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Continuing Commentary

Commentary on **Alex Byrne & David R. Hilbert (2003). Color realism and color science. BBS 26(1):3–21.**

Abstract of the original article: The target article is an attempt to make some progress on the problem of color realism. Are objects colored? And what is the nature of the color properties? We defend the view that physical objects (for instance, tomatoes, radishes, and rubies) are colored, and that colors are physical properties, specifically, types of reflectance. This is probably a minority opinion, at least among color scientists. Textbooks frequently claim that physical objects are not colored, and that the colors are "subjective" or "in the mind." The article has two other purposes: First, to introduce an interdisciplinary audience to some distinctively philosophical tools that are useful in tackling the problem of color realism and, second, to clarify the various positions and central arguments in the debate.

The first part explains the problem of color realism and makes some useful distinctions. These distinctions are then used to expose various confusions that often prevent people from seeing that the issues are genuine and difficult, and that the problem of color realism ought to be of interest to anyone working in the field of color science. The second part explains the various leading answers to the problem of color realism, and (briefly) argues that all views other than our own have serious difficulties or are unmotivated. The third part explains and motivates our own view, that colors are types of reflectances, and defends it against objections made in the recent literature that are often taken as fatal.

Colors as explainers?

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Abstract: Byrne & Hilbert (B&H) argue that colors are reflectance properties of objects. They also claim that a necessary condition for something's being a color is that it causally explain – or be causally implicated in the explanation of – our perceptions of color. I argue that these two positions are in conflict.

Byrne & Hilbert (2003; henceforth B&H) argue for reflectance physicalism, the view that colors are reflectance properties of objects. A necessary condition for something's being a color, in their opinion, is that it should causally explain – or be causally implicated in the explanation of – our perceptions of color.[1] Their view is that "[a]ny plausible version of physicalism will identify the colors with physical properties implicated in the causal process that underlies the perception of color" (sect. 3.1). So, according to B&H, if I come to believe that there is a red object in front of me, the color of that object must causally explain my perceptual belief that the object is red. Call this the *Causal Explanation Condition.*

The problem of metamerism is that quite different reflectance properties, given suitable illumination, can match in color. This is a problem for reflectance physicalism because it strongly suggests that color cannot be identified with any specific reflectance property. In response to this problem, B&H suggest that we would do better to identify colors with "reflectance-types (or sets of reflectances) rather than with the specific reflectances themselves" (sect. 3.1.1).[2]

Let us suppose that R1, R2, and R3 are reflectances that match in color – say red – under some suitable illumination. Then, according to B&H, the color red is to be identified with the set {R1, R2, R3}. Let us also suppose that I am looking at an object O, the surface of which has reflectance property R2, and that as a result I come to believe that O is red. It might be thought that on B&H's view O's color is what causally explains my perceptual belief that O is red. But appearances can be deceiving. For what property causally explains my perception of redness? Arguably R2. After all, since that is the reflectance property that O has, on B&H's view that must be the property of O that causally explains my perception of red. But according to B&H, R2 is not the color red; rather, the color red is the set {R1, R2, R3}. So it seems that if the color red is identified with a set of reflectance properties, then the color red cannot be what causally explains our perceptions of redness. So given the Causal Explanation Condition, the color red cannot be identified with a set of reflectances. Similar remarks apply to the other colors.[3]

Objection: If O has the reflectance property R2, then trivially O has either R1 or R2 or R3, and so trivially O is red. So if O has R2, then the color red does causally explain my perceptual belief that O is red. But this won't do. For consider: If O has reflectance property R2, then trivially it has either R2 or B1 or B3 – where B1 and B3 are, we shall suppose, the unique reflectance properties that match with respect to the color blue under suitable illumination – and so trivially is *red or blue.* So, if O has R2, then the color red or blue causally explains my perceptual belief that O is red. But this is false. It is not O's having the color red or blue that causally explains my perceptual belief that O is red; it's O's having the color red. So the preceding argument for the claim that the color red causally explains my perception of redness must be rejected.

