much ado about opponency when we turn to study our language of experience?

3. Beyond opponency. To the extent that colour science focusses on attributes and coding properties of isolated, decontextualized colour patches, fundamental features tend to escape our theoretical attention. The most important seem to me those aspects that relate to the dialectic relationship between lights and surfaces. We still lack a suitable theoretical language for the phenomenal description of the percepts associated with the interplay of perceived illumination and perceived objects that must deal with aspects of, for example, vagueness, abstraction, and categorization. In the classical literature we find many attempts to describe carefully the phenomenal peculiarities that are characteristic of colour appearances under (chromatic) illumination. Von Helmholtz (1911b, p. 243) described them as "colours that can be seen *at the same location* of the visual field one behind the other." Bühler (1922, p. 40) emphazised that "colours appear as if they were composed of the actual object colour and a coating by the chromatic illumination." Katz (1911, p. 274) noted "the curious lability of colours under chromatic illumination." Similar observations can be found in Hering (1888), Fuchs (1923), or Gelb (1929). These and other theoretical and empirical observations support the idea that all colour processing is cast into a *dual* colour code based on innate semantic perceptual categories of "object colour" and "illumination colour" (Mausfeld 1997), which, in turn, are intimately interwoven with codes based on elementary perceptual categories for the representation of surfaces, form, and space. This fundamental dialectic relationship between lights and surfaces came, in the course of evolution, to be deeply mirrored not only in perceptual coding but, arguably, also in language.

A monochrome view of colour

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Abstract: Saunders & van Brakel's criticism of Berlin & Kay's methodology misunderstands the fact that scientific hypotheses are tested by generating new, replicable data with novel explanatory power. Thus, although Berlin and Kay studied differences in colour words *between* languages, the same patterns are also present in colour word usage *within* languages, in a range of literary and other textual databases.

Saunders & van Brakel (S&vB) have written a strongly argued criticism of the influential work of Berlin and Kay (1969). They have several criticisms, but they seem largely reducible to methodological failings of one sort or another. If in Berlin and Kay they find "an appearance of sloppiness," then it can only be said that in their own criticisms there is an appearance of nitpicking, and of the setting up of artificial standards, unrealistic and arbitrary hurdles that perhaps no study could ever meet. Worse still, such standards are perhaps irrelevant. Of course it would be better to study many more languages with hundreds of bilingual speakers, not one of whom was acculturated to the West (but is that possible if they are properly bilingual?). Surely these criticisms are close to becoming irrefutable? And of course there are a thousand confounding measures that Berlin and Kay did not take into account. To be flippant, it *might* be the case that the colour of the interviewer's socks altered the respondent's behaviour; and there were of course no controls for this. But do such criticisms mean we can discount the Berlin and Kay hypothesis entirely?

Somewhere, S&vB have misunderstood the nature of science. It creates *hypotheses* that may or may not be right or useful. Its test is in findings that replicate and are useful for predicting and explaining other unanticipated phenomena. And as such the Berlin and Kay hypothesis is undoubtedly extremely useful. Of course more extensive data would be nice, and it is therefore strange to find S&vB not mentioning the World Color Survey (WCS) of Kay et al. (1991).[1]

The WCS looks at 25 speakers from each of 111 languages. Whether this will be sufficient for S&vB is not clear; one has a sneaking suspicion that it will not be, but it certainly appears methodologically sound. That the original Berlin and Kay position is genuinely a working hypothesis is shown in the WCS analyses by the discovery of some anomalies that require the original schema to be modified (although they hardly invalidate the basic formulation in any serious way).

Does the Berlin and Kay hypothesis provide insight into data remote from those on which the hypothesis was created? The original study says colour names are not mere categories but can be ordered, with each colour term having a number indicating its evolutionary antiquity, and white and black being older in some sense than orange and purple. Berlin and Kay derived their ordering from comparisons *between* languages, and said nothing about differences between colour words *within* languages. Differences in the use of colour words within a single language that correlate with the Berlin and Kay order therefore provide indirect support for the meaningfulness of that ordering. In 1983 I reported three sets of data on the frequency of colour words in English poetry, in English novels, and in Chinese poetry, in which there was a highly significant association with the Berlin and Kay order, older words being used more (McManus 1983). The older words also had longer entries in the Oxford English Dictionary, were listed more often in spontaneous colour word listings, and on the semantic differential had higher evaluation and activity, but not potency (McManus 1983). Previously, Hays et al. (1972) had reported similar colour word frequency effects in English, Spanish, French, German, Russian, Romanian, and Hebrew. Recently I (McManus, in press) have extended my earlier study by using eight large and very different computerised text bases (Biological Abstracts, Dissertation Abstracts, English Poetry, English Verse drama, GeoRef, MathSci, MedLine, and PsycLit). The 11 basic colour terms were used more than half a million times, and again there was a highly significant correlation of frequency with the Berlin and Kay ordering (Spearman's rho = 0.802; n = 11; p = .003), with no significant heterogeneity between sources. Such results suggest that the Berlin and Kay hypothesis satisfies the main criterion of being scientifically testable – it can generate new and testable hypotheses. Without Berlin and Kay it is difficult to produce any coherent explanation of such consistent differences that is not merely *ad hoc.*

Ultimately, the criticisms of S&vB have a familiar ring: they are those of the Standard Social Science Model (SSSM), so eloquently described by Tooby and Cosmides (1992) with its strong emphasis on the strength of social and cultural influences on psychological processes, and the denial of biological or neurological influences. That description also overlaps with the "radical cultural relativism" that Kay et al. (1991a) suggest afflicts so much cultural anthropology. As a result, perhaps the S&vB target article misses much of the potential excitement when both cultural *and* biological factors come into a dynamic, evolutionary interplay. To use an obvious but nevertheless appropriate metaphor, their analysis is disappointingly monochrome, perhaps even jaundiced, when full colour is so much more interesting.

NOTE

1. As an aside, there is here "an appearance of sloppiness" in S&vB, because although they mention the paper, albeit only as an aside, they cite it wrongly in their final manuscript; it actually appeared in the *Journal of Linguistic Anthropology*, not *Linguistic Anthropology,* and the proper title is "Biocultural implications *of systems* of colour naming" [my emphasis; S&vB's omission]. It is tempting to suggest that this a secondary citation, just as the criticism of the colour preference work looks secondary, in that Davidoff (1991) is cited, but not McManus et al. (1981), which specifically addresses the question about preference for hue, saturation or chroma, discussed only rhetorically by S&vB.

Over the rainbow: The classification of unique hues

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Abstract: Saunders & van Brakel's analysis of the phenomenal categorization and subsequent experimental research in unique hues fails to include contemporary methodological improvements. Alternative strategies are offered from the author's research that rely less on language and world knowledge and provide strong evidence for the general theoretical constructs of elemental hue, nonbasic, and basic color terms.

There is no doubt that one may find occasional anomalous results from the long history of color research. However, 300 years of experimentation have yielded *some* methodological progress. Unfortunately, in section 4.1, Saunders & van Brakel (S&vB) concentrate mostly on early work that had serious flaws and ignore the more recent methodological refinements for determining unique hues. One can certainly manufacture apparent disagreement among researchers with such a technique. And by accumulating results from many studies that use many (sometimes flawed) methods, one can amass what looks like a huge spectral range for each unique hue. But even S&vB are less than enthusiastic about such artifice, and ultimately return to their own private brand of folk psychology.

S&vB note that there have been a number of attempts to analyze the spectrum into constituent hues. After several paragraphs devoted to this issue, S&vB concede that this issue is "irrelevant" to questions about unique hues. However, their point about the unreliability of casual color naming is well taken. Color phenomenology, although useful as a starting point, must be refined by careful experimentation.

Overall, S&vB (sect. 4.2) give the impression that the methods used for determining unique (elemental) hues are rather haphazard and that researchers proceed simply by "asking people to point out the unique hues." Indeed, one might think from reading S&vB that free naming of rainbows is the current state of the art. In fact, there are now well-controlled methods for assessing elemental and other hues, using monochromatic stimuli, randomly presented from a range of wavelengths, equated for brightness, and of controlled duration, size, and surround field. Experimental procedures are carefully constructed with standardized instructions and structured responses. Observer responses are analyzed with uniform criteria for hue classification.

The most common method for assessing the elementality of hues is that of Sternheim and Boynton (1966). This requires that a color term be both sufficient *and* necessary to describe a stimulus for that color to be declared elemental. This criterion of sufficient and necessary usage requires the observer to use several multiple-term color name sets. Various color term sets are tested by including, then excluding the colors of interest. Colors that are both sufficient and necessary for a given stimulus wavelength are deemed elemental (unique). Colors that fail to be both sufficient and necessary are not elemental (see Wooten & Miller 1997 for a complete description).

The elementality of black, white, blue, green, yellow, and red has been shown across many studies. In other studies, orange, purple, gray, brown, and pink were shown to be nonelemental (see review of studies in Wooten & Miller 1997). When looking across these studies one is struck by the similarities of observer responses. Individual responses, both within and across observers, are quite consistent – not merely some statistical sleight-of-hand involving S&vB's dreaded "average observer."

Having summarily dismissed the above research, S&vB return to the intuitive argument that, "many people are trained to see green in terms of blue and yellow," (sect. 4.2). Though we doubt that anyone *sees* green as a bluish-yellow, almost all of our observers have been taught that green may be made from a mixture of blue and yellow paint. We have labeled this "little bit of knowledge" the "paint bias" (Miller & Wooten 1992). Several empirical observations can be made about the paint bias. First, it seems to be exclusive to green; there is no purple or aqua paint bias. Second, the majority of observers demonstrate that green is elemental *in spite of* the paint bias. Third, perhaps as many as one-third of observers exhibit a paint bias and respond as if green were a redundant (nonelemental) hue. Given the aforementioned facts about green, must we conclude with S&vB that "all arguments against the uniqueness of purple, orange, aqua, and so on fall away"? Or should we conclude that green is a special case where world knowledge may, in some observers, interfere with the Sternheim and Boynton method? To address these methodological problems, Miller (1993) undertook a series of experiments with several methodological improvements of the Sternheim and Boynton hue scaling technique in a paradigm more hospitable to contemporary cognitive psychology.

This series of experiments (detailed in Miller 1997) addressed the paint bias problem while reducing the possible intrusion of language and world knowledge. The hue functions and elementals were quite comparable to earlier studies. By reducing the Sternheim and Boynton task to a single-hue naming method, observer paint bias was nearly eliminated. A second experiment cast the task as a simple forced-choice recognition task, which measured hue recognition and reaction time with six independent color terms. Measured differences in responses to two nonunique terms (orange and chartreuse) validated the general theoretical constructs of elemental hue, nonbasic, and basic color term.

It is always good to ask fundamental questions of an established research paradigm. Many of the issues raised by S&vB were a concern for research in unique hues. Indeed, those issues were the impetus for our methodological evolution. However, I contest S&vB's assertion that the main body of elemental hue research reveals nothing more than an observer's command of English. I believe that the existence of unique hues has been amply established by empirical observation.

Is there no cross-cultural evidence in colour categories of psychological laws, only of cultural rules?

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Abstract: Two points are made on the basis of (mainly) the cross-cultural psychological record. The first is that cross-cultural data indicate at least weak, nontrivial constraints on colour classification. The second is that exceptions to cross-cultural regularities as described by Saunders & van Brakel are compatible with the view that constraints on colour categories are probabilistic rather than deterministic.

Colour has a high perceptual salience. Empirical evidence shows that subjects can readily abstract colour as a "property of objects independent of other properties" (cf. the first hypothesis of Saunders & van Brakel [S&vB]). In cross-cultural studies of object sorting according to colour, form, or function, it has been found repeatedly that there is a clear developmental order with sorting by colour appearing first (cf. Segall et al. 1990).

