

More on the origins of the hues: A reply to Broackes

Wayne Wright

Abstract: This paper responds to Justin Broackes' reply to my paper, "On the retinal origins of the Hering primaries." This paper aims to clarify and further develop the ideas presented in that article. I take up several of the points Broackes raises regarding the connection between my work and that of William Thornton (1971 and 1999) on the "prime" and "anti-prime" colors of the human visual system, the connection between those prime and anti-prime colors and hue category boundaries, and my attempt to link facts about low-level processing to the unique hues. Ultimately, while Broackes brings up several interesting issues and usefully illustrates the limits of my approach, I see no reason to retreat from the main claims I made about human spectral sensitivity and the hues.

I begin by thanking Justin Broackes for his insightful reply to my 2011, as well as the remarkable referee's report he provided when I submitted that paper to this journal.¹ I have learned much from his feedback. His reply provides a good opportunity for me to further clarify and reflect on the project undertaken in my 2011.

1. Issues regarding Thornton's work

Some remarks are in order regarding Broackes' discussion of the connection between the ideas proposed in my paper and the details of William Thornton's work on the "prime colors." These points also relate to my use of Romney & Chiao's (2009; hereafter, "R&C") color appearance model. Recall that the prime colors are those wavelengths to which the visual system has the strongest response, while the anti-primes are the wavelengths that evoke a minimal response.

¹ Broackes identified himself as one of my submission's referees and the idea to have him write a reply to my paper naturally grew out of the comprehensive report he provided.

First, Broackes notes that the prime and anti-prime wavelengths I work with based on my replication of R&C's model differ from those in Thornton's (1971) paper on the luminosity and color rendering performance of artificial white lights. I derive primes of 445, 535, and 605 nm and anti-primes of 490 and 570 nm.² Those fit well with the prime and anti-prime locations Thornton (1999, pp.152,154) finds in color matching experiments: those primes are 450, 535, and 610 nm and the anti-primes are 490 and 570 nm. The fit is slightly less good with the primes and anti-primes Thornton (1971, pp.1157-1159) reports: primes of 450, 540, and 610 nm and anti-primes of 495 and 575 nm. The latter anti-prime is presented by Broackes as offering a counter-example to my account. I claim that borders between hue categories (e.g., between yellow and red) are defined by primes and anti-primes, but the 575 nm anti-prime of Thornton (1971) is more-or-less the position of unique yellow. Given the phenomenological purity of the unique hues – e.g., that unique yellow has no tinge of either reddishness or greenishness in it – this would seem to pose a nearly lethal threat to my approach.

I don't think the details of the different concerns of these two papers from Thornton are relevant to the slight (but potentially crucial for my proposal) disagreement between their reported primes and anti-primes. What matters is that the 1971 study was based on theoretical considerations developed using the color matching functions (CMFs) of the CIE Standard Observer as "weighting functions" on the light signal reaching the eye; i.e., the spectral power distribution (SPD) of the incoming light is multiplied, at each wavelength, by the CMFs in order to determine the chromaticity of the light (taking integrals or sums over the products for each CMF). By contrast, the 1999 study uses experimental data as part of a larger project that

² I also include an additional anti-prime in the extra-spectral region for reasons explain in my 2011. Since that point is irrelevant to the issues treated in this section, I pass over it here.

Thornton (pp.155-156) contends establishes (i) that manipulations to the data from which the CIE Standard Observer was developed have had deleterious effects on color research and (ii) that, vital for present purposes, color matching functions fail when used as weighting functions. Those failures do not show up in many cases in which the CMFs are used as weighting functions. They emerge only with strongly metameric stimuli that are physically similar throughout the spectrum except that they have pronounced differences around the anti-primaries. In such cases, the weighting functions do not “see” as anywhere near equivalent stimuli which are indistinguishable to actual human observers. For these reasons, I regard the primaries and anti-primaries from Thornton (1999) (and the multi-part study on colorimetry Thornton published in Color Research and Application throughout the 1990s) as more secure. While I did not state this in my 2011 but should have, I take the results of the 1971 study to be merely suggestive of the actual primary and anti-primary wavelengths of the human visual system. If Thornton’s later reasoning is right, those earlier results should not be taken as the final word on the matter, especially when it comes to the locations of the anti-primary wavelengths.