Suppose, then, we opt instead for the view that colors are reflectance types rather than sets of reflectances. Then, if the color red is identified with the property of having some reflectance property that plays a certain specified role – call it the "R-role" – in the production of perceptual beliefs about redness it might be thought that the present objection fails. But again, suppose I am looking at an object, the surface of which has reflectance property R2, and I thereby come to believe that the object is red. What property causally explains my perception of redness? Again, arguably, it is R2. But on the present proposal we have:

1. The color red = the property of having some reflectance property that plays the R-role.

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2. R2 = a reflectance property that plays the R-role

So again, R2 is not the color red. But if what causes my perceptions of redness is R2, then once more, the color red fails to cause my perceptions of redness. So again, given the Causal Explanation Condition, the color red cannot be identified with a reflectance type.

It might be thought that there is an easy way around these objections. Say that an object O's color causally explains our perception of color at a time *t* if, first, O has one of the properties in the relevant set of reflectances – or a property playing the appropriate role – and second, that this particular reflectance property causally explains our perception of color at *t*. And take our object O again. By hypothesis, O has the reflectance property R2. And we are assuming that R2 causally explains our perception of redness at *t*. So it might seem that O's color – namely, red – causally explains our perception of redness after all. What is wrong with this view?

What is wrong is that whatever else R2 is, it isn't the color red, at least not on B&H's view. So it is hard to see how it can be the case *both* that R2 is what causally explains our perceptions of redness at *t, and* that the color red is what causally explains our perceptions of redness at *t*. We can certainly say, of course, that O appears to be red because it has the reflectance property R2, but this is not the same as saying that O appears to be red because it is red.

The upshot is that B&H's identification of colors with sets of reflectance properties, or types of reflectances, sits badly with the Causal Explanation Condition. If B&H insist that colors are sets of reflectance properties, then it isn't clear that colors can causally explain perceptual beliefs about color. And if B&H insist that colors must causally explain perceptual beliefs about color, then it isn't clear that colors can be identified with sets of reflectance properties.

ACKNOWLEDGMENTS

I would like to thank Alex Byrne for his support and encouragement.

NOTES

1. There is clearly a difference between the concept of causal explanation and that of causal implication. For while it is true that, if X causally explains Y, then X is causally implicated in the explanation of Y, the converse fails: X could be causally implicated in the explanation of Y without causally explaining Y. For ease of exposition, however, I will simply talk about causal explanation in what follows. What I say about causal explanation applies to causal implication as well.

2. Although it is not clear to me whether B&H intend these to be distinct claims, for present purposes I will treat them separately.

3. This sort of worry is by no means unique to philosophical discussions of color. To take a familiar example, some philosophers of mind hold the view that the property of being in pain is a disjunctive property. In humans, the property of being in pain is the physical property Ph; in dogs, the physical property Pd; in Martians, the physical property Pm; and so on. So pain turns out to be the disjunctive property Ph or Pd or Pm, or the set {Ph, Pd, Pm}. And the causal worry remains: It is not my being in pain that is causing my headache, but my having Ph. For more on this sort of worry see the papers collected in Kim (1993).

Do opponent process theories help physicalism about color?

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Abstract: Byrne & Hilbert (B&H) give some excellent replies to the objections to realism about color. However, the particular form of realism they propose, based on opponent processing, prompts several challenges. Why characterize a color by its tendency to produce an intermediate brain signal, rather than in terms of the final effect – either a perception or a neural substrate for it? At the level of the retina, and even of the cortex, there are processes that partly parallel the structure of color experience; but the correspondence is not exact. Must we assume that there is any place in the brain where an exact structural correspondence is found? At the level of psychophysical functioning, there is indeed opponency; but it is not clear that this gives us the kind of type-reduction that B&H want.