The minimal condition for nontrivial constraints on colour classification is a nonrandom distribution of colour categories in the space of possible locations. Support for such nonrandomness is found in the work by Rosch Heider (1972a) in New Guinea. Despite problems with the interpretation of local terminology and difficulties of testing local subjects, S&vB agree that Rosch Heider found better recognition and memory for focal than for nonfocal colour chips. Problems with the definition of foci (with or without saturation) and the question of whether Rosch Heider provided the best examples of foci do not negate this result. Experiments by Lucy and Shweder (1979) on recognition memory showed essentially negative results for focality, but these are not very relevant, because colours with equal discriminability rather than colours with equal distance in terms of physical parameters were used. In a sense this amounts to making more difficult what subjects can do well, and less difficult what they can do less well (Berry et al. 1992, p. 142). Perhaps the most pertinent evidence on nonrandomness of colour categories comes from the study by Bornstein et al. (1976) in which dishabituation effects in 4-month-old infants were stronger for a shift in the colour of a stimulus from one colour domain to another than for a shift within the same colour domain when the size of the shifts in wavelength was the same. We conclude that the question posed in the title of the target article is answerable and that the answer is affirmative.

How could S&vB arrive at a question mark? First, they have not distinguished clearly between the nonrandom character of data as such, and the theoretical interpretations that various authors have given to their data. For example, criticisms of Bornstein focus on the theory of colour bands that he used for the selection of stimuli, but it remains unclear how the alleged weaknesses can explain the patterning of the babies' responses. Second, the target article is clear in its critique of the received view, but less so in the formulation of consequences, let alone alternative views. Various perspectives on constraints regarding colour categorization exist. S&vB do not opt for a strictly relativist position that categories are random within the perceptual space. According to their argument, a deterministic view like that of Berlin and Kay (1969) on the evolutionary emergence of colour terms is equally unlikely. These relativist and absolutist views can be taken to define the endpoints of a continuum (Berry et al. 1992).

One need not stop here, however; the available evidence seems compatible with a position of moderate universality that leads to expectations of probabilistic rather than deterministic cross-cultural correspondence. Such a position entails cross-cultural regularities as well as limited local variations in regularities. A lawful relationship will emerge only after data from different populations are aggregated. The evidence on regularities and irregularities in linguistic colour categories seems to fit such a view. Even if there should be fairly strict constraints on the perception of colour categories, this imposes strong constraints on neither the frequency of use of colour for categorization, nor the combination of colour with other modes of categorization (as in the Dutch "appelschimmel" [= apple + mould] for a dapple-grey horse, where form and color are combined), nor the use of other colour terms as so-called basic colour terms or monolexemics. The genesis of colour words is multifaceted and finds an origin in part in sociocultural context.

Although practices, customs, or conventions (in the sense of cultural agreements) may differ, they are not entirely arbitrary across the full range of imaginable alternatives (Munroe & Munroe 1997). An example from another area of perception is the convention in pictorial representation of a horizon on which lines converge when in three-dimensional reality they are parallel. One of the arguments for the conventional character of this depth cue is its absence in many art traditions; even in Western Europe it emerged only during the Renaissance. In addition, this linear perspective leads to demonstrable misrepresentation in some instances (Ten Doesschate 1964). At the same time, it is not an arbitrary convention; it provides for a useful representation of spatial reality in more cases than any other known way of depiction (Berry et al. 1992).

The target article illustrates a well-known phenomenon in cross-cultural psychology. Laws derived from psychology or linguistics may be universally valid. Still, universality does not necessarily imply that the law is expressed in the same way across cultural populations. Cultural manifestations (and hence cultural differences) are often not simple one-to-one mappings of psychological and linguistic laws (Poortinga 1997). Hence the observation of a (sometimes bewildering) variety of cultural rules need not challenge the existence of such laws.

Trichromacy and the neural basis of color discrimination

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Abstract: I take issue with Saunders & van Brakel's claim that neural processes play no interesting role in determining color categorizations. I distinguish an aspect of color categorization, namely, color discrimination, from other aspects. The law of trichromacy describes conditions under which physical properties cannot be discriminated in terms of color. Trichromacy is explained by properties of neural processes.

Saunders & van Brakel (S&vB) claim that neural processes play no interesting role in determining color categorizations. I will take issue with this claim, focusing on only one aspect of what S&vB consider to be color categorization, namely, the color discriminations we can make. I propose to distinguish the question of what color discriminations we can make from the question of what color terms are used in a language, and what symbolic value colors may be given.

In rejecting the notion that neural processes play an interesting role in determining color categorizations, S&vB's arguments target the Opponent Process Theory. According to this theory, qualitative similarity relations among colors that are represented in the phenomenal color space are explained in terms of mixtures and opponencies of unique hues and black and white. These in turn are explained in terms of opponent neural processes.

But the question of our ability to classify colors as being the same or different can also be distinguished from the question of the qualitative similarity relations among colors. By focusing only on neurophysiological explanations of color discriminations, one can, independent of the opponent process theory, argue that neural processes are important in determining color categories.

The argument is as follows. An indefinitely large group of physical properties with little in common independent of their relations to the human visual system are sensed as being the same color. The law of trichromacy describes conditions under which physical properties produce sensations of the same color, where colors are identical if they match on the dimensions of hue, brightness, and saturation. According to trichromacy, a suitable mixture of the intensities of the components of a light composed of three well-chosen wavelengths will match the appearance of a light of any color.

Trichromacy is explained by properties of neural processes. Any two lights that have an identical effect on the three types of cones will be identical in appearance. The light mixture can be made to match the appearance of a light of any color because it can be made to have the same effect on the cones as a light of any color (Cornsweet 1970, pp. 170–72; Teller 1991). Furthermore, aspects of color appearance, such as hue, are explained in terms of aspects of the activation of the cones. Visual sensations of specific hues are associated with specific ratios of activation of the three types of cones (Cornsweet 1970, pp. 243–57).

If what the law of trichromacy states is true and can be explained by neural processes, there are nontrivial neural constraints on color discrimination. S&vB cannot object that the issue of color discrimination is irrelevant to color categorization, but they might hold that focusing exclusively on color discrimination, independent of other aspects of color categorization, relies on having an artificially abstract notion of color.

In section 5.1, S&vB argue that describing identity of color appearance only on the dimensions of hue, saturation, and brightness is artificial. They point to other aspects of color appearance, including glossiness, lustre, and sparkle. They are right that the dimensions of hue, saturation, and brightness do not completely describe color appearance of surfaces. Still, these dimensions are necessary for a description of color appearance. So if neural processes play an important role in determining discriminations by hue, saturation, and brightness, they determine aspects of color categorizations.

S&vB point out that hue, saturation, and brightness are interdependent in some respects (sect. 5.2), but they do not establish that these dimensions of color are interdependent in ways that make it impossible, for example, to explain hue in neural terms independent of saturation and brightness. Consider the Bezold-Brucke effect in which perceived hue changes as luminance – the intensity of light reaching the eye – changes (Cornsweet 1970). S&vB hold that this shows that hue, saturation, and brightness are interdependent. The relation between luminance and perceived hue is distinct from the relation between perceived hue and neural processes, however. Many factors determine the relation between luminance and the activation of the three types of cones. Still, the same ratio of activation of the three types of cones is associated with the identity of perceived hue.

S&vB also claim that cross-cultural studies show that "the *inherent* independence and/or salience of hue (or brightness) does not seem a well-supported conclusion" (sect. 5.3, para. 3). However, in connection with the studies S&vB cite, it is important to distinguish between independence and salience. The salience of hue, where salience is understood as a preferred aspect of colors, may be determined culturally. It is not clear, however, whether such preferences have anything to do with the color discriminations that subjects can make.

In section 4.1, S&vB claim to "illustrate the difficulty of separating what is seen from theoretical presuppositions and prejudices" (para. 4). If the color discriminations we can make are determined by our theoretical presuppositions, a culturally determined preference for hue over brightness may have an important effect on what we see. But S&vB never argue for the very strong claim that our theoretical presuppositions determine the color discriminations we can make.

If one draws some crucial distinctions, it becomes clear that neural processes determine an aspect of color categorization. S&vB may reject the distinctions by claiming that the color discriminations we can make are determined by our theoretical presuppositions. By simply assuming this strong claim, however, they do not genuinely engage theorists who hold that there are nontrivial constraints on color categorization.

The irrelevance of the psychophysical argument

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Abstract: The longevity of the Berlin and Kay theses results from the way in which they were formulated, contrasted with extreme relativism. Saunders & van Brakel need not reject colour opponency to reject universal colour categories. Colour opponency does not manifest itself in language, even when dealing directly with spectral colours.

In section 2.1 of their target article, Saunders & van Brakel (S&vB) give a cursory account of what is the central mystery of the longevity of the Berlin and Kay theses. They conclude from their extensive literature review that those theses are quite “soft”; they accurately note that “most detailed reviews of Berlin and Kay (1969) were critical of their methods of gathering and/or presenting data.” Yet, nearly 30 years later, this work still spawns a large industry of favourable citation, directing (misdirecting) theoretical and practical research. S&vB offer this reasonable account of Berlin and Kay’s sustained popularity: “What we have here is an apparently coherent story of the type that most scientists would like to believe in, but is seriously entertained only because people over charitably assume that the parts in which they are *not* experts are sound” (sect. 1).

I find this a plausible line of argument. I have tended to be less charitable in my own accounts of such assumptions. I have argued that proponents of the Berlin and Kay theses often accept them too quickly for fear of having to accept a specific, if unstated, alternative (Simpson 1991). Berlin and Kay set this historical context. They specifically see their work as a defense against an extreme form of relativism (as they characterize it). This false dichotomy is compelling, particularly to the degree that it is not obvious. That this is a “forced” choice is made more plausible by the fact that their evolutionary thesis has become (in part by their own hands) largely unrecognizable – the once rigid ordering of basic colour terms blown open by a multitude of exceptions.

Even a casual literature review provides a startling number of favourable citations, particularly given the seriousness of both the initial and the ongoing criticisms. Again, two eminent authors have felt compelled to take up the challenge of pointing out the myriad problems with the work started by Berlin and Kay. In many regards, then, their critique is not startling, touching on many familiar domains. Indeed, every author who takes up the challenge of putting to rest the work of Berlin and Kay must engage the same critiques. Many of these are now historic, like the many methodological inadequacies that are set forth in Nancy Hickerson’s (1971) book review, or those set out in numerous articles that have appeared in the intervening years.

The strategy suggested by the above quote is to take a multidisciplinary approach, to appeal to the disparate disciplines, detailing to each the failure of all the other disciplines to support adequately the Berlin and Kay theses. Clearly, S&vB hope to curb all but the most frivolous and dogmatic citations in this manner.

I am particularly interested in S&vB’s critique of the physiological evidence for colour opponency. Whereas the evolutionary thesis has largely fallen apart, the opponent-process theory, it is claimed, gives new life to the universalist thesis. But does it? Two lines of critique are open. S&vB opt for one; they undercut the plausibility, or at least the empirical certainty of, the opponent-process theory. My preference is for the second line; to undercut the supposed support even a fully established opponent-process theory would offer. Separate from being different, this line is, I believe, more sound.

In taking the first line, S&vB fall into the same miasma that most proponents and opponents of the Berlin and Kay thesis do. The assumption is that to establish the validity of the theory of colour opponency is to establish the validity of the theory of basic colour categorization. This is false. As arguments in the rest of S&vB’s target article tend to demonstrate, a contextually situated colour perception is not attuned to spectral (or even spectral plus aperture) colours. The determination of basic colour terms initially undertaken by Berlin and Kay reflects not only an ethnocentric bias, but also a spectral bias. Basic colour terms, as they initially establish the criteria, must refer to spectral or insubstantial colours.

It is possible to grant the theory of colour opponency (Simpson 1991; 1996) without also granting that this would have any significant impact on the development of language in the natural world where the observance of spectra is minimal. The assumed support provided by the opponent-process theory hinges on the psychological salience of the unitary hues. The experience of such hues is rare, and so can hardly be expected to play a significant role in the development of practical languages. In addition, the experience of such unitary hues has failed, historically, to demonstrate consistent psychological salience. Greek and Latin writers from antiquity through Newton describe the rainbow, the most persistent spectral array, as variously comprised of from 2 to over 1,000 bands. Even when spectra are carefully observed there is no robust consensus. This undercuts the claim to both universality of categories and of foci.