In adopting this approach, I am simply following Thornton’s own perspective on his work. Thornton (2000, p.148) characterizes the primaries identified in his earlier publications (e.g., his 1971 and 1972) as those of the CIE Standard Observer while describing the primaries presented in his 1999 as those of a normal human observer. In the same place, he claims that the primaries from his 1999 are “more accurate” than what he previously offered. Thornton (2000, p.149) also notes that to maximize visual efficiency in a color imaging device, an engineer ought to use the primary colors for actual normal human beings and not those of a CIE Standard Observer. Additionally, his remarks in his 2000 give no suggestion at all that Thornton would expect there to be different primary colors for color matching, on the one hand, and artificial illumination, on

the other. In fact, it is quite clear that he takes the specific primes identified in his later work as central to a wide range of phenomena, such as matching, imaging, illumination, and printing.

Another relevant issue has to do with Broackes' handling of Thornton's explanation of the remarkable efficacy of the prime colors and the troublesome nature of the anti-primes. Broackes is certainly right that Thornton (1999, p.154) both interprets the primes as peaks of spectral sensitivity in the human visual system and proposes that new spectral sensitivity curves – ones that differ from those of the CIE Standard Observer and which can be properly used as weighting functions – be organized around them; Thornton (1999, fig.27) offers a sketch of what such curves might look like. Broackes, at least initially, reads those hypothesized curves as new cone functions and points out that they are in profound disagreement with a large body of established research on cone sensitivity, such as that of Stockman & Sharpe (2000). Clearly, a lot of work would have to be done to justify such an extreme departure from the received understanding of the cone sensitivities. If Thornton's ideas about the true spectral sensitivities of the human visual system really demand such a heresy, linking my account of the origins of the Hering primaries to Thornton's work looks like an unwise or risky choice.

The fact is, though, that Thornton does not offer his hypothetical weighting functions as cone sensitivity curves. That they are presented as representative of “elementary visual system sensitivities” (1999, p.154) might suggest such an understanding. However, Thornton (p.155) explicitly states that those curves are to be treated as CMFs (in the sense of weighting functions), not cone functions. No-one seems much bothered by the fact that the x-bar function of the CIE CMFs peaks around 600 nm while the L cone curve of Stockman & Sharpe peaks at 570 nm. Consequently, I don't see a problem for Thornton on this score. Note also that Thornton is “assuming the traditional model of the visual system” (p.139). Since visual processes do not have

direct access to cone quantum catches or anything else along such lines, talk of the “basic” or “elementary” spectral sensitivities of the visual system need not commit one to specific claims about cone sensitivities. I take it to be a virtue of the R&C model I employed that it provides a computational means of getting from the Stockman & Sharpe cone sensitivities to curves representing low-level spectral response that are strongly in agreement with Thornton's findings and his hypothesized sensitivities; besides the coincidence of the derived primes and anti-primes with Thornton's, compare fig.27 of Thornton (1999) with fig.6 of my 2011. One of the most interesting features of the R&C model is that it links insights from the often separate worlds of academic color science (such as Stockman & Sharpe's cone sensitivity curves) and practical color technology (such as Thornton's work on artificial illuminants); see Romney & Chiao (2009, p.10380).

While Thornton is not proposing a radical reinterpretation of the established cone curves and does not call into question the CIE data itself, his rejection of the CIE Standard Observer CMFs as weighting functions is far outside the mainstream. This bears on Broackes' remarks about the CIE luminous efficiency function – $V(\lambda)$, which is \bar{y} of the CIE CMFs – and the model wavelength components derived using the R&C model. Broackes claims that while the model wavelength components I derive in my replication of the R&C model – functions e_1 , e_2 , and e_3 in figure 2 of my 2011 – might have certain virtues when it comes to efficient transmission of information, they are in some way unrealistic when considered from the perspective of what is known about human spectral response. Of chief concern is the supposed lack of any correlate to achromatic response in the R&C model. The e_1 function I (and R&C) work with, which is interpreted as an achromatic dimension, looks nothing $V(\lambda)$. However, I should remind the reader of some key points made in my 2011. R&C (p.10378) observe that

model output for Munsell chips on their e1 dimension (which for all relevant purposes is the same as my e1) correlates 0.9992 with the chips' achromatic L* coordinates in CIE L*a*b*. Additionally, while the e1 curve diverges from V(λ), it very closely resembles the brightness curve derived by Schrödinger (1920/1970, p.164). While Schrödinger dismissed this sort of curve on what seem to be aesthetic grounds, its peaks occur in the immediate vicinity of wavelengths known to maximize luminous efficiency in artificial lights and it bottoms out in a region recognized to impair luminous efficiency.