Byrne & Hilbert (2003; henceforth B&H) want to use opponent process theory to provide an answer to the question "what is redness?," in such a way as to make it easy to respond to some standard challenges to physicalist reductions of color. One important challenge is that these reductions cannot properly capture such facts as "purple is more similar to blue than to green," because the physical correlate for purple might well not be physically more similar to the physical correlate for blue than it is to the physical correlate for green. Another challenge is that reductions do not capture the structure of colors, or, for example, the fact that red is a unique hue and orange is not; for there's surely no sense in which (the physical correlate for) orange is, in physical terms, composite, whereas (the correlate for) red is not.

B&H do excellent work, I think, in clearing the ground for a realist view of color. But I am not sure that once the objections are removed, the kind of physicalist reduction they propose is going to work. My concerns are basically these: In the retina and beyond, there is plenty of evidence of some kind of physiological opponent processing. If we follow one recent report (Dacey & Lee 1999), midget bipolar cells show something like (+R -G) opponency in the form of an (L-M) signal; and the bistratified ganglion cells show something like (+B -Y) opponency in the form of an S-(L+M) signal. However, no one thinks that these cells constitute the seat of visual consciousness; and only a little reflection shows that the exact form of these signal functions does not correspond at all exactly to the perception of redness versus greenness and the perception of blueness versus yellowness. (In particular, a simple (L-M) signal will never be significantly positive in the violet part of the spectrum – whereas of course the violet hues are actually seen as reddish. One solution would be to have some negative input from S cones to the (+R -G) function; but this has not, I think, been reported in the physiology of the retina.) What is more, it would be more or less make-believe to suppose that the actual weights on the L, M, and S inputs to the opponent processes in post-retinal coding are such as to yield functions reaching their maxima and minima at just the points where perceivers register maximum degrees of redness and other unique hues.

There is a form of opponent process theory that is in much more direct correspondence with perceptual experience: The one developed by Hurvich and Jameson (1955; cf. also Jameson & Hurvich 1955). But their chromatic response functions are contributions to psychophysics, not physiology; they describe the performance of human subjects performing a hue-cancellation task – mixing with sample lights varying quantities of standard lights (red or green, yellow or blue), in order to cancel any appearance in the sample of the complementary hue (i.e., green or red, blue or yellow).

The details of this theory are interesting. But it is questionable whether it provides much help to the kind of physicalism B&H seem to want to support. Suppose we take it that reddishness is that kind of reflectivity which gives rise to "reddish" reactions (i.e., to a positive value of the (+R -G) function). Is this any better than saying that reddishness is that kind of reflectivity which gives rise to experiences of reddishness? After all, what Hurvich and Jameson effectively mean by saying that a light has a positive value on the (+R -G) function is that it needs some green to be mixed with it in order to lose its appearance of reddishness. The "chromatic response functions" simply record where an appearance of reddishness (and so on) is found, and how much of a complementary color is needed to cancel it; they don't tell us what the reddishness, or the experience of it, consists in.[1] One might perhaps take the Hurvich and Jameson chromatic response functions as actual descriptions of the output of physiological processes somewhere in the brain; but then one would be committed to finding, not processes with an output something like (L-M) and (L+M)-S (as is plausible), but instead processes with the outputs 1.6645L −

2.2301M + 0.3676S and 0.3410L + 0.0615M − 0.7130S.[2] I don't think many people feel confident that such brain processes exist.

We can identify a number of different stages in the process of vision. A red thing, illuminated by a standard illuminant will send out, as a reflection,

(a) a certain kind of light, which, in turn, will cause changes in (among other things):

(b) the cones of the retina,

(c) the bipolar and ganglion cells of the retina,

(d) the cells of the LGN, and

(e) the cells of the cortex, for example, in areas V1, V2, and (depending on your guide), V4 or V8; and there will of course be

(f) an experience of the thing's looking red.

No doubt the occurrence of (f) will in some sense be nothing over and above the occurrence of the various physiological changes in the organism – at any rate, I shall assume some such physicalism as common ground. B&H propose that redness is a certain kind of reflectivity, defined in terms of a certain kind of effect it produces. But what kind? B&H see the difficulty of finding at level (a) or (b) a straightforward correlate – a common effect, distinctive of all red or reddish things – such as would provide a quick reply to the objections they consider. They therefore look further along in the chain of processing and cite opponent processes, apparently encouraged by parallels between the structure of apparent color space and the structure of physiological processing at certain levels. I worry, however, that the physiological evidence suggests that there may well not be any exact structural parallel at other physiological levels either; so the question arises, whether a plausible physicalism should set its sights on finding one. My own preference would be for a less simple physicalism that does not expect exact or simple correlates in the first place.