As I argue elsewhere (Simpson 1996) it was not until there were technological developments for controlling and measuring spectral colours (of the rainbow and light dispersed through prisms) that a keen interest arose in fixing the categories of the perceived colours. It was not until accurate and repeatable measurements were possible that spectral categories became a useful rule of comparison and classification. In one sense, then, the move is not from concrete occurrences to abstract, but rather from concrete to concrete as the understanding of spectral light and colour changed.

More than possible, it is prudent to grant the theory of colour opponency. The pace at which psychophysiological and neurophysiological research is currently progressing makes it difficult at best to survey accurately the state of the theory. For those trying to write across the disciplines it is distressing but true that research done in the mid- and late-1980s (like most of that cited here) is out of date. Given that there is a practical alternative, rather than stepping into these swiftly moving waters it seems more prudent to follow the second strategy that leaves untouched the particulars of evidence in support of opponency. In this way, S&vB may also win over those readers who want to hold onto the opponent-process theory.

Four-dimensional color space

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Abstract: Multidimensional scaling of subjective color differences has shown that color stimuli are located on a hypersphere in four-dimensional space. The semantic space of color names is isomorphic with perceptual color space. A spherical four-dimensional space revealed in monkeys and fish suggests the primacy of common neuronal basis.

Multidimensional scaling (MDS) of large suprathreshold color differences has shown that color stimuli can be represented in four-dimensional space so that interpoint distances correlate closely with subjective differences. Color points do not fill the space uniformly, but form a spherical surface. The four axes of the color space refer to red-green, blue-yellow, brightness, and darkness neurons (Sokolov & Izmailov 1983). The three angles of the hypersphere correspond to subjective aspects of color perception: hue, lightness, and saturation (Izmailov & Sokolov 1991).

The MDS of matrices of subjective differences between color names in different languages demonstrates that color names are likewise located on a hypersphere in four-dimensional space. Each color term corresponds to a particular area of perceptual color space association after learning of arbitrary symbols with specific colors has shown that these artificial color names are also located on a spherical surface in the four-dimensional space. The locations of color names on the spherical surface were predetermined by colors with which they were associated (Izmailov & Sokolov 1991).

The color spaces in protan and deutan are reduced (Paramei et al. 1991). In protans because of a reduction of the red-green axis, all colors were located along the blue-yellow axis. In deutans, color space is stretched in the yellow-orange region (Paramei 1996). Accordingly, their color names had to be adjusted to modified perceptual spaces. Using the color-naming technique Paramei (1996) has reconstructed perceptual color spaces that differ in normal subjects, protans, and deutans. The perceptual color spaces reconstructed from color names closely match the corresponding color spaces obtained by direct rating of color dissimilarities. The reconstruction of perceptual color space from color naming data strongly suggests that color names follow regularities of color perception (Sokolov 1996).

A four-dimensional perceptual color space has also been revealed by color instrumental conditioning in rhesus monkeys and fish (carp) (Sokolov 1994). Four-dimensional perceptual color space in animals is evidence that color names are not involved in the organization of color space structure in humans. On the contrary, color names follow the organization of color perception.

Making light of keeping color categories in the dark: Some arguments against Saunders and van Brakel's notions of trivial constraints in color nomenclature

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Abstract: Saunders & van Brakel prematurely reject the idea of nontrivial constraints in color nomenclature. Their claim that the universality of color naming is caused by Western contact and cultural dominance is inadequate because of the great variety of terminology systems still found in the world. The complex interactions of hue, brightness, and saturation can be studied rigorously. If we discard the standard models of color nomenclature because of some discrepancies and anomalies, we will not be able to explain the vast array of remaining data that *is* consistent with current theories.

In this target article, Saunders & van Brakel (S&vB), as usual, present interesting challenges to some of the critical assumptions and findings that many color researchers hold sacred (S&vB 1988; 1995). Once again, however, I believe their attacks to be a little premature and overly enthusiastic.

First, S&vB contend in section 1 that the reason the overall standard model of color nomenclature is still accepted is that researchers "over charitably assume that the parts in which they are *not* experts are sound," although the same researchers actually have serious doubts about its validity in the area where they *are* expert. To me, it is disingenuous to claim that serious scientists will hold onto an idea when there is supposedly so little evidence to support it (all Kuhnian paradigm shifts aside). Aside from this, however, the more important implication is that people working on color do not talk to one another to find out just how bad things are. In a multidisciplinary area like color research this is simply not true (e.g., see the philosophers, linguists, anthropologists, and neurophysiologists in Hardin & Maffi [in press].)

S&vB also claim that the reason we see any degree of consistency in the world's color terminology systems is because Euro-American categories have become imposed on non-Western and nonliterate societies. I think there are at least three things wrong with this argument. First, how can we account for the many similarities that modern, ancient, and classical civilizations *do* demonstrate? The Chinese, for example, had basic terms for white, red, black, yellow, and so on, probably thousands of years before the rise of the major European powers. Second, if Westerners did indeed impose their color categories on unsuspecting non-Westerners, is it not true that they could do so only if these new terms were somehow mentally in place in the locals' heads, waiting for an outside stimulus? Clearly, S&vB do not believe this, rejecting the notion of basic color terms lying in some cognitive jungle "awaiting their evolutionary triggering" as they point out in section 2.2. Third, if Western notions of color are being forced on other cultures, why does everyone not have the same 11-term set? Even the most cursory examination of the sparsest of cross-cultural data indicates that people *do* vary in the number and kinds of color categories they use.

It is certainly true, as any field researcher knows, that weird and strange color terms will sometimes arise. Does this mean that the overall standard theory should be thrown out, or should it just be revised? For example, opponent process theory claims that red-green or blue-yellow color categories should not be found. But S&vB argue that evidence seems to indicate that such terms are used in old Sanskrit or old Serbo-Croatian. Even granting the legitimacy of every aberrant case, however, there is still the overwhelming preponderance of data that are consistent and cannot be ignored. The fact is, most color terms given by most informants in most languages generally conform to the standard model. The case is hardly closed, but neither should a mistrial be declared.

S&vB also argue in section 6 that the neurophysiological data fail to support "the autonomy of color." Other factors besides wavelength, such as motion, space, and form also go into the determination of what a color might be. And at the level of cones and such, "there is no evidence for anything that might be called, even metaphorically, a 'colour-coded' cell" (sect. 6.1). That may be, but how might S&vB account for phenomena such as color-blindness, which is color-specific, easily identifiable, and cross-culturally universal? No doubt in the real world color is determined by many factors other than the eye's differential response to hue, brightness, and saturation. I think it unwise, however, to question the whole notion of color autonomy just because the physiological data are so complex and, in many ways, still uninterpretable.

S&vB rightly claim that the complex interactions of hue, brightness, and saturation have often been neglected in color nomenclature theory, as have other dimensions such as luminosity, texture, or dampness. Hue, they say, is usually the primary determinant of what a color is to be labelled. These criticisms are legitimate, but I do not feel they require us to posit the "disintegration of the colour concept" as S&vB suggest in section 6.2. For example, in my work on Japanese colors (1987; 1992; 1997) a dark navy blue term, *kon*, was very highly salient, though a fair number of informants had trouble finding it on the Munsell (Berlin and Kay) array. I eventually discovered that if less saturated colors were used – rather than the maximally saturated ones on the surface of the color chart – informants had little trouble picking this term out, and were quite consistent in their choices. That is, given the right stimulus – in this case, chips that would be "below" the surface of the two-dimensional array of brightness by maximally saturated hue – a clear and interesting interaction of brightness, saturation, and hue could be found. Likewise, in other cultures many external elements do become involved in color naming. Among the Nuer, for example, the patterns of a cow's spots also affect what "color" that cow is to be called. It is also true that many cultures do not conceive of kinship in the same biologically based way that most Western societies do. I do not believe it is useful, however, to completely dismiss notions of kin – and their relations to the society as a whole – any more than I think we get very far by dismissing the concept of color.

Finally, in their rejection of the four main tenets of current color theory, S&vB fail to suggest anything that even comes close to the explanatory power that the current standard models offer. It is true that the Berlin and Kay hypotheses have many internal inconsistencies as I myself have noted (Stanlaw 1997). Opponent process theories also have difficulties (see Boynton 1979 or Thompson 1995, in addition to those mentioned by S&vB). But is dismissing these powerful theories the right answer? And what should we do with the accumulated data that *does* support them? In section 1 they say "To avoid misunderstanding, we emphasize that we do not argue for . . . [the hypothesis that] relativism and unconstrained plasticity should prevail." They go on to say that "the right approach is hermeneutics and/or social constructivism." But what does this mean? No alternatives are ever offered and, thus, it seems that the implication of Saunders & van Brakel's claims is that color terminology *can* vary without linguistic, cultural, physiological, or psychological constraint. Is color naming random or totally arbitrary? I believe that eventually we will find that it is not.

Universal colour perception versus contingent colour naming: A paradox?

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Abstract: Confusion concerning the issue of universality of colour categorization would greatly diminish if context regains its fundamental status in psychological research and we give up on the reductionist notion that biological universality implies behavioral universality.

Although we are largely sympathetic to the four conclusions Saunders & van Brakel (S&vB) arrive at, we disagree with their general message, namely, that there are no nontrivial constraints on colour categorization. Nontrivial constraints on colour categorization must exist. Why? Because, if they do not, it becomes pretty difficult to explain why people within a society talk about and communicate by means of colour so easily. For example, apart from the colour blind, we have a common image about the colour green, and when necessary we use colour names for object identification. Thus, colours cannot get their names through completely arbitrary denotation. This may seem trivial, and surely it is not what the authors meant when they asked if there are nontrivial constraints on colour categorization. Yet, our goal is to show that this introduction illustrates the confusion from which this domain suffers.

1. Colour perception. General agreement exists about the universality of the biological colour-vision system in Homo Sapiens.[1] When linguistic responses are not required, colour (dis)similarity judgments, one way of categorizing colour, show amazing uniformity (Allen 1879; Berlin & Kay 1969; De Valois & Jacobs 1968). Thus, from this perspective, the general claim that colour categorization is not universal appears untenable. Moreover, it undermines the claim that psychophysics and neurophysiology fail to set nontrivial constraints on colour categorization.

2. Colour naming. No agreement exists about the existence of universal basic colour categories. S&vB supply ample, convincing linguistic evidence for the claim that there is no ground for the universality of basic colour terms. Note, colour naming is yet another means available to humans for categorizing colours.

An apparent paradox emerges: colour categorization without the involvement of linguistics (henceforth, perceptual categorization) shows that there are universal features, whereas colour categorization in which linguistics plays a fundamental role (henceforth, colour naming) shows that there are no universal features. The first step out of this inconsistency is to take seriously the *context* in which categorization takes place. In this particular case, an essential contextual constraint is whether or not linguistics is involved in the task. Thus, perceptual categorization is functionally different from colour naming, which is, perhaps unwillingly, shown by the example presented by S&vB in section 2.2. Dani people were worse than Americans in pointing out focal colours shown 30 seconds before in an array of 160 colours. S&vB do not invoke it as a potential explanation, but the difference between the two groups is that the Americans were probably able to use colour names to remember the presented colour chip, whereas the Dani people do not have this linguistic mnemonic at their disposal. This contextual difference is fundamental to the process of colour categorization, but it is not in any respect indicative of the absence of universality in colour perception.

The pervasiveness of the confusion just discussed is succinctly expressed in the following statement by S&vB: "Although not the focus of concern, a central problem in reviewing evidence for the four assumptions is *the relation between language and vision*" (sect. 1, emphasis is ours). If a *central* problem in colour categorization is the relation between language and vision, why is *that* not the focus of their target article? We believe that the paradox expressed in our commentary title disappears, once we accept the fundamental interrelatedness between language (i.e., colour naming) and vision (i.e., colour perception) without assuming a reductionist view. The reductionist approach is expressed in the view that biological universals should imply automatically and isomorphically psychological universals, as stated in the S&vB's first sentence of section 4.3: "If four unique hues were a universal human perceptual grounding, cross-cultural research would confirm it." We do not adhere to this "effect equals structure" assumption, that is, to the idea that the presence of an experimental effect implies the presence of a mental structure. For further discussion of reductionism and the "effect equals structure" assumption, see Lakoff (1987), Putnam (1981), Van Orden et al. (1996), or Van Orden et al. (in press).