For such reasons, I disagree with Broackes about there not being a correspondence between the R&C model wavelength dimensions and achromatic response. What there most certainly is not, though, is a simple correspondence between any of those dimensions and V(λ). I don't find this troubling, at least if Thornton is right about the inappropriateness of the CIE CMFs as weighting functions. Moreover, with respect to how realistic such functions might be, Thornton points out that “no CMFs derived from real primaries [in color matching experiments]” peak at the 555 nm position that \bar{y} was forced to peak at through the transformation to imaginary primaries. Fairman, Brill, and Hemmendinger (1997, p.21) have also argued that “none of [the] formulating principles [of the CIE system] would be adopted if the system were formulated from a fresh start today.” One of those principles is to incorporate V(λ) into the CMFs as \bar{y} . Considering the important differences between brightness and luminance, it is not obvious why one would expect the achromatic dimension of a color appearance model to resemble the CIE luminosity curve; see Fairman, Brill, and Hemmendinger (1997, p.20).

2. Primes, anti-primes, and hue category boundaries

Broackes raises some interesting points regarding the correspondence between hue category boundaries and the prime and anti-prime colors, as well as how such correspondences might be of explanatory value. He seems to be saying that the correspondences I make so much of might not be particularly good (at least taken as a whole) and that even if the correspondences stand up, it is unclear why we should lend them any significance. Basically, the worry is that I am engaged in little more than “numerology” (or “data snooping”) and perhaps not even very interesting numerology! I think I can allay the bulk of these concerns. Moreover, the residue usefully serves to illustrate the limited nature of my proposal. I tried to convey those limits in my 2011, but I suspect certain of my statements there might suggest that I am trying to do more than what I really set out to. Thus this is a welcome chance to clarify my actual ambitions and what I take myself to have shown, strengthen my case a bit, and also come fully clean on what I think my approach cannot achieve.

Regarding the hue category boundaries, my claim is that as one goes around the hue circle (as in fig.9 of my 2011), the primes and anti-primes mark crucial boundaries between the Hering primaries (red, green, blue, and yellow), plus purple. I take three of these boundaries to be securely shown. For example, based on the mean hue functions reported in Abramov & Gordon's (1994 and 2005) hue-scaling studies, the balance points (i.e., 50% of one hue and 50% of the other) for blue and green, green and yellow, and yellow and red are 490, 570, and 600-610 nm, respectively. That's two anti-primes and a prime. Other considerations based on the ranges of Munsell chips and wavelengths selected as exemplifying a unique hue were also presented in my 2011. What about the two remaining primes (445 and 535 nm) and the extra-spectral “anti-

prime” I introduce? As Broackes states, the 535 nm prime gets its own special story, so I'll set that to the side for now.

About 445 nm, it is not a point of balanced hue judgment between blue and red. Unsurprisingly, such a balance point doesn't appear in Abramov & Gordon's research, as a “pure” purple (which would be the natural candidate for being the balance point between blue and red)³ is extra-spectral and they used spectral lights. Of course, my extra-spectral anti-prime does not show up in their studies. However, the region between 445 nm and the extra-spectral anti-prime is quite interesting; see figure 9 of my 2011. The extra-spectral point falls right at the break between the Munsell Red-Purple and Purple sectors. The 445 nm point falls between hue steps 10 and 7.5 of the Munsell Purple-Blue sector. Thus the region fenced in by those two