In B&H's proposal, reddishness is (approximately) a kind of reflectivity which, when present in a surface illuminated with equal-energy light, leads to a particular kind of opponent signal, for example, a positive (L-M) signal. (See target article, sect. 3.2.3: "an object has some value of **R** if and only if, under an equal energy illuminant, it would reflect light with a greater L-intensity than M-intensity – the greater the difference, the higher the value of **R.**") This makes reddishness something like a kind of reflectivity characterized by its disposition to cause a particular brain process. It is unclear exactly which level of brain processing the authors are talking of, but they seem to be interested in an L-M "signal" which goes on to produce a "red response" (see sect. 3.2.2, paras. 2 and 3), so I presume they are talking of retinal and post-retinal processing. But in that case, why do they characterize reddish type of reflectivity by its tendency to produce the L-M signal, rather than by its tendency to produce the "red response"? Why concentrate on the intermediate, rather than the final product? Why not, instead, characterize reddishness by its tendency to produce a particular experience (at level [f]); or at some substrate of the experience, for example, at the level of the cortex, (e)?

Why should we expect opponency at levels (c) or (d) to be important in the definition of color? Any process at the level of the lateral geniculate nucleus (LGN), for example, which we single out in a philosophical theory of what redness is, will surely be singled out precisely because of its significance at level (e) or (f); that is, because it is a process that leads to the perception of redness. Any structuring exhibited at that level will be interesting to us as physiologists and neurologists, but it may or may not be preserved in later processing, and it will surely not be crucial in telling us what redness or reddishness consists in, or what is the basis for whatever pattern and unity exists among red and reddish things. For that, we would expect to look further on.

It would be certainly be nice to discover at level (e) a structure of opponent processing that corresponded directly to the structure of color appearance. But in the cortex, there is a lot of evidence of *non*opponent processing. (See, e.g., Lennie 1999, p. 240: "The chromatic preferences among V1 neurons plainly do not fall into four neat classes of the sorts postulated to account for psychophysical observations." Krauskopf 1999, p. 308, talks of "multiple mechanisms and . . . at least partially, independent mechanisms". See also, e.g., Webster & Mollon 1991.) And suppose it turned out that in the cortex there were some processes (let us put them in class [e1]) that were opponent (and specially tuned to Red vs. Green, Blue vs. Yellow), and other processes (in class [e2]) that were not; would we then want a philosophical theory that linked reddishness definitively with the production of responses of type (e1), and ignored the nonopponent responses of type (e2)? Opponent processing may seem to solve easily the particular problems of a simple identity theory of color, but at a price – of potentially ignoring a large part of what constitutes the experience of color.

We should ask what really underlies the belief in a form of physiological opponency exactly corresponding to the structure of color space manifested in different ways in psychophysics and ordinary experience. It is, I think, the assumption that the structure of experience has to be simply parallel to the structure of brain processing. I find it hard to see that this is necessary to any good physicalism. It is rather as if we expected to find a little picture in the brain where the image we experience appeared, only in a very simple code. We would surely be sceptical of that idea, as we would be of an argument that claimed, for example, that, because there is a central unity of consciousness, brain processing can therefore not be distributed over an area. No doubt B&H would be sceptical of such views too when put so baldly; but then the question arises, was it necessary for them to link physicalism with opponency in the way they do?[3]