Finally, colour naming is functionally different from perceptual categorization, because the fundamental constraints on colour naming are different from those on perceptual categorization. In colour naming, the need to communicate puts major constraints on colour vocabularies. Idiosyncrasies at the cultural level (in effect, people with different languages) and at the level of the social-cultural environment within a language determine the way people divide the spectrum, both in number and type. Examples are provided abundantly by S&vB (see for example, sect. 6.2.) The Xhosa people distinguish among 26 cattle colours; this is probably very useful in their habitat. The colour terms of the people from Arawak correlate strongly with the level of ripeness of their fruit and vegetables, *imoroto* for unripe or green, *koreto* for ripe, red or orange, and *bunaroto* for overripe or brown. (For other examples, see van Kruysbergen & Bosman 1987).[2]

In summary, continuing to study psychological phenomena without providing a fundamental role for context (i.e., a linguistic or nonlinguistic one) gives rise to yet another stalemate (see also Van Orden et al. 1996) in the study of cognitive psychology (i.e., universal colour perception vs. contingent colour naming). Rejecting a reductionist view solves the apparent paradox. Invoking communicational constraints explains the absence of universal colour vocabularies, but does not contradict the biological universality of colour vision.

NOTES

1. This claim is not seriously challenged by the possibility of peripheral regional adaptations as suggested by Bornstein (1973a). He voices the opinion that people living in tropical climates developed a yellow filter (so-called "built-in sunglasses") to reduce the level of ultraviolet light entering their eyes.

2. Trying to separate colour from cattle idiom or to decide whether a word refers to a colour or to an aspect of growth is yet another trap that information-processing theory has set for us. It is a chicken-egg problem for which there is no solution. Assuming interrelatedness (in these examples clear correlations exist) causes the question to be superfluous.

Ekphrasis in colour categorisation: Time for research, or time for revolution?

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Abstract: Saunders and van Brakel propose that we rethink or reject much of current colour theory. Many of the problems they cite appear to call for further research rather than a radical rethinking of colour theory. The controversy described in this target article appears to be itself a case of "ekphrasis," or something that does not exist.

In their interesting and controversial target article, Saunders & van Brakel (S&vB) put forward the thesis that much of the current wisdom about colour vision and colour categorisation is either wrong or built on very shaky foundations. They imply that we need to rethink much of current theory, or cast it away and start again. Despite the large amount of evidence marshalled to illustrate their points, much of what S&vB imply are fatal problems with current theories are either well known and being actively investigated in the hope of improving, not discounting the theories, or seem to be the result of methodological rather than substantive theoretical or evidential factors.

In their section on the evidence for the universality of colour experience (sect. 2), S&vB restate the well-known problems with Berlin and Kay's (1969) original work. When criticising this work, however, S&vB often seem to go further than the evidence suggests. For example, in considering perceptual universals, the methodological problems of Rosch's work with the Dani are given as evidence not of poor communication between the experimenter and participant, but of undermining "the universal salience of both focality and prototypicality" (sect. 2.2, para. 5). Developmental psychology in particular has recently needed to rethink findings based on misunderstandings between participants and experimenters, or "actors and observers" (Siegal & Peterson 1994, p. 427) and has tried to reappraise previous work in this light. For example, the problems described in the Berlin and Berlin (1975) study appear very similar to those encountered in studies with participants who do not understand experimental instructions (for developmental, linguistic, cultural, intellectual or situational reasons).

S&vB point out a number of problems caused by taking a simplistic view of basic opponency theory. However, much of the evidence cited is consistent with a maturing theory, rather than one approaching an imminent demise. For example, although Mervis and Roth (1980) do indeed show how Kay and McDaniel's (1978) fuzzy sets cannot differentiate basic from nonbasic colour categories, it could be that an improved methodology using both fuzzy sets and reaction times to basic and nonbasic colours will be able to distinguish reliably between them. A further example is the apparent claim that opponency cannot work, because researchers do not agree on the exact weightings of cone contributions to the individual opponency channels. Surely this is a call for new, refined research rather than a new theory.

Work in our laboratory at Portsmouth into "colour nameability," which combines the observed effects of the consistency of free colour naming, naming confidence, and reaction times has shown repeatedly that an opponent relationship exists between colour names for coloured patches, and that such colour nameability is predicted by opponency appearance models such as Hunt's (1991) model (Guest & Van Laar 1995). This model does not depend on experimentally flawed expectations of predefined colour categories or any underlying theories of opponency, but is derived directly from the 32,000 data points collected.

The section regarding hue, brightness, and saturation appears confused. For example, lightness and brightness are often confounded and used as synonyms. Saturation is also confused with chroma and colourfulness. For example: "It is generally assumed that colour has three independent psychological dimensions: hue, brightness, and saturation (Munsell's *hue, value,* and *chroma.*)" (sect. 5.1, para. 1). Value is a measure of lightness, not brightness, and chroma is a measure of the combined effects of lightness and saturation, not just saturation (Hunt 1991). With this in mind, most of the problems voiced in this section are answered.

S&vB's conclusions are couched in less controversial terms than most of the rest of their text, and there is much to agree with here. That neither neurophysiology nor psychophysiology confirm that there are exactly two opponent hues or three pairs of opponent colours is not surprising; research is still ongoing, but the balance of the available evidence seems to support this theory rather than any other combination. It is not a new finding that many cultures, when examined by current research methods, do not show 4 primitive hues or 11 basic colour categories. However, this is by far the general rule for most Stage VII languages. Hue, brightness, and saturation are well known not to be independent, and there are also well understood links between brightness and luminance (Hunt 1991; Yaguchi et al. 1993).

In conclusion, there does indeed appear to be a sense of "ekphrasis" (sect. 1, para. 1) or something that does not exist about this debate, but not in S&vB's sense of lack of agreement about generally held theories, but rather in their idea of controversy where none exists. The scientific method is used to increment knowledge through the dialectic process of theory and antitheory, the fitter theory at any point being the one that best accounts for the most evidence at any given time. Although many theories are known to be flawed, they are generally only rejected when a better theory with more explanatory power is offered in its place. In this target article, Saunders & van Brakel appear to criticise and discredit theories that may be flawed, but they fail in their scientific endeavour when they do not propose a better alternative.

Hue opponency: A constraint on colour categorization known from experience and experiment

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Abstract: The terms red, green, yellow, and blue are both necessary and sufficient to describe our chromatic experience. Their uniqueness and opponent nature is supported by evidence obtained under suprathreshold conditions, especially hue cancellation. These constraints are nontrivial. How some electrophysiologically identified mechanisms contribute to colour appearance is not known, but their complexities do not refute our experience of elemental hues.

True, if one examines the hues of the spectrum, one could easily divide them into the seven categories of Newton, or some arbitrarily larger number. This exercise places no constraints on colour categorization. More interesting and informative is to ask not how *many* terms one might use to categorize the hues of the spectrum, but how *few* terms are required for a complete account of colour appearance. There is a substantial body of evidence to support Hering's (1920b) contention that only four hue terms – red, green, yellow, and blue – are necessary *and* sufficient to describe colours. Experiments have shown that terms such as orange and purple are not necessary, but can be reduced to yellowish-reds and reddish-blues, respectively, whereas red, green, yellow, and blue cannot be reduced to any other hues. This experience implies powerful constraints on how we categorize colours and how the nervous system codes them.

Perhaps more critical is that some combinations of hues, although they are logically possible, do not exist in our own experience. We do not experience reddish-greens and bluish-yellows. These pairs are virtually never used in combination to describe hues in colour-naming experiments. In addition, when red and green (or blue and yellow) lights are combined additively, they cancel each other. For this reason, Hering referred to them as opponent sensations. That these hues are mutually exclusive in our experience provides powerful evidence of internal constraints on our colour categorization. Saunders & van Brakel (S&vB) assert that this is not necessarily correct. Although they reject virtually all colour-naming evidence as supporting the existence of constraints, they embrace colour-naming evidence to the contrary, citing Crane and Piantanida's (1983) suggestion that one can experience reddish-greens. It should be noted, however, that the conditions of that experiment, though more unnatural than any of the experiments that S&vB dismiss as unnatural, produce unstable percepts, more accurately described as percepts varying spatially or temporally *between* red and green. Anyone who doubts this should try to buy an item of clothing, requesting that the clerk provide a "solid" reddish-green or yellowish-blue. It will cause some confusion because the concept is inconsistent with normal experience. Even those skeptics who claim they can imagine such a hue would have to admit that it is not at all as natural as imagining a reddish-yellow that we would normally call orange.

That yellow and blue, like red and green, are linked in our neural network by an opponent code is shown by still other evidence that we know from experience and experiment. These paired colours covary with changes in intensity (the Bezold-Brücke hue shift) and over space and time. Yellowness in one part of the visual field induces its opponent colour, blue, in neighboring regions of the visual field as demonstrated by Goethe (1810). Similarly, exposure to a bright field of one hue (e.g., red), induces a nearly complementary colour in the after-image (e.g., green). The induced hue in this and some related situations is not perfectly complementary because of adaptation effects prior to the opponent stage. As S&vB correctly note, contrast effects are not limited to red and green, and blue and yellow; an infinite number of complementary colours can be induced in this way. This experience in no way negates Hering's opponent-colour theory, as this is explained by dual activation and induction involving two opponent channels concurrently. These phenomena can be experienced using simple devices to show that our perception under natural conditions of viewing is powerfully constrained by the pairing of these hues.

S&vB suggest that the evidence does not support the view that the elemental colours are those of Hering. No evidence, they believe, eliminates the possibility that, instead, purple and orange (or some other arbitrarily chosen hues) are elementary. Indeed, there are pairs of lights that would cancel each other, but not for every member of the related set (such as all oranges, or all purples, etc.). In contrast, any yellow can cancel any blue, and any red can cancel any green, as would be expected if these were the elemental hues. Moreover, the effectiveness of purple, orange, or other colours, if used as cancelling stimuli would be predictable from Hering's unique hue components in the light mixture. In fact, contrary to assertions by S&vB, it is not necessary to measure hue-cancellation functions using unique hues. Spectrally unique hues are generally used for blue, green, and yellow cancelling lights, but a yellowish-red (670 nm) typically has been found more convenient than a nonspectral mixture that would be required to use a unique red cancelling stimulus.

Recent physiological studies show that many cells in the visual pathway coding for colour also multiplex signals coding for other perceptual attributes such as contrast, size, and velocity. As a result, some aspects of hue depend on the spatial and temporal parameters of the stimulus, as Hering recognized. Physiological evidence further shows that the physiological coding through various stages (at least those studied so far) is clearly not isomorphic with Hering's hue channels. Although this is important to know, it cannot in any way invalidate what we know from our own experience. In other words, our experience can tell us what the neural network is accomplishing and it does not matter how it is carried out or whether it can ever be verified by monitoring the activity of a subset of the network units.

Just as physiological evidence is irrelevant in this context, so, too, is the fact that the opponent channels are nonlinear under some conditions. Along the same lines, it is not clear why the variability in the wavelength of the unique hues or the neutral points of single cells is seen as troublesome for the Hering view. Why should variability in this context be lacking when it occurs in every other aspect of biological structure and function? In short, virtually all of the psychophysical and physiological evidence cited by S&vB is irrelevant and obfuscates the central issue of whether there are constraints on the way in which we perceive and categorize hue. The evidence they take so much stock in shows only that the mappings between the physics of light, the physiological code, and experience are complicated, not that the system is doing something other than what we know it does from our own experience.

Color categories and biology: Considerations from molecular genetics, neurobiology, and evolutionary theory

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Abstract: Evidence from molecular genetics bolsters the claim that color is not a perceptuolinguistic and behavioral universal. Neurobiology continues to fill in many details about the flow of color information from photon reception to central processing in the brain. Humans have the most acute color vision in the biosphere because of natural selection and adaptation, not coincidence.