³ I say this while also wanting to raise a point about phenomenology. Broackes claims that “There are no purples that do not look to have some red and some blue in them” (p.XXX) and in correspondence has said much the same about red and yellow always being seen in orange samples. Such claims are, of course, common in philosophical and scientific discussions of color. While I can understand their appeal, my own phenomenological reflection leads me to not know what to make of them; I have normal color vision by the usual tests. For example, I can find in the Munsell Book of Color orange (and purple) chips with hues that appear to me as basic as any unique Hering primary. I do not detect any redness or yellowness (/redness or blueness) in them, but find only certain similarities to redness and yellowness (/redness and blueness), in the same way that I find similarities between blue and green. However, samples between, say, focal yellow and this balanced/unique orange have a binary appearance that is described pretty well by “seeing amounts of yellow and red in it,” although at a certain point it becomes natural for me to prefer describing them as more-or-less orangish. Similar remarks apply for purple. On the other hand, samples between blue and green and between green and yellow all have a binary appearance to me. These observations are among the reasons I have for taking seriously Jameson & D’Andrade’s (1997) dissent from mainstream thought about the hues. See also Jameson (2010).

points contains all the Munsell Purple chips plus Purple-Blue chips that have a strong “purply” appearance. Furthermore, if one considers the ranges of Munsell chips chosen as unique blue reported in studies like Kuehni (2005), one finds that the span (which extends all the way through the Blue sector) begins at 7.5 Purple-Blue; i.e. right up against the 445 nm prime on its blue side. As for unique red selections, the range begins in the Red sector very close to the 605 nm prime and extends into Red-Purple, on some reports about midway in the sector and on others even closer to the Purple sector (see Kuehni 2001 and Hinks et al 2007). I take these points to support my claim that the 445-490 nm range in the model contains the most distinctively blue samples and that it should be thought of as the “core” of the hue category of blue. Similar remarks obtain for red and the region between 605 nm and the extra-spectral anti-prime. As I noted in my 2011, linking such regions to the Hering primaries (or, as I now do, calling them the core of those categories) is not to say that, for example, blueness is found only between 445 and 490 nm. The idea instead is that the stimuli most highly characteristic of a given hue plot in those regions.

Purple does complicate my account. I agree with Broackes that subjects could be counted on to make judgments of blue content in excess of 50% in some purple samples and that a purple sample judged to be 50% blue will not line up with the 445 nm prime. A fairly radical way of addressing this point would be to adopt the five hue model endorsed by Jameson & D'Andrade (1997) and to claim that purple is just as psychologically “basic” as the Hering primaries. According to Jameson & D'Andrade, the Hering primaries are perceptually salient – and color names tend to divide up the color space around them – because they correspond to distinctive regions in color space. It is not due to them being fundamental elements in terms of which all hue percepts are organized. Jameson & D'Andrade’s view has the upshot that the distinctiveness

of those regions allows them to serve as “distance landmarks” in hue judgments, despite the Hering primaries not being quantities out of which all hue percepts are forged. Thus hue scaling studies turn out to be tapping subjects’ judgments about distances from these landmarks, not anything having to do with weighted contributions of elemental hues, despite the task instructions typically being given in terms of the latter. This is a serious break from the received interpretation of such experiments. On this view, purple should be put on a par with the Hering primaries, as including it helps sharpen subjects’ judgments about the redness and blueness of stimuli; see Indow (1987). In fact, from this perspective one might conclude that the failure of typical hue-scaling studies to include purple as a term to describe hue stimuli (and, perhaps, the omission of extra-spectral samples) ought to squelch any concerns one might have about the 445 nm prime not corresponding to a point of balanced hue.

In my 2011, I noted that I have a good deal of sympathy for Jameson & D’Andrade’s proposal. Nonetheless, it is unorthodox enough that I should consider other means of dealing with Broackes’ challenge regarding purple. It is at this point that the limits of what I am trying to do become relevant. In presenting an account of what I call the retinal origins of the Hering primaries, in no way do I mean to be claiming that the formation of the Hering primaries is complete at the level of processing addressed by the R&C model. The hue categories and their focal exemplars (i.e., the unique hues) are psychological phenomena, while the R&C model and Thornton’s work address low-level details of the visual system. The hue-related phenomena that appear at those early stages of processing may very well end up being mixed-and-matched in various ways along the way to conscious color experience and subject performance in experiments. In fact, I have every expectation that they would be. There is variation across subjects in their hue-scaling judgments and their unique hue choices. Some of that variation