It is worth recognizing that B&H distinguish their "magnitude proposal" (namely, that "visual experience represents objects as having proportions of hue-magnitudes," sect. 3.2.1) from opponent process theory: "there is nothing in the magnitude proposal that requires the truth of opponent-process theory, let alone the simplified version of it we will use for the purposes of illustration" (sect. 3.2.2). I agree with this, and count myself a supporter of something like their magnitude proposal. However, while the two doctrines are separate, some variety of both seems to be required for B&H's rejoinder to the two key objections to physicalism. They say "we must show" that there is "a physicalist account of the hue-magnitudes" (sect. 3.2.3), and it is opponent process theory that is supposed to provide that physicalist account. They are, of course, well aware of shortcomings in the simple opponent-process model they describe; but they clearly believe that if that model fails, then a more complex model can simply be put in its place. And that is where I have my doubts. No doubt there is some revised physiological story to be told. But as far as I can see, there is no need for the structure of color experience to be exactly mapped in the physiology: It might well be, for example, that the lines joining unique red to unique green, and unique blue to unique yellow were cardinal directions in phenomenal experience, but that slightly different lines formed the cardinal directions in physiological space.

Perhaps B&H would accept even this possibility. It is unclear exactly what is essential and what accidental in their use of the simplified opponent process model. Does the claim "purple is more similar to blue than to green" count as true simply because of the phenomenal structure of color space? Or do the authors count it as true because they believe that that phenomenal structure is also mimicked at the physiological level? (In which case the "purple response" – if captured at the "right level" – really is more similar to the "blue response" than to the "green response.") Perhaps they intend only the first point. If so, then the opponent theory is, I think, an unnecessary detour. It is also unclear that what really "joins together" the various reflectances which all count as reddish is their production of degrees of some *physiological* response, rather than their production of the *experience* of reddishness in various degrees. Any physiological responses we fix upon will be singled out, after all, solely because of their role in producing or underlying the experiences. If, as I suspect, B&H really want to maintain the second point as well – and claim a precise parallelism between the structure of phenomenal color space and that of the physiological processes underlying it – then I think this is a dan-

gerous dogma, unsupported by the present empirical evidence. Either way, the idea of opponent processing, though it has some virtues in physiology and other virtues in psychophysics, is not the solution to the problems of physicalism.

NOTES

1. One could raise further questions about the psychophysical opponent theory. It is all too easy to suppose that it provides ready measures, for example, for the degree of reddishness or greenishness. But is the *amount of red light needed to cancel* the greenness of a stimulus really an accurate measure of its degree of *greenness*? There may also be a disparity between the results of cancellation and direct matching methods (see, e.g., Ingling et al. 1978; Wyszecki & Styles 1982, pp. 652–54). Further, a quick look at the Hurvich and Jameson color response functions tells us that the *non*-unique spectral stimuli often have higher values on those functions than the unique stimuli do. A yellowish green of 525 nm would count as "greener" than a unique green of 498 nm – because, indeed, it needs more red to desaturate it and cancel any apparent greenness. We might well decide to adopt *amount of red for cancellation* as our criterion in the future for *degree of greenness;* but then it would be unclear that we were talking of the same old property as when we judged degree of greenness by our ordinary standards. Of course, there is no requirement for opponent process theorists to identify degree of redness, and so on, with values of these particular functions. But B&H are indeed attracted by a fairly direct identification, linking **R** (for reddishness) with the value of the (L-M) function, for example (sect. 3.2.3).

2. Hurvich and Jameson define chromatic response functions for the CIE 1931 standard observer as $1.0x(\lambda)$-$1.0y(\mathrm{l})$ and $0.4y(\mathrm{l})$-$0.4z(\mathrm{l})$. Re-expressing these functions in terms of the König cone response fundamentals (here called L, M, and S), we get the functions I specify here. (These are my own calculations from the standard definitions of the König fundamentals in Wyszecki & Styles 1982, pp. 604–608; see also pp. 457–58, 643–44.) Of course, the CIE data have their own problems, and the König fundamentals inherit them; using newer and better cone fundamentals, we could calculate corresponding hue cancellation functions; they would be equally unlikely to give the output of particular brain processes.