It is unfortunate that Saunders & van Brakel (S&vB) fail to consider any relevant data from the field of molecular genetics in their treatment of nontrivial biological constraints on color categorization. Such information would actually bolster their attempt to refute the hypothesis that color is a perceptuolinguistic and behavioral universal. It now appears that the genetic apparatus underlying normal human color vision is not truly universal, contrary to what was generally believed prior to 1992 (Durham 1991). Indeed, instead of passing over the problem of polymorphism as S&vB explicitly choose to do in section 3.3, even an elementary treatment of the evolutionary history and extant genetic variation of our photoreceptor system leads to the conclusion that some people who are not in any way color-blind may actually differ in their ability to perceive color. Our highly specialized photopigment system, which we share only with other Catarrhine primates such as apes and Old World monkeys, evolved to its present normally trichromic state more recently than the Catarrhine-New World monkey split some 35–40 million years ago. It is equally clear that this system continues to evolve in contemporary human populations (Zegura 1995; 1997).

At a fundamental biological level human color vision represents the neurologically filtered phenotypic manifestation of a multigene family that codes for a set of evolutionarily related retinal cell photopigments. These 7-transmembrane receptor apoproteins are known as opsins. Humans possess four different opsins: rhodopsin in rod cells of the retina and the three different retinal cone cell opsins responsible for color vision (i.e., the short wavelength-sensitive or "blue" opsin, the middle wavelength-sensitive or "green" opsin, and the long wavelength-sensitive or "red" opsin). The genes for our "red" and "green" opsins lie next to each other near the end of the long arm of the X chromosome (Xq28), our "blue" opsin gene is located on chromosome #7 (7q22 → qterm), and the rhodopsin gene is on chromosome #3 (3q21 → qterm).

The ancestral human color pigment gene probably diverged from the rhodopsin ancestral gene about 800 million years ago, presumably coincident with the first appearance of cone cells (Yokoyama & Yokoyama 1989). The "blue" versus a joint "red/green" opsin gene divergence occurred in our vertebrate ancestors about 200 million years ago, and the gene duplication event that produced separate "red" and "green" opsin loci on the X chromosome occurred in an early (perhaps Oligocene) Catarrhine primate. New World monkeys have only one pigment gene on the X chromosome (Jacobs 1994/1995); all normal-sighted Catarrhines have at least two (i.e., one "red" opsin gene and one "green" opsin gene). Normal-sighted humans have been reported with from one to four copies of the "red" opsin gene followed by from one to seven copies of the "green" opsin gene (Neitz & Neitz 1995). Both "red" opsin gene polymorphism in heterozygous females and the possible existence of multiple functional "red" opsin genes in males have led to the same conclusion: some humans are tetrachromic rather than trichromic because they have four functional pigment types and can experience an extra dimension of hue in the red portion of the color spectrum (Merbs & Nathans 1992; Mollon 1992b; Neitz & Neitz 1995; Winderickx et al. 1992; Zegura 1995).

All known organismal visual systems operate by coupling an opsin to a vitamin A-derived chromophore, which is the actual photon-absorbing molecule. The functions of the opsin are to fine-tune light absorbance by the associated chromophore (cis-retinal in humans) and to initiate a G-protein-signalling cascade that eventually results in the hyperpolarization of the retinal cell membrane and subsequent nerve impulse conduction. It is the photon-induced transformation of cis-retinal to trans-retinal that triggers a shape change in the associated opsin molecule that in turn activates the G-protein, transducin. Genetically encoded differences in opsin gene structure can result in amino acid substitutions that provide a different chemical environment for cis-retinal leading to shifted absorbance curves with differing absorbance maxima. Indeed, functionally important shifted human spectral absorbance curves are evident for various kinds of color-blindness as well as for cases of phenotypically normal individuals with variant opsin genes (Jacobs 1994/1995; Merbs & Nathans 1992). As Mollon (1992b) stresses, these people actually exist in a different phenomenal world!

On a completely different front, S&vB appear to be pessimistic about the efficacy of neurobiological research for identifying an autonomous color pathway and its various components. Recent technological advances have made the eventual discovery of the details of the pertinent brain circuitry eminently possible (if such a distinct pathway does indeed exist). Both functional magnetic resonance imaging (fMRI) and positron emission tomography (PET) measure local hemodynamic changes correlated with experimentally defined cognitive tasks (Ungerleider 1995). Color-associated perception yields activity patterns that are remarkably consistent across studies (Corbetta et al. 1995; Maunsell 1995; Ungerleider 1995). Somehow the visual cortex is able to create an edited representation of the visual world that is dynamically modified according to the goals of the viewer (Maunsell 1995), thus blurring the commonly invoked distinction between perception and cognition. Information about color flows from the retinal photoreceptors via a series of well-substantiated intermediate pathways to area V4 of the Prestriate Cortex where specific visual processing of color takes place.

More and more experimentally derived details about this information flow and subsequent cortical activity are gradually becoming known in humans as well as in other closely related primates. For example, a unique anatomical type of retinal ganglion cell coding for blue-yellow opponency has recently been conclusively described (Masland 1996), although the red-green analog still eludes detection. Also, Martin et al. (1995) used changes in regional cerebral blood flow measured by PET to discover an intriguing spatial relationship between color terms and color perception. Generation of color words selectively activated a region in the ventral temporal lobe just anterior to the area involved in color perception. These and other data in their paper led to the suggestion that "object knowledge is organized as a distributed system in which the attributes of an object are stored close to the regions of the cortex that mediate perception of those attributes" (Martin et al. 1995, p. 102).

I am just as convinced that linking propositions among theories of language, vision, and biology can form a unified framework for understanding the present and future limits of human perceptuolinguistic and behavioral competencies in the realm of color information processing as S&vB are about their "ekphrasis" analogy. It would be ironic if Sereno's provocative conjecture that human linguistic capabilities evolved as an exaptation (*sensu* Gould & Vrba 1982), wherein a relatively minor rewiring of the visual system led to a dramatically new functional realm, turns out to be correct (Gutin 1996). According to Sereno, we use our visual areas as our primary means of processing language precisely because they are what we use to make sense of our surroundings (Gutin 1996). In this view, color terms themselves ultimately represent an evolutionary outcome of changes in the visual system brought about by natural selection.

Color vision is vitally important for mate selection, food acquisition, recognition of predators or prey, communication, and extraction of information about the environment in taxonomically diverse organisms (Zegura 1997). As a consequence, color vision is widely construed to be a biological adaptation for the members of these species, including *Homo sapiens.* Might Berlin and Kay's (1969) basic color terms represent phenotypic attributes that are both biological and cultural adaptations involving highly efficient information extraction from the environment? Hull (1988, p. 26) underscores the import of knowledge acquisition for humans as follows: "Success in the knowledge game is hardly an incidental feature of *Homo sapiens.* It is our chief adaptation. It is the only thing in the struggle for existence that we do better than any other species." Rather than using philosophical discourse to answer Saunders & van Brakel's query: "But why pick up *colour?*" (sect. 6.1), the basic principles of evolutionary biology lead me to think it is hardly a coincidence that we also have the most acute color vision in the biosphere (Jacobs 1994/1995)!

Authors' Response

Colour: An exosomatic organ?

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Abstract: Sections R1 to R3 attempt to take the sting out of hostile commentaries. Sections R4 to R5 engage Berlin and Kay and the World Color Survey to correct the record. Section R6 begins the formulation of a new theory of colour as an engineering project with a technological developmental trajectory. It is recommended that the colour space be abandoned.

R1. Introduction. We are grateful to all our commentators for the opportunity to correct misunderstandings (sects. R2

Table R1. *Classification of types of communications keyed to sections of response (see also Table R2)*

Commentators [section numbers]	Type of commentary
Dubois [6], **Gellatly** [2,6], **van Kruysbergen et al.** [3,5], **Simpson** [6]	Broadly in agreement (with adjustments or further interpretation)
Abramov & Gordon [3], **Braisby & Franks** [3], **Brill** [6], **Mausfeld** [6], **Costall** [3]	Broadly in agreement, but criticize style, relevance, purpose, or orientation
Bornstein [2,3], **Broackes** [2,3,5], **Byrne & Hilbert** [2,3], **MacLaury** [5], **Miller** [2,3], **Ingling** [2,3,4], **Werner & Bieber** [2,3]	Defend existence of four unique hues
Kay & Berlin [4,5], **Dedrick** [3,5], **Foss** [4,5], **McManus** [3], **Stanlaw** [2,5]	Defend theory of *Basic Color Terms*
Poortinga & Van de Vijver [5,6], **Ross** [2,5], **Van Laar** [3,5], **Zegura** [2]	Defend constraints on colour categorisation of some sort
Davies [4], **Davidoff & Roberson** [5,6], **Hardin** [3], **Hubey** [2,3], **Jameson** [3], **Sokolov** [2,3]	Target on details, fail to address content of target article

and R3 of Response), to specify further problems with *Basic Color Terms* and the *World Color Survey* (sects. R4 and R5), and to gesture towards new directions (sect. R6).[1]

Table R1 keys sections of the Authors' Response to the main theme of each commentary. Table R2 lists issues that are not discussed further. Where there is modest consensus nothing more is said. This applies both to conclusions 4 (**Broackes; Dedrick; Ingling; Jameson; Mausfeld; Werner & Bieber**) and 3 of the target article (sect. 7) (**Dedrick; Mausfeld; Van Laar**) – but see Tables R4 and R5 on **Jameson** and **Hardin.**

R2. Misunderstandings. Due to compression, some agreement is hidden by misunderstanding or misconstrual. Although the goal of the target article is to dislodge four specific hypotheses (concerning two opponent channels/pathways, unique hues, hue-saturation-brightness, and the autonomy of colour), it is read as an attack on colour science (**Abramov & Gordon; Dedrick; Hubey; Jameson**). Only if these hypotheses are *essential* to colour science, however, is the inference correct. Jameson takes the target article to imply "the enterprise has made no progress." But the question is: What is *the enterprise?* When neurophysiological grounding for Hering's *Urfarben* is denied by well-known colour scientists, as Jameson acknowledges, that is progress. The utility of colour science for tightly defined practical, but parochial purposes is not denied (contra **Byrne & Hilbert; Hardin;** Jameson). Practical success may suggest "psychological naturalness" (Jameson), but to draw the conclusion that it *is* natural, is a fallacy. (See how **Brill;** Hunt 1991, p. 213; Judd & Wyszecki 1975, pp. 274, 327; Mollon 1989b; and Wyszecki & Stiles 1982, pp. 582–86 refer to and defend their models.)

The conflation of *colour* vision, perception, sensation, categorisation, and naming with *discriminatory* capacities or the *spectrum* – distributions of spectra, wavelength-coded cells – is common (**Bornstein; Braisby & Franks; Byrne & Hilbert; Davidoff & Roberson; Hubey; Ross; Stanlaw; van Kruysbergen et al.**). Thus, contra **Ross,** it is neither assumed that theoretical presuppositions determine colour discrimination, nor denied that the law of trichromacy is useful to describe the phenomenon of metamers. Stanlaw suggests that the cross-cultural occurrence of colour blindness makes a plausible case for colour-coded cells. That's fine if taken only to mean: things like eyes, brains, and bodies have to be in order. **Zegura** suggests that humans have acute colour vision, but whether or not that is so seems irrelevant to what is at issue. Dogs may have the most acute olfactory system in the biosphere, but that sets no interesting constraints on their "olfactory categories." **Ingling** writes, "Starve the observers," but this misses the point. It is those colour scientists who accept the four hypotheses who are the Whorfians (contra Davidoff & Roberson; **Davies; Foss;** Ingling; **Kay & Berlin;** and **McManus).**

Unfortunately, the phrase "all arguments against the uniqueness of purple, orange, aqua, and so on fall away (sect. 4.2)" conveys the wrong impression to **Broackes** and **Miller.** The target article reads, "If green is an *Urfarbe* 'intuitively' then all arguments against the uniqueness of purple, orange, aqua, and so on fall away." The sentence after "then" is conditional. It is not said that purple has an equal claim to uniqueness. If someone claims green is an *Urfarbe* "intuitively," someone else may claim orange too is an *Urfarbe* "intuitively." Whether this or that is or is not a better or an equally plausible candidate for unique hue status is beside the point.