presumably is due to differences at the very front-end in the visual system, such as cone polymorphisms. A version of the R&C model can be constructed for individual perceivers, by using their actual (if known) cone sensitivities rather than the Stockman & Sharpe functions. Differences in the resulting models should be relevant to – but will not nearly be full explanations of! – interobserver variation in hue judgments. The visual processing at subsequent stages that builds on that early processing will also vary across different perceivers, for reasons that are not yet well understood. Additionally, since hue scaling and unique hue choices involve judgments about category membership, representativeness, and the like, cognitive factors that can differ across individuals would also have to be taken into account. Once these later factors and their connection to hue judgments are better known, more of the picture can be filled in. For now, the crucial idea being proposed is that certain key features of hue experience and hue judgments are strongly influenced by – if not constrained by – specific phenomena that appear very early in visual processing; viz., the prime and anti-prime colors. This stands in stark contrast to the thesis that the hues are entirely a product of (imposed by, etc) higher level mechanisms.

Suppose that we agree that the four Hering primaries are perceived uniquely or in binary combinations in all hue percepts, while also granting (in the light of studies like Indow 1987) that purple has an important role to play in at least some hue judgments. In that case, one might suppose that higher levels of processing whittle down the five regions I highlight to four for certain purposes, while those five regions are (more-or-less) retained for other purposes. Those different processes (and potentially their interaction) may produce results consistent with what has been noted about blue and purple. No Munsell chip on the purple side of the 445 nm prime is selected as unique blue, but there are stimuli in that area that are judged as having bluish content of greater than 50%. It may very well be that whatever processes are responsible for directing

unique hue judgments and which can be tuned in different ways that account for the observed interpersonal variation in such judgments, the 445 nm prime is a border that cannot be crossed by any such tuning. Because purple is in some sense a distinctive hue category due to being marked off by the 445 nm prime and the extraspectral anti-prime, perhaps this excludes any possibility that unique blue could fall within its range. On the other hand, it is a regular feature of judgments that blue is the predominant hue component in a stimulus that they can go across that border, thus suggesting that the distinctiveness of purple has little or no effect on them. There's admittedly a good bit of speculation here, as well as further details that need to be worked out. Whether any of this is defensible will depend on how future empirical research develops. Nonetheless, the proposal has enough going for it so far that it's worth considering seriously as we wait to see what empirical results come in regarding these other factors.

The account of the green-yellow region fenced in by the 535 nm prime and 570 anti-prime illustrates much the same point, although to an opposite purpose. While purple has a claim to perceptual salience and being treated as a distinct basic hue category, green-yellow is not perceptually distinctive as such and does not form a basic hue. One might suspect that this fundamental difference between purple and green-yellow is attributable, at least in part, to differences in their environmental significance to our evolutionary antecedents. Particularly relevant here are the plant pigments anthocyanin and chlorophyll. Anthocyanins are associated with nutritious rewards for many animals, such as primates. Anthocyanin molecules come in many varieties, including purples; for more on anthocyanins, see Lee (2007, pp.73-77, 245-247,292-293). Thus it may very well be biologically advantageous for purple to be so perceptually salient. On the other hand, the background against which a great many biologically crucial tasks take place is green-yellow due to the optical properties of the chlorophyll molecules

in leaves and grasses; note that the two varieties of chlorophyll “are the most abundant pigment molecules on earth” (Lee 2007, p.64). As I suggested in my 2011, it is plausible that green-yellow would be marked out as special somewhere in the visual system, precisely so that it can be made inconspicuous, thereby allowing important stimuli to stand out in their green-yellow surroundings.