3. As evidence of B&H's drive to find a parallelism, one might cite their rejection of the CIE system, because CIE coordinates "do not capture perceived similarity relations" (target article, sect. 3.1.1). I would submit, however, that to suppose we will find a couple of parameters of brain representations in, say, V4, that capture perceived similarity relations *better* than the CIE coordinates do, is quite unnecessary. There are indeed divergences between the CIE spaces and perceptual color space; but we should not expect a simple relation between brain representation and perceptual color space either.

Forestalling a food fight over color[1]

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Abstract: The disagreement between "color realists" and "color eliminativists" is better seen as a disagreement over pedagogical policies, about how best to satisfy lay curiosity about "what color is," than as a clash of substantive theories. To suppose otherwise is to endow the ordinary language term "color" with more authority than it can bear.

Byrne & Hilbert (2003; henceforth B&H) provide valuable clarification of the complexities – undreamt of by the layman – that make it hard to answer the question of what color is, and that often lead color scientists to say such remarkable and extravagant things. They emphasize at the outset that their issue is not just how to define the ordinary language term "color": "*The problem of color realism is like the investigation of what humans can digest, not the investigation of the folk category of food.*" (target article, sect. 1.1, para. 4), but then I am puzzled by a tension in the target article regarding the weight the authors put on our ordinary intuitions about color. The very setting of the issue as a disagreement between "color realists" and "color eliminativists" endows the everyday concept with somewhat more authority than it deserves – comparable to an imaginary debate between biologists who were "food realists" and "food eliminativists"!

On the one hand, the authors have no problem with gathering together types of reflectances that "will seem a motley jumble" (sect. 3.1.1) and they find the proposal that there are different types of colors (surface colors, volume colors, source colors) "quite unacceptable" because these different kinds of objects "*can look the same* in respect of color" (sect. 3.1.2, emphasis mine) – which they take as trumping all other considerations. They go on: "Therefore, the natural inference is that there is a *single* property that vision represents all these objects as having" (sect. 3.1.2, emphasis mine). They insist, then, on "a single property" because common intuition demands it, but that property can nevertheless be a motley jumble of types of reflectances. Are disjunctive properties good, real properties? My own metaphysical intuitions are not finely enough tuned to permit me to adjudicate the soundness of this proposal, but I don't think the issue needs to arise.

Like many philosophical projects, the target article is an exercise in definition. Is there a way to escape the dread counterexamples by adroitly adjusting the definition to meet all cases? Maybe there's always a bullet-proof definition at the end of such a quest, but only a few hardy philosophers will persist long enough to tolerate its multiple clauses. It seems to me that a better question, a more scientifically useful one, is somewhat less ambitious, more relaxed: Is there a way of defining the property of color so that it is

1. scientifically sound
2. largely consonant with everyday usage, our starting point
3. compact enough to be wieldable, teachable, explicable, usable?

There really is a use for such a definition if it can be found: as a curiosity-satisfier. It would solve what might be only pedagogical or public-relations problems, but these can be severe. We start out wondering about colors, using the everyday word as we find it, and using our everyday experience as anchor. What, we wonder, can science tell us *about all that*? A good workaday definition of color that people could agree on could be used to *answer* the naive question "What, if anything, is color?" in a way that is scientifically defensible, informative, and readily shown to be a satisfier of *that* innocent and admirable curiosity, not just the answer to some wildly different substitute question. And if it turns out that no definition of color can meet these contingently imposed requirements, then we can explain why the ordinary "folk category" of color doesn't happen to pick out anything real, and be "realists" about whatever we put in its stead. (There is no need for biologists to form realist and anti-realist camps about what is digestible, after all.)

B&H show that various color scientists are not "color realists" by finding telling quotations which invite or demand that interpretation. I wonder if these scientists would be better described as just a little too impatient with this pedagogical task, willing to strike a compromise bargain, oversimplifying or exaggerating in order to fend off the most serious and ubiquitous confusions about color. In other words, I suspect that these killer quotations should be viewed as less than successful stabs at public education, not expressions of considered "theories" of the ontological status of colors. But perhaps I am overusing the principle of charity.