Byrne & Hilbert are unsure whether or not the CIE definition is accepted in the target article. The confusion arises because arguments are offered both against those who adopt it and against those who appeal to "experience" (see next section). The target article presupposes that all colour scientists accept the CIE definition; see, for example, **Werner & Bieber,** Gordon et al. (1994, p. 29), Hunt (1991, p. 146), Kuehni (1983, p. 39), Lennie and D'Zmura (1988, p. 337), Pokorny et al. (1991, p. 44), and Wyszecki and Stiles (1982, p. 487).

The target article did not evade accurate, precise discussion of colour by omitting mathematical and physical discussions of "it" (**Hubey; Sokolov**). The concern is with the referent of "it." Hubey seems to change the subject, leaving vague what constraints the mathematical approach provides. If received wisdom were to declare cyan/orange an opponent pair of unique hues – as an indisputable fact in the sense of **Ingling** – then Hubey could no doubt come up with an algebra to fit it.

Several commentators complain that the target article is insufficiently critical about the literature. It is not self-evident, however, why Sternheim and Boynton (1966) are absolutely reliable, whereas Crane and Piantanida (1983)

Table R2. *Issues not further discussed due to marginality (*) scope (&), or conflicting assessments (?)*

Issue	Commentators	Type	Brief comment or references
"True" structure of colour space	**Abramov & Gordon; Brill; Broakes; Hubey; Jameson; Mausfield; Sokolov**	?, &	Red herring, except for practical needs; cf. section R6
Evolutionary aspects	**Zegura**	&	Requires extensive discussion of neo-Darwinism
Natural endowment	**Broakes**	&	Requires extensive discussion of "natural"
Language–vision–cognition	**van Kruysbergen et al.; Zegura**	&	Scope too wide for target article or reply
Rosch's work with the Dani	**Byrne & Hilbert; Davidoff & Roberson; Dedrick; Poortinga & Van de Vijver; Van Laar**	?, &	Dubois & Resche-Rigon 1995; Ratner 1989; Saunders 1995
Bezold–Brücke effect	**Abramov & Gordon; Dedrick; Hardin; Ross; Werner & Bieber**	&	Vos 1986
Attributive/referential uses	**Braisby & Franks**	&	Saunders 1992; 1995
What things are in themselves	**Foss**	&	Requires extensive discussion of eliminativism
Fuzzy sets/concepts	**Abramov & Gordon; Braisby & Franks; Dedrick; Van Laar**	?, *	Relevance not sufficiently explained
Right approach is hermeneutics	**Stanlaw**	*	Misunderstanding due to change of lay-out by press
Goethe	**Costall; Ingling; Werner & Bieber**	?, *	Judd 1970; Gage 1993; Sepper 1988
BCTs in Chinese	**McManus; Stanlaw**	*	Baxter 1983; Beffa 1978; Gernet 1957; van Strauss 1879
Japanese "kon"	**Stanlaw**	*	Uchikawa & Boynton 1987; von Wattenwyl & Zollinger 1979

are not. No argument in the target article depends on one source alone. This also applies to Crane and Piantanida (1983) (contra **Werner & Bieber,** the target article does not cite Crane and Piantanida's "suggestion that one can experience reddish-greens" – only that their results "suggest that whether or not this is so is an empirical matter"). As **Byrne & Hilbert** point out correctly, Teller (1984) has shown why the opponent theory is not committed to the assumption that the visual system is opponent-coded at all levels. That is to say: The possibility of an experience of reddish-green – whatever that might mean in ordinary English – is an empirical issue (the point made in the target article).

Bornstein writes that we deny findings in infant psychology with sweeping, uninformed generalizations. The latter, however, were based on Bornstein's work (Bornstein 1985; Teller & Bornstein 1987). Bornstein (and also **Poortinga & Van de Vijver** and **Dedrick**) seem to discount the possibility that patterns of infant response could be an artefact of forced discrimination (cf. **Gellatly**'s astute criticism of Bornstein). To suggest that the only inference from the data is to four unique hues is to beg the question as Bornstein does in saying that "the bee sees ultraviolet but not red like human beings" (cf. **Zegura**'s "some humans are tetrachromatic"). Jacobs's (1992, p. 41) comment about identifying species with tetrachromat, pentachromat, and so on, is relevant here: "with nonhuman subjects the outcome often has multiple interpretations." The same applies to 4-month-old infants.

R3. Sloppiness. The word "sloppiness" is contested by **McManus** in connection with Berlin and Kay (1969). No commentator disputes the evidence for this label. Concerning colour preference, McManus objects that the discussion is based on secondary literature, although we provided five original references, three of which postdate McManus et al. (1981). **Bornstein** too is concerned about our "misrepresentation" of the infancy literature on preference. Neither commentator, however, indicates what is wrong with the sources or critical discussion in the target article. McManus indicates that he has reported data on the frequency of colour words in Chinese poetry, which would lend support to Berlin and Kay's (1969) order of evolutionary antiquity. Table 1 and Figure 1 of McManus's (1983) data are labelled "Chinese poetry" (Chou & Chen 1935). However, Chou & Chen (1935) say nothing about Chinese poetry, providing data only on colour preferences of Chinese students.

Jameson seems to misrepresent the work of Indow (see Table R3). The empirical data seem to point to an obvious conclusion: "Achieving a metric colour space" only makes sense for a limited range of colour differences – and only to a degree of approximation (see Table R3B; cf. **Brill**). The commentary of **Sokolov** should be read in this light. The Jameson/Indow "sweet spot" – saturations /4–/6; lightness 3/–8/ – is what in American English folk terminology is meant by colour, which again begs the question (see also Saunders 1992, pp. 11–12). In this context it should be asked what the *theoretical* relevance is of working from the assumption that colour "is" a/the colour space (see sect. 6). Similarly, it is difficult to assess Hardin's commentary, in

Table R3. **Jameson**'s *suggestion that Indow (1988) is ignored (A);* **Jameson**'s *selective reading of Indow (B)*

A. Jameson: "There is a body of similarity scaling research, ignored by S&vB, indicating that hue, saturation, and brightness . . . are real psychological constructs. . . . Research by Indow and colleagues (cited in Indow 1988) shows that . . . (Munsell colors) . . . can . . . be embedded in a 3-D manifold with locally Euclidian metric."

Target Article (sect. 5.1): "Work on the OSA system has shown it impossible to represent colour in Euclidian space (Man & MacAdam 1989; Nickerson 1981). None of the existing systems of colour classification achieves the goal of uniform perceptual intervals between any two adjacent colours (Derefeldt 1991; Indow 1988)."

B. *Indow 1988,* pp. 466, 468: "what has really been shown in our Studies . . . is that the whole Munsell space can be covered by overlapping Euclidian subspaces. Roughly speaking, the whole Munsell space may be something like a manifold Euclidian connection (. . . , or a Riemannian space with the global cylindrical co-ordinates.) . . . If representation by other than Riemannian metrics is sought for, the simplest case will be a power metric of Minkowski."

Table R4. **Hardin**'s *reading of Burns and Shepp (1988)*

Hardin: "They [S&vB] cite Burns and Shepp (1988), who provide good evidence that the three variables are not psychologically independent and then ask whether or not it makes sense to base a metric on them. . . . But S&vB make no use of these important insights, being content to say that Burns and Shepp "review evidence that physical attributes of colour do not independently affect the psychological dimensions."

Target article (sect. 5.1): "Burns and Shepp (1988) argue that there are serious problems about a three-dimensional spatial metric as the proper psychological dimension of colour vision. They review evidence that physical attributes of colour do not independently affect the psychological dimensions."

Burns** & **Shepp 1988, pp. 494, 505: "There is a serious problem with the use of spatial metrics as a converging operation for the characterization of stimulus structure. . . . Clearly, if the underlying assumptions of psychological space are violated, the adherence to a metric as part of an operational definition for stimulus integrality is unfounded. . . . There is considerable evidence that the physical attributes of color do not independently affect the psychological dimensions of color."

Hardin: "Burns and Shepp remark that the three variables 'structure and order perception in a consistent way,' so theirs is not a critique of the use of three variables in standard color-order systems."

Burns** & **Shepp 1988, p. 494: "The term [dimension] has also been used to mean an organizing principle that structures and orders perceptions in a consistent way." [What Burns and Shepp say is that the term "dimension" has been used in a particular sense. They neither endorse that sense nor draw the conclusion Hardin does from it.]

particular because of his reading of Burns and Shepp (1988); see Table R4.

It is unclear why **Hubey** finds it unsurprising that the nodal colours of his simple 3D space are those that occur earliest in cultural colour naming systems, as noted by Berlin and Kay (1969). Cyan and magenta, two of Hubey's nodes, do not occur among the Basic Color Terms (BCTs), whereas the BCT "pink" isn't even on an edge between two nodes. (On the theory of BCTs, see the next section.)

Bornstein; Ingling; Miller; and **Werner & Bieber** repeat the familiar description of unique hues. Disappointingly, these commentaries add nothing new. The most important taken-for-granted assumption is the reliability of "observers" (often students or family members of professors at American universities) taken as representative of the human species. It is noteworthy that notwithstanding the passion with which the received view is defended, different commentators appeal to different kinds of evidence. **Byrne & Hilbert,** citing Boynton (1979), appeal to "subjective experience"; Werner & Bieber to "what we know from our own experience" – an appeal not much different from Mach's (1919/1900, pp. 55f). Bornstein appeals to the "qualitative appearance of the chromatic spectrum," Ingling to "objective transition points in the spectrum," and Miller to "careful experimentation." Evidence that is neither allowed nor relevant is "introspective musing" (Ingling) or "phenomenal self-evidence" (Byrne & Hilbert). The so-called self-evidence of unique hues may be compared with **Braisby & Franks's** "a red is a kind of brown." All in all, it remains unclear what the objective evidence is for the universal truth of four unique hues.

Abramov & Gordon say they accept four canonical fuzzy sets of red, yellow, green, and blue, which they derive from Kay and McDaniel (1978) – cf. Abramov and Gordon (1994) and Gordon et al. (1994). They offer as justification the standard arguments for four unique hues found in Hurvich (1981), but fail to address our criticism of Kay and McDaniel and the discussion in section 4 of the target article. Abramov and Gordon also do not seem to have noted that in the target article the term "(neural) pathway" is used for what they call channels – the term "channel" being reserved for *psychophysical* channels. Their term "hue mechanisms" corresponds to our colour-coded cells," thereby invalidating their criticism. Contra Abramov & Gordon, it is not claimed that "the responses of all these neurons" are epiphenomena. The claim that such neuron responses fail to support the four hypotheses. Finally, they make the unwarranted assertion that in order for the signalling of species-important objects to work, a limited set of colour categories is required.

Dedrick's plea for a "space between the perceptual and the linguistic" is a good suggestion, but marred by the uncritical reiteration of views associated with Berlin and Kay, Rosch, and Bornstein, and a failure to address criticisms in the target article. He calls his conclusion a "minimal toehold," but it remains unclear whether he is thinking of BCTs, Rosch-type prototypical colours *and* exactly four hue categories, *or* "some colours" and "some colour categories" with which he starts his commentary.

Many commentators refer to recent and unpublished literature. For example, **Miller** suggests that the target article fails to cite methodological improvements. The reference appears to be to Miller's dissertation and to his unpublished work (Miller 1997; Wooten & Miller 1997).

Table R5. *Putative neglect of the World Color Survey in the target article; bold type added*

Broakes: "We might look to the World Color survey, that huge investigation from which preliminary reports are now appearing"

Foss: "To Berlin and Kay's credit, they and their followers have labored for improvements. . . . the ongoing World Color Survey"

McManus: "Of course more extensive data would be nice, and it is therefore strange to find S&vB not mentioning the World Color Survey (WCS) of Kay et al. (1991)."