Within the framework of the account I propose, the first step in doing that is achieved by isolating such stimuli between the 535 nm prime and 570 nm anti-prime. Of course, somewhere along the way to conscious color experience (and likely also involving cognitive factors that influence hue judgments), green-yellow effectively gets lumped together with green rather than yellow; e.g., unique green choices extend into green-yellow while unique yellow choices do not, hue-scaling stimuli are judged as being predominantly greenish in content up to the 570 nm anti-prime, green-yellow grass and leaves are routinely cited as paradigm examples of green. This likely stems from two (related) facts. First, typical green and green-yellow stimuli have similar bell-shaped reflectance spectra that peak near the middle of the visible spectrum while many common yellows (e.g., flowers, fruits, and Munsell spectra) have staircase-shaped reflectance spectra that are in the midst of the rise from their low-reflectance piece to their high-reflectance piece in the middle of the visible spectrum; see figure 1 below and Maloney (2003, p.243). Second, it is biologically advantageous to have certain yellow stimuli be easily discriminable from their green-yellow surroundings. In a sense, the idea is that green-yellow, since it does not form its own distinctive hue category (for the reasons offered above), has to go somewhere when it comes to hue categorization and it is more expedient for it to go in the green category rather than the yellow category. That is accomplished by processes that take place after the stage(s) of processing addressed by the resources employed in my 2011.

The preceding remarks about purple and green-yellow lead to some points that are relevant to Broackes' question about what rationale there would be for the prime and anti-prime colors to signal transitions between hue categories in the way I propose. Broackes is right that narrowband or monochromatic stimuli are rare or altogether absent in natural viewing circumstances and that changes of hue with wavelength are not something we are aware of as such in our color experience. However, the prime and anti-prime wavelengths turn out to be diagnostic of important patterns in biological (and artificial, for that matter) surface reflectance properties. As just noted, biological greens tend to have a characteristic element that is bell-shaped and that peaks in the middle of the spectrum. Many biological blues have a similar bell-shaped element that peaks in the shorter wavelength portion of the spectrum. Reddish and yellowish plant spectra are often staircase-shaped, with a low, basically flat component through the short wavelength region and into the middle wavelength region, and a rapid rise that peaks at longer wavelengths. These features (and others) are due to the optical properties of plant pigments like those mentioned above. The "characteristic reflectance curves" in figure 1 demonstrate these patterns.⁴ Similar patterns are found in Munsell chip spectra, with Munsell Reds and Blues having flatter long wavelength components than their biological counterparts.

⁴ Characteristic curves were determined by first separating plant reflectances (those used in my 2011) denominated by 'red', 'green', 'blue' (or 'violet'), or 'yellow' into four groups based on those generic color descriptors; "reds" with strongly U-shaped reflectance spectra were reassigned to blue. For each sample, a new curve was derived, with the value at each wavelength determined by dividing the sample's reflectance at that wavelength by the sum of the sample's reflectances across all measured wavelengths. For each hue group, the characteristic curve was generated by taking the mean of the values of its members' new curves at each wavelength. The rationale for using the new curves and characteristic curves based on them, rather than empirical