Vision involving spectral differences (not just luminance differences) turns out to be wonderfully strange, and I think there has been enough astute canvassing of the prospects so that we can be pretty sure that *any* theory "of color" will have some counterintuitive bite-the-bullet implications in one corner or another; but I am surprised at one particular bullet B&H decide to bite: unknowable color facts about, for instance, which chip is (really) unique green. For this to be a fact, there has to be some standard of correctness which isn't just majority rule or something like it. But it isn't just that we don't yet know any such standard; we have good reason to believe that there couldn't be one, any more than there could be an unknowable fact about the *correct* pronunciation of the word "controversy" (who really has it right, the Brits or the Americans?). B&H have a clear understanding, it seems, of the coevolutionary coordination of color vision and reflectance

properties, and it would seem to *follow from this* that the "ideal" of a unique green independent of (human) physiology is as indefinable as the ideal of a correct pronunciation of a word independent of human social practices. Color isn't like distance or horizontality, for the simple reason that distance and horizontality properties didn't co-evolve with spatial vision. Yes, people can make mistakes about unique green, and about how "controversy" is pronounced, but that doesn't mean that there is a people-independent way of fixing what is right in these cases.

NOTE

1. The Editor apologizes to the author for mislaying this commentary (submitted June 27, 2002) and thanks him for taking the trouble to resubmit it for continuing commentary.

Physicalism without unknowable colors

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Abstract: Byrne & Hilbert (2003; henceforth B&H) do not adequately explain how it is that phenomenal colors are physical colors, as their physicalism claims. This explanation requires more characterization of the relationship between the epistemology and nature of color than B&H provide. With this characterization, we can see that a physicalist need not accept unknowable color facts, as B&H do.

Byrne & Hilbert's (B&H's) physicalism relies on drawing a sharp distinction between the epistemology of color and the nature of color, in particular between conditions of perceptual access to color (including properties of visual experiences and viewing conditions), and the physical properties of objects, which constitutes color (target article, sect. 1.3.3, para. 2). Also, B&H distinguish physical color from phenomenal color ("colors-as-we-see-them") (sect. 1.3.4, para. 4). Furthermore, according to their physicalism, phenomenal colors are physical properties (sect. 1.3.4, para. 5). However, having distinguished between physical color and phenomenal color, they do not adequately explain how it is that phenomenal colors are physical colors. This explanation requires more characterization of the relationship between the epistemology and the nature of color than B&H provide. While I am in broad agreement with B&H's physicalism, filling in this characterization allows us to see how a physicalist need not accept unknowable color facts the way B&H do.

Drawing a distinction between phenomenal color and physical colors seems to invite identifying phenomenal colors with some property other than the physical properties of objects. This problem for physicalism is made plain by the fact that phenomenal colors are relative to the perceiver and viewing conditions in a way that physical properties of objects are not. Physicalism generally cannot account for differences in an object's phenomenal colors in terms of differences in physical properties of the object, because most differences in an object's phenomenal color – such as those due to differences in perceiver type, for example, color blind as opposed to standard human perceivers – are not due to differences in these physical properties. Since an object's phenomenal color can vary while its physical properties remain unchanged, it seems that phenomenal colors are not physical properties. Rather, the physical properties of objects seem to be merely the stimulus for the visual experience of color, and the nature of color must involve properties apart from this physical stimulus. Relying on this point, dispositionalists and subjectivists hold that the properties of visual experiences are constitutive of phenomenal color.

However, the physicalist accounts for differences in an object's phenomenal color, such as those due to differences in perceiver type, in terms of differences in perceptual access to the same physical color (e.g., a surface reflectance property). The sharp distinction between the epistemology and the nature of color indicates that such access conditions are not part of the nature of color. However, the physicalist can positively characterize the relationship between the epistemology and the nature of color as follows. The physicalist can claim that a description of the conditions of perceptual access is a reference fixer – that is, this description picks out the physical properties which constitute colors in terms of nonconstitutive properties. Since conditions of perceptual access include properties of visual experiences of color as well as viewing conditions, this description includes a description of properties of visual experiences (in terms of cone activation and later stage gauging of cone activation by opponent processes) as well as lighting, visual distance and angle, and other perceptual access conditions. The physicalist can, in turn, use this characterization of the relationship between epistemology and nature to explain how phenomenal colors are physical colors despite the relativity of color to perceptual access. Given any phenomenal color, we can describe conditions of perceptual access and take this description to be a reference fixer for a physical color.