Target article (sect. 4.3)

"Of the Munsell colour chips commonly used in cross-cultural experiments, 60–80% often remain unnamed (see Berlin et al. 1991). . . . One informant, when asked to show where all the red chips were, took the pen and carefully circled the entire board. Similar examples can be found in Berlin et al. (1991), . . . The common solution to such problems is to conclude that lack of abstract colour categories is the result of evolutionary backwardness (see comments of field workers in Berlin et al. 1991). . . . Dark blue and black are also frequently considered a unified entity (see Almquist (1883); Berlin et al. (1991), . . . In many languages blues and greens are mapped together under one BCT (see Berlin et al. 1991)"

Target article, reference list

Berlin, B., Kay, P. & Merrifield, W. R. (1991) *The world color survey*. Photocopied data available from the Summer Institute of Linguistics, Dallas, TX.

However, the method for assessing elementalness of hues, Miller indicates, is still that of Sternheim and Boynton (1966). (Miller says he has improved the technique; the problem with unique green is solved by giving it a name: the "paint-bias.") Miller adds that individual responses, both within and across observers, are quite consistent and **Van Laar** predicts that results will improve when participants understand experimental instructions better. No doubt this will become true as subjects and experimental designs become increasingly attuned to one another or, in the terminology of **Braisby & Franks** and **van Kruysbergen et al.,** as soon as subjects "automatically" take up the "right" perspective (cf. **Costall**'s reference to artificial laboratory conditions and **Brill**'s "conventions that *eventually* appear universals"). Finally, Van Laar does not seem to note that hue, value, and chroma are the Munsell system terms for hue, brightness, and saturation, and **Foss** makes the invalid inference that our goal is to defend relativism.

R4. The original Berlin and Kay theory.

Kay & Berlin suggest that each of the sentences (1)–(5) in their commentary, section 1, is either false or meaningless. Each sentence is a compressed summary of a long argument. (1)–(5) should be read in this light as should be clear from the immediately preceding paragraph. Briefly, the following may help clarify the five sentences:

(1) "Perceptuolinguistic system" is a label for the citation from Berlin and Kay (1969, pp. 109f), placing it both in the tradition of Lenneberg ("logico-linguistic systems using elemental terms . . . are axiomatic" [Lenneberg & Roberts 1956, pp. 30–31]) and the neo-Chomskyan ethnoscience programme (see Murray 1993; Saunders 1992).

(2) The use of English words to name universals indicates (1) is taken for granted, as it is in **Kay & Berlin.** Potential exceptions will be interpreted as "noise" (cf. Table R6).

(3) "Translation rule" refers to Hickerson (1971), a devastating critique of Berlin and Kay (1969), as **Simpson** notes.

(4) Because of (3), data will automatically support assumptions underlying (1)–(2).

(5) Berlin and Kay (1969) is a modification of the work of Lenneberg (1953), Brown and Lenneberg (1954), and Lenneberg and Roberts (1956).

Kay & Berlin present Lenneberg and Roberts as "relativists." This fails to take into account the arguments of the target article concerning the alleged autonomy and universality of colour. Many commentators write as if autonomy were a non-issue. For example, **Ingling** identifies it with "surveys of different cultures" about which he has "nothing to say." In the circular logic of universalist–relativist thinking, Lenneberg and Roberts are both universalist *and* relativist, as are Kay & Berlin. Both Lenneberg and Roberts as well as Kay & Berlin calibrate languages to what they regard as *the universal domain of colour.* Criticism levelled at methodology or alternative readings of data are dismissed as "relativism." This is also the approach of **Davies; Davidoff & Roberson; Foss; Ingling;** and **McManus.** Only **Simpson** recognises the false dichotomy presented by Berlin and Kay (1969). Contrary to his reading, however, Berlin and Kay were not the prime target of our article. Berlin and Kay's original thesis is very different from the notion of four unique hues. The tradition of **Kay & Berlin** and **MacLaury** in anthropological linguistics is wrong to see BCTs and unique hues as converging theories about the same issue. Other commentators address one or the other of the issues without answering the question whether they can be simultaneously true. For example, **Davies** implies that a perceptual-cognitive universal structure underlies "pink," without considering the implications for opponent colours and unique hues.

Lenneberg (1953; 1961; 1967; Brown & Lenneberg 1954; Lenneberg & Roberts 1956) seem to have developed much of Berlin & Kay's theory and all of their methodology. It is misleading to see Lenneberg as a relativist. His roots are in logical positivism (Lenneberg & Roberts 1956, p. 30, referring to Mach 1919/1900). Lenneberg (1953, p. 471) says that it is possible in limited cases to specify "meaning" by referring to the physical properties of stimuli – for example, colour (cf. **Braisby & Franks**). For Lenneberg (1967, p. 339), the colour space or "the entire world of colour" orders and measures "reality."

Brown and Lenneberg (1954, p. 458) asked five American English judges "to pick out the best red, orange, yellow, green, blue, purple, pink and brown" from the entire series of Munsell colours at the highest level of saturation (240 chips), because "these names are the most frequently appearing color terms in English," referring to Thorndike and Lorge (1944). Brown (1976, pp. 136–37) states that it was always evident that colour concepts are not proper sets but include small prototypical or focal colour areas as well as larger areas of colour chips falling less certainly under a given term. By 1960 the notion of focus was common currency. Also formulated (in 1954 and 1956) were the linguistic criteria by which Berlin and Kay systematized linguistic responses into BCTs. Berlin and Kay (1969) added three achromatic BCTs to Lenneberg and Roberts's American English categories, and proposed that BCTs are

Table R6. *Information on selected languages from the WCS (to be read in conjunction with* **Kay & Berlin***'s diagrams and data in Kay et al. [1991a; in press; in preparation]). Citations are from reports of trained field linguists who collected data or those who did first data processing; "field gloss" is a term assigned in the field (missing for most languages); "WCS gloss" refers to the term later assigned; "grue" is the WCS gloss that covers, at least, greens and blues*

Language	Country	Comments
Abidji	Ivory Coast	"Lots of variation among subjects. . . . Alternate analysis possible for many subjects."
Agta	Philippines	English and Spanish loan words provide four terms for grue: *asul, berde, blu,* and *grin.*
Bauzi	Indonesia	"Three other informants . . . were not given stage assignments because they had one basic term that included both white and black."
Bhili	India	"These are three terms . . . that are used for red. . . . The area covered by each term varies widely between informants."
Carib	Guatemala	"Terms 1–3, 11, 15 are Arawakan terms. The first three are primary color words. 1 is related to a word meaning 'clear,' 2 to a word meaning 'bad,' and 3 to a word meaning 'ripe.' 11 and 15 are not color words in their primary senses. Terms 4, 5, 8, 9, 12, 14 are borrowed from Spanish. Terms 10, 13 are from French. The sources of terms 6, 7 are unknown."
Cree	Canada	"The investigators had great difficulty in eliciting color terms. As a result most slides were left unnamed."
Gunu	Cameroon	"[O]ne of the associates, who helped with speakers 5, 9, 11, 19, 23, consistently recorded fewer terms than other investigators."
Halbi	India	"Well over thirty color words were recorded in the field, several noted to be from Hindi, making it difficult to interpret and keyboard this data set."
Iduna	Papua New Guinea	"Yellow always includes orange hues, often extends to pinks, browns, and even, interestingly enough, to purples. But purples are also often grouped with black as is brown."
Iwam	Papua New Guinea	"The large number of Speakers who mixed their colors probably indicates errors in the elicitation process."
Kalam	Papua New Guinea	"The naming of black is surprisingly inconsistent. The most common black term (S) is only used by eleven informants. Seven other terms are used for black including the white term (T) once, the green term (K) twice and the blue term (M) four times."
Karaja	Brazil	"[T]he responses may be more regular than normal as the chips were shown around the room and their responses were taped. Some may have given the same response as the previous one because it was more convenient not to disagree."
Konkomba	Ghana	"Some speakers had both Yellow and Green terms, others used the same term (usually yaankal) for both areas while others had no term at all for these two areas."
Kriol	Australia	"The majority of Kriol informants exhibit Stage VII color terminology with eleven basic terms all derived from English."
Kuku-Yalinji	Australia	"The yellow area is named in various ways with green (3), with grue (5, 9), with red (2), with a wild term (6, 7, 15 [field gloss 'dirty']), with blue (8), and dark-cool (10)." Field gloss "unripe" becomes WCS gloss *green;* "ripe" becomes *yellow* + *blue;* field gloss "clean bright" becomes WCS gloss *white* + *black.*
Lele	Chad	"Because there is such a small amount of data in these inventories, the analysis is very subjective and is not very reliable."
Mampruli	Ghana	"Two or three nonbasic words scatter in nonfocal areas and are, therefore, referred to as 'off' without reference to any particular focal term. In some cases – but not all – they tend to name light, pastel chips."
Mantjiltjara	Australia	Field gloss "blood-blood" becomes WCS gloss *red;* "earth" becomes *brown* as a WCS gloss, but is also referred to as pink (in the stage assignment of speaker #7).
Mazahua	Mexico	"Brown, purple, orange, pink and olive greens are basic terms for the majority of speakers. . . . Two red terms are used by a number of speakers; one focused in orange-red and the other in magenta."
Mazatec	Mexico	"The color system of Mazatec is a blend of the Native and the Spanish systems. . . . A borrowing for pink has become the general red term."
Papago	United States	"The investigator does not list Spanish as a second language for any of the informants, yet Spanish loan words are used by several informants and many have Spanish names."
Tifal	Papua New Guinea	"There are six words for red with none especially more prominent than the others."
W. Tarahumara	Mexico	"The ranges of term # 7 [used by 8 out of a total of 8 speakers] are erratic, making it difficult to even assign it to any one colour."
Zapotec	Mexico	22 out of 25 speakers listed as monolingual, but all use Spanish-derived colour words.

evolutionarily ordered in seven stages, noting that Geiger (1871; 1872) and Rivers (1901) had already formulated similar stage-theories.

Van de Geer (1960, p. 8; also Brown 1976, p. 134) spelled out the implications: a language-perception correlation gives no indication of the direction of causation. Hence results can be explained in two ways: (1) language determines perception (relativism) or (2) perception determines language (universalism). Although Lenneberg and Berlin and Kay adopt opposing positions on the issue, both take it as given that because sensory mechanisms and basic sensory stimuli are everywhere available, every language contains referents to elementary sensations.

R5. World Color Survey. Contra **Broackes; Foss;** and **McManus,** the World Color Survey (WCS) was indeed used in the preparation of the target article (cf. Table 5). For comments on the WCS, see also Saunders (1992; 1995a), Saunders and van Brakel (1995), and van Brakel (1994a). Some historical data may be relevant. The project "now nearing completion" (**Kay & Berlin**) appears to have begun in 1976/77 (NSF grant BNS 76-14153 – see Saunders 1992, p. 117), "was nearing completion during 1979" (MacLaury 1986, p. 4), appeared as an unofficial report in 1986, but was subsequently "being revised" (MacLaury 1987, p. 120). Some results were then presented at the 1989 American Anthropological Association Meeting and published (Kay et al. 1991a). MacLaury discussed them in (1992). Later copies of the WCS data sheets (Berlin et al. 1991), which had been sold by the Summer Institute of Linguistics (SIL, Dallas, Texas), were recalled on the grounds they were "only preliminary notes of the WCS project" that "were distributed prematurely and in error" and "which should not become part of any published record" (SIL letter, 2 July 1993). The flavour of the 1991 version of the WCS is given in Table 6, which should be read in conjunction with the diagrams in Kay & Berlin's commentary.