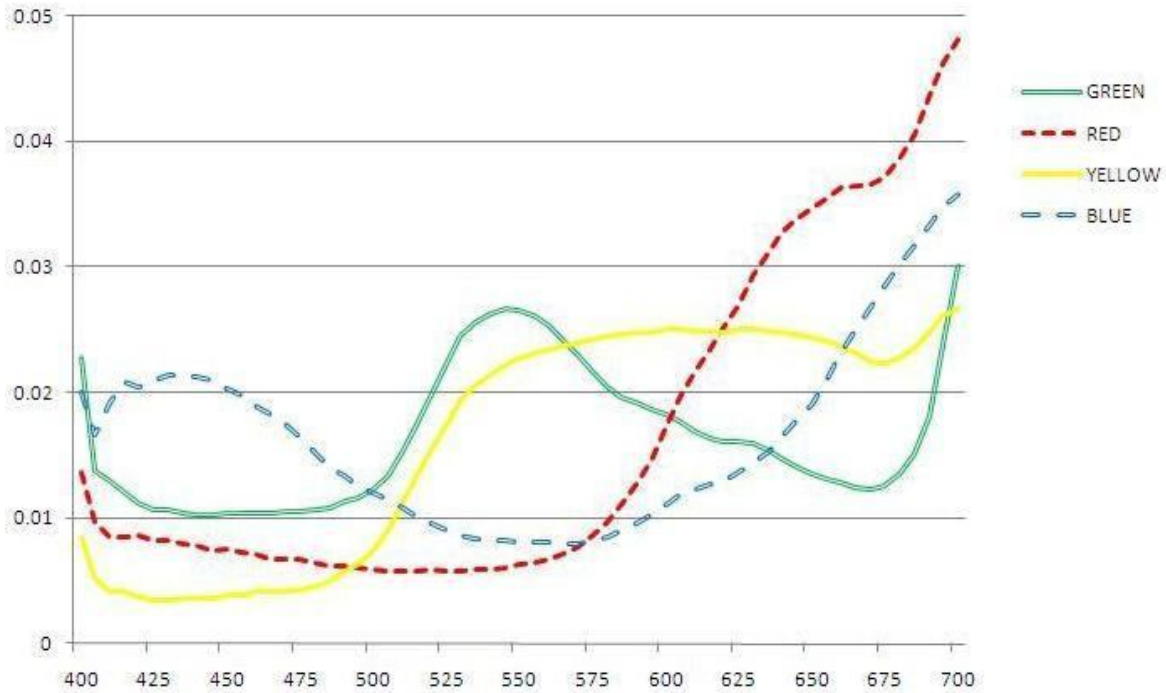


Figure 1: Characteristic reflectance curves for plant samples denominated as ‘red’, ‘green’, ‘blue’ or ‘yellow’.

Focusing on ordinate value differences at each wavelength between the curves in figure 1 reveals that prime and anti-prime colors fall in regions of the spectrum that facilitate useful distinctions amongst stimuli. Greens, reds, and yellows have largely flat components at short wavelengths, leading to stable differences between them there. However, the differences between the blue curve and each of the others rise and fall in that region, with peak differences at or close to the 445 nm prime. At 490 nm the differences between blue and green are rapidly nearing a

reflectances, was to enable comparisons of patterns in natural spectral shapes that are unaffected by variations in absolute reflectivity throughout the spectrum between individual samples or groups.

zero-crossing that marks a definitive transition between the two bell-shaped curves. The differences between red and yellow also have a zero-crossing at 490-495 nm. It is in that neighborhood that yellow begins its climb while red remains flat and low. In the vicinity of the 535 nm prime are maxima in the differences between green and both red and blue. Yellow and green closely track one another in shape between 400 nm and green's peak near 535 nm, after which the right-hand descent of the green "bell" begins while yellow remains high and flat throughout the rest of the spectrum. This leads to a zero-crossing in the difference between yellow and green that is flanked by 565 nm and the 570 nm anti-prime. As with the 490 nm anti-prime, there is a second zero-crossing close to 570 nm, this one in the differences between red and blue at 570-575 nm. Thus in this region yellow and green each has a large difference with both blue and red. The 605 nm prime is the location of a zero-crossing between red and green, while there are clear differences between those two curves and the other two at that point.

While the features just noted merit more careful attention, for now it suffices to observe that they provide further reason for taking seriously that the hues are closely connected to the prime and anti-prime colors. Of course, some features that emerge from inspection of the differences between the characteristic reflectance curves do not correspond to the prime and anti-prime locations. Some of the neglected features may have been ignored because they are redundant with or less useful than those that were seized on. Perhaps other factors that likely shaped the evolution of the human visual system – such as the optical characteristics of blood and skin (Changizi et al 2006) – led to arrangements that prevented capitalizing on them. Chance alone is also a possible explanation. Future work along these lines should shed light on such matters. For now it suffices to say that if one wanted to distinguish between many different kinds

of plants on the basis of their surface reflectance properties, sampling at the prime and anti-prime wavelengths would be very effective.

3. Unique hues

I close by taking up some of what Broackes has to say about my treatment of the unique hues. The way Broackes presents things suggests that my focus on details of visual processing to account for certain hue-related phenomena comes at the price of excluding consideration of ecological factors. As the above discussion should make plain, I don't see a focus on low-level processing as excluding ecological factors. One can concentrate on the environment or visual processing, without taking the position that either is the "true" or "complete" way of accounting for things. Presumably, ecological factors are relevant to explaining why our visual equipment behaves as it does, although I doubt that every feature of visual processing and conscious experience admits of or requires such explanation. I certainly think that environmental features do bear on the connection between hue category boundaries and the prime and anti-prime colors. I also see there being no real conflict between my account and much of what Broackes says about unique blue and unique yellow. Like him, I also find unique green and unique red harder to deal with.

Broackes notes that (going by the means found in various studies) unique blue falls at 476 nm and unique yellow at 576 nm. Those are also the locations on the spectral locus with which skylight and direct sunlight plot in alignment, respectively. This is true in both the CIE xy chromaticity diagram and the R&C model. Those wavelengths lie at opposite ends of a line on which much of the daylight locus (the variation in natural illumination due to the sun's height, cloud cover, and so forth) falls. This leads Broackes – following Roger Shepard (1992) and John

Mollon (2005) – to entertain the idea that the positions of unique blue and unique yellow are “set by the illuminant of the environment” (p.XXX). This would be advantageous for, among other things, achieving color constancy as the illumination changes throughout the day. I see nothing objectionable here at all and am tempted by the same kind of considerations. However, our visual system cannot directly determine what in the environment is the source of the illumination and straightforwardly isolate that from all the other factors that shape the color signal reaching the eye. As Broackes himself suggests, the visual system might be able to identify, for example, unique blues based on characteristic features of how such sources affect the light reaching the eye (relative to how other sources affect that light) and how the eye (and visual system more generally) responds, particularly when it comes to changes in achromatic response as the illumination on the surface changes.

The role of certain aspects of visual response in identifying unique blue and unique yellow stimuli is precisely the sort of thing addressed by the account I offered. Thus it is unsurprising that the method of predicting unique hues based on Jameson & D'Andrade's (1997) conjecture that the unique hues are linked to regions of distinctive ratios of chromatic to achromatic sensitivity, predicts unique blue at 475-480 nm and unique yellow at 575 nm.⁵ None of what I said in my 2011 was meant to imply that something's being unique yellow is no more than a matter of it plotting at a point with an extremely low ratio of chromatic to achromatic sensitivity. From a standpoint that considers only the visual system itself, which seems to me a perfectly legitimate perspective to adopt for many scientific purposes, that might be all that one

⁵ For the record, I am convinced that the other method of predicting unique hues that I considered in section 5 of my 2011 is altogether a nonstarter. I noted its shortcoming when it was introduced and nothing since then has led me to think it can be rehabilitated.

is interested in saying about unique yellow, at least when it comes to low-level operations. One does not have to be a content externalist, however, to recognize that some of our explanatory interests will be satisfied only when we have in place a fully developed account of all the steps from the distal stimulus to the conscious percept, as well as that percept's role in our cognitive economy. Part of such an effort will plausibly include an attempt to rationalize, at least as far as possible, aspects of neural processing or conscious experience in terms of environmental features. My own view, which largely agrees with that of Frances Egan (2010) about such matters, is that a great deal of useful and productive scientific research on perception can and does occur without taking up issues regarding the relationship between the structure of the environment and everything that goes on once light strikes the retina. Nonetheless, such questions are often what get scientific inquiry about perception off the ground and attempts to answer them may prove helpful to what are otherwise “internalist” endeavors. At any rate, I do not see my emphasis on visual processing and Broackes' on the environment as rival alternatives and am glad to co-opt his discussion of unique blue and unique yellow into my own account.

I close with the admission that I am not sure exactly what should be said about either environmental factors or visual processes that might set the locations of unique green and, especially, unique red. While I am still trying to figure out where to begin with unique red, I at least have some sense of what might be tried out for unique green. The method inspired by Jameson & D'Andrade tallies with the 510-520 nm peak of the bimodal distribution for unique green (the prediction falls between 510 and 515 nm), but is silent about the peak near 540 nm. In my 2011, I suggest that this bimodal distribution might be linked to the fact that ubiquitous plant surfaces, such as leaves and grasses, plot as green-yellow, clustering between 550 and 560 nm in the R&C model. For some perceivers, intense exposure to such stimuli might pull their unique

green position toward green-yellow. That might involve either changes in visual processing or the criteria subjects use in choosing a focal or ideal green. The unique green settings of other perceivers, for unknown reasons, do not show such an effect and instead align with the same low-level facts about the ratio of chromatic to achromatic sensitivity that are relevant to unique blue and unique yellow. Plausibly, this suggestion could be combined with what Broackes notes about the observations from Mollon & Jordan (1997) regarding leaf coloration, intersubjective variation in iris pigmentation, and unique green settings. That, like several of the ideas discussed above and in my 2011, remains to be explored in future research.

4. References

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