MacLaury (1992) used the WCS to challenge Berlin and Kay's original theory, arguing that prior to the evolution of hue stages, five evolutionary stages of brightness terms existed (cf. **Gellatly**'s comments on MacLaury 1992 and Luria 1976). **MacLaury** now uses the WCS to support four unique hues. He finds that 24% of the chosen foci fall on unique hues. This statement contains many suppositions. First, falling on unique hues means that 24% of foci fall in the four (out of 40) most-favoured columns of the Munsell chart; thus "unique hue" is identified (without argument) with a column of chips at maximum saturation, varying in lightness from 2/ to 9/. This means "Munsell unique hue" *red* includes a very pale pink chip; "Munsell unique hue" *yellow* includes apricot and very dark brown chips, and so on. Second, it is unclear with what the 24% should be compared. MacLaury says pure chance would be 10%. The point is not that 10% is too low – it is not difficult to process WCS data so that the difference between chance and what is found is more impressive. The point is that pure chance is unclear if unknown factors have systematic effect on outcome. MacLaury acknowledges that arbitrarily curtailed saturation levels are influential (cf. note 7 of the target article; **Davidoff & Roberson; Stanlaw;** and **Poortinga & Van de Vijver**) and correctly adds that "saturation-effect" is not the only factor – thus passing over the problem of talking about saturation in the first place (cf. **Mausfeld**). But the unaddressed question remains: How many determining factors are there? Finally, what MacLaury calls plurality peaks occur not only for red (8.1%), apricot + yellow + brown (6.5%), blue (4.7%), and green (4.5%), but also for orange + red-brown (2.7%), green on the blue side of "unique green" (2.4%), and purple (1.8%). That the "cut-off point" is after green and not, say, purple, seems to suggest some tailoring to unique hues. To respond that the cut-off point is "a little bit" before 2.5% (because that is pure chance) is ad hoc.

Kay & Berlin and **Stanlaw** are of course right that not *all* chromatic categorization/naming is determined by Western values and colour vocabulary. Stanlaw; **Poortinga & Van de Vijver;** and **van Kruysbergen et al.** are quite right that the classification of colours is never random or totally arbitrary; *if* one talks about *colour*, there is (trivially) more to it than "social idiosyncrasy" (**Dedrick**). There is plenty of order in the WCS. This order, however, is partly apparent and partly real, and the part that is real is "real" in different senses. Part of the order is created by the method used (the Munsell system), which limits possible responses in such a way that it measures nothing but degree of compatibility between subject and experimenter (cf. the attempt to fit the "Fore" into Ekman's theory of universal facial expressions for basic emotions [van Brakel 1994b, pp. 189–91]). Second, order is created by data processing (cf. Table 6). Given that anything that is not "hue" is eradicated as noise, what remains is influenced by methodological problems, such as distinguishing brightness as a salience parameter from the "thick" notion of a colour category (cf. also **Dedrick**'s criticism of composite BCTs; **Ross** is right that salience and independence are not the same). No doubt, as **Foss** suggests, the fact that the majority of languages contains a word that can in some contexts be translated as "red," has something to do with what all systems "humans + environment" have in common. This and Kay & Berlin's "grue" and "red-or-yellow" terms were pointed out in the nineteenth century and are irrelevant to the four hypotheses in the target article.

Poortinga & Van de Vijver and **Van Laar** weaken the universality claim (of Berlin & Kay and the WCS for example) in favour of probabilistic cross-cultural correspondence. But if the topic is changed from determinism to probability, then progressively more models fit the same data, rendering increasingly implausible the claim that the one universally valid law is tracked. The unscientific situation of adding extra (fudge) factors/dimensions/parameters to the theory on an ad hoc basis follows, a situation that to us aptly describes the current state of WCS data processing.

R6. Whither colour? Colour research should be recognised as an engineering project with a technological developmental trajectory. **Brill; Dubois; Gellatly; Mausfeld;** and **Simpson** in various ways recognise this. The colour space has become a *standard*, like the meter in Paris – the human colour space being nothing but this standard. It looks like the inference of a scientific law from an experimental outcome, but only against the backdrop of very strong assumptions: factual, general, and metaphysical. Those measuring what they call "experience" (**Werner & Bieber; Bornstein**) and those appealing to "the constraints inherent in the definitions of [unique hue and colour opponency]" (**Ingling**), always confirm their expectations because of the input–output ambiguity (Dreyfus 1994) and circular definition of colour (as **Dubois** and **Gellatly** note).

The colour space as the universal autonomous essence of colour must be abandoned. Colour is better characterized as the historical outcome of the development of an exosomatic organ. An exosomatic organ is an institutional structure that substitutes for, extends, or compensates for the inchoate powers of the human body (as do print, microscopes, telescopes, and computers). Just as alphanumerical systems extend the powers of calculation and theorisation, so too colour, now technologically defined (in terms of mathematical models, information systems, and practical utilities), recursively alters the powers of vision (cf. **Mausfeld**). It is this that colour science measures and describes.

To move colour into an exosomatic organ-space does not mean everything must go. Research on the elusive firings of opponent cells may lead to important insights and practical results, though it cannot claim to be investigating colour (as **Simpson** notes). "Explanatory simplification" (**Mausfeld**) of models for matching, difference thresholds, and constancy may go unchallenged (**Brill**) – albeit subject to Lockhead's (1992) and others' caveats about strict psychophysical laws. And "colour categories may still be useful if not 'real' " (Brill). If serious scientists (**Mausfeld; Poortinga & Van de Vijver; Zegura**) protest, the burden of proof is on them. They must answer: What must be true of a colour experiment if a general law of any form is to be inferred from it? Its design must also display a strong sense of what will enable or disable it. In the target article, four conditions essential to different contexts of the notion of colour space are queried. It is abundantly clear that each is flawed. None can be used to establish a general law.

To begin an alternative account, let us sketch how the colour space as a techno-scientific standard came into being. First, Descartes transfered pictorial properties – colour and line – from the visible to the mental to illuminate the power of reason in its quest for certainty (cf. **Dubois**'s "overemphasis on perceptual processes"). The deterministic mechanical system of the universe produces clarity and distinctness in the inner theatre of the mind, thereby providing the criterion of truth by which knowledge can emerge (Judowitz 1993). However, what is clear and distinct is not a mimetic image of the world delivered by the senses, but the innate compositional elements of that image of the world delivered by the senses, but the innate compositional elements of that image governed by reason. Nature being inherently mechanical, the certitude of compositional design is guaranteed by mathematical constraint. Truth in vision is to see a few things systematically in terms of mathematics (exemplified by **Hubey**). In outline, this provides the a priori definition of the colour space dominating (putatively) empirical research, exemplified by **Davidoff & Roberson's** suggestion that the problem raised by the target article is an empirical issue concerning the organization of the internal colour space. Many commentators take for granted, (i.e., as a priori) the existence of an internal colour space relative to which empirical issues are defined (**Abramov & Gordon; Davies; Foss; Hardin; Hubey; Jameson; Poortinga & Van de Vijver;** and **Sokolov**).

Second, Newton bootstrapped the correspondence between spectral and surface colours into existence. He held that the a priori correspondence between regularity and consonance, guaranteed by alchemy and the mechanics of matter, reveals fundamental truths to be ratios in the soul and cosmos (Gouk 1986/87). As numerically representable relationships, they are realised, represented, and exhibited in an endless variety of ways – as structures in man and cosmos mirror one another. Only through the modern enactment of experiment with its special setting can the (numerical) nature of structure (regularity and consonance) reveal itself. Whereas the endorsement of the combination of mathematical and experimental method characterizes modern science, neo-Platonism, Pythagorean number mysticism, and the Kabbalah suffused Newton's thinking.

In accordance with the unity of light and matter, to confirm the pictorial constraints and inner geometrical coherence, Newton rolled up the spectrum to form the colour circle (*Opticks*, p. 155). In his *Optical papers* (pp. 541–43), Newton stated that spectral proportions reveal "the reason . . . why" pigments combine as they do. Because optical mixing (of prismatic colours) and pigment mixing (of coloured powders) are analogous, their explanations can be reciprocally adjusted to one another (Topper 1990). Here is the source of the conflation of chromatic lights and coloured surfaces (cf. **Costall,** contra **Mausfeld,** whose dialectical relationship between lights and surfaces, which includes an appeal to "elementary perceptual categories," is a different issue).

Most present-day scientists probably think of Newton's *Opticks* in terms of Humean inductions, but it is better described in terms of deductions. The data plus the description of the experimental set-up deductively imply the law to be established. Newton is the prime advocate of the deductive method in reasoning from background assumptions to experiment to law (Cartwright 1992). His *experimentum crucis* sets the framework for what **Mausfeld** describes as the reciprocal shaping of theory and practice, and **Brill** considers the interaction between technological tools and modes of expression.

To understand the modern colour space, it is necessary to grasp modifications to this legacy: first, by the elaboration of Descartes' space of chromatic figuration and Newton's spectrum into three dimensions; second, by the application of physiological theory to sensations, thereby translating sensation into a form congenial to mathematical generalisation (cf. the approach of **Hubey** and **Sokolov**). As quantifying sensation becomes increasingly central to the experimental method, measurement requires a numerically graded stimulus series (Danziger 1994), which Newton's spectrum provides. Chromatic sensation (sense data) is correlated with dominant wavelength of light, and wavelength (physical stimulus) measures the inner sense (psychological variables, neurons).

Although permutating each genus of colour with lightness has been attempted since Aristotle, it was not systematically rendered until Runge's *Farbenkugel* in 1810. The third dimension of the colour space, purity, remained elusive until Grassman (in 1853) produced his Leibnizean notion of saturation (although this was rejected by many early colour scientists, who were aware of the physicalistic trap of slicing colour perception along the joints of elementary physics – as **Mausfeld** points out).

Retaining freedom from mimetic illusion, with the addition of the third dimension, Descartes' original space of chromatic figuration became the modern colour space. This a priori colour space figures in all discussions of colour and colorimetry, and is further scholasticized into a variety of subspaces: for example, the "receptoral" and "post-receptoral" colour space, the psychophysical and physiological channels spaces, the third level colour space, the lightness colour space, and the reflectance colour space (Thompson 1995, p. 8). The only important change since Newton is that the *telos* of colour vision is no longer consonant with God's Sensorium but rather an evolutionary adaptation for integrating a physically heterogeneous collection of distal stimuli into a small set of visually salient equivalence classes deployed in a variety of perceptual conditions (Hardin 1988; Thompson et al. 1992; Thompson 1995). The original *telos* of harmonic structure can still be felt, for example, in Shepard's (1991) proposal that three degrees of freedom of terrestrial illumination correspond to the light-dark, red-green, and blue-yellow opponent processes in the brain (cf. **Zegura**).

Colour as part of the exosomatic organ-space has come to exist as both an objective and a subjective fact of the world. Once brought forth in technological form, like literacy and numeracy, it feeds back into and conditions its originating universe and mode of awareness (cf. **Brill**'s colour as a learned "universal" order). Seeing colour, like reading and mathematical calculation, becomes an action requiring skill, a modern technological skill in which a set of particulars are shaped as a specific tool to guide attention and action towards particular fixed distinctions. Manufactured coloured entities become built into consciousness through a process of standardisation and taken-for-grantedness that builds up a stable framework (as **Brill** and **Simpson** note). The tool, skill, and framework become so deeply interiorised that a change in the habits of existence is brought about, those exposed to the apparatus becoming so fused with it that they cannot avoid being subject to its operational conditions. When chromaticity is brought forth according to the rational and objective image – the model of a mathematicized colour space – that image modifies all subsequent vision. Once in place, optical habits and daily practice combine to consolidate and feed back that image into its originating universe of processes and events.

The four hypotheses reviewed in the target article should be read as a measure of the degree to which this apparatus has come to monopolise the ground of perception. It is a measure of the narrowing of the polymorphous potentialities of vision to a single way of accounting for the world – a measure of vision changed by technology as the habits of the eye are slowly altered to become accustomed to images typical of industrial production. The fundamental error has consisted in proclaiming this system of relevance to be the pre-existing model-in-thought or essence of colour, instead of recognising it for what it is: an acquired bodily tool, skill, and framework or exosomatic organ.

NOTE

1. In response to comments of **Bornstein, Kay & Berlin,** and **McManus,** the following corrections were made to the final text of the target article (added text is in italics; deleted text is in square brackets). Section 5.3: "It has been suggested that people in those areas have different *macula and lens* [cone] pigments (Bornstein 1973a; Rivers 1901)." Section 2.1: "Thorndike's *& Lorge's The* Teacher's *Word Book* [Handbook] *of 30,000 Words*." Under Kay et al. (1991) in the list of references: "Biocultural implications *of systems* of colour naming. *Journal of* Linguistic Anthropology."

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Letters "a" and "r" appearing before author's initials refer to target article and response, respectively.

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