In sum, dispositionalists and subjectivists propose that we account for differences in phenomenal color in terms of differences in perceptual access and incorporate properties of perceptual access – in particular, properties of visual experiences – into the nature of color. Physicalism, by contrast, rejects this incorporation of the epistemology into the nature of color, and draws a sharp distinction between conditions of perceptual access and the physical properties of objects which constitute colors.

The physicalist can support this sharp distinction by pointing to phenomenal spatial properties. As B&H note (sect. 3.4, paras. 8–9), there are cases where it is clear that we must account for differences in phenomenal spatial properties in terms of differences in perceptual access – and, in particular, in terms of the properties of visual experiences. Nevertheless, it is plausible that any phenomenal spatial property is a physical spatial property. Furthermore, we can characterize the relationship between the epistemology and the nature of spatial properties in terms of reference fixing descriptions. Given any phenomenal spatial property, we can – as with phenomenal colors – describe conditions of perceptual access and take this description to be a reference fixer for a physical property.

Moreover, with the reference fixing characterization of the relationship between the epistemology and the nature of color, a physicalist can avoid B&H's acceptance of unknowable color facts (target article, Note 50). The relativity of phenomenal color to perceiver and viewing condition raises the question of what any given object's true or veridical color is. Because there is no test which is independent of perceptual access for determining an answer, this amounts to asking: What perceptual conditions provide access to the object's veridical color? For B&H, the fact that there is no independent test, along with their assertion that the object has a single true determinate color, leads them to accept that there are unknowable color facts.

Yet physicalism is consistent with a pragmatic account of veridical color, in particular an account holding that an object's veridical color is relative to favored conditions of perceptual access (e.g., standard perceivers in standard viewing conditions). All that is required for physicalism is that, given any phenomenal color, we can describe conditions of perceptual access and take this description to be a reference fixer for a physical color. It is open to the physicalist to hold that the perceptual conditions that provide access to an object's veridical color are determined pragmatically (see Ross 2000 for an extended discussion). This alternative pragmatic account of veridical color does entail the consequence that an object may have different veridical colors, depending on what perceptual access conditions are favored. But the pragmatic account, by characterizing veridical color in terms of favored conditions of perceptual access, has the advantage of avoiding unknowable color facts.

The right kind of content for a physicalist about color

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Abstract: Color experiences have representational content. But this content need not include a propositional component, particularly for reflectance physicalists such as Byrne & Hilbert (B&H). Insisting on such content gives primacy to language where it is not required, and makes the extension of the argument to nonhuman animals suspect.

As a materialist and a color realist, I find much in Byrne & Hilbert's (2003; henceforth B&H) article to agree with. I do find one issue to be unsatisfying, however. This is their view of the representational content of color experience.

The particular problem with B&H's view is that part of the content of color visual experience is a proposition. For example, according to the authors, when I reach to pick a lemon from the tree in daylight, part of the content of my visual experience is a proposition something to the effect that "there is a yellow object hanging from the branch." The problem is that there is no need of such content for a reflectance-based view of physicalism about color. Indeed, insisting on such content gives primacy to language where it is not required, and makes the extension of the argument to nonhuman animals suspect.

A physicalist about color does not need propositional content in order to account for the content of experience. When experiencing a color, the physical property which is identified with the color – reflectance – is all that is needed for the content of an experience of that color (Dretske 1995; Tye 1995). Stripped to its most basic form, when I experience the yellow of the lemon, I experience the physical property yellow (the reflectance). The content of my experience is this property. There are thus only two physical properties in play here: the experience in my head, and the property yellow. I experience yellow; yellow is the content of my experience (Skokowski 2002). No propositional content is called for, since the property of yellow reflectance exemplified by the lemon *is* the content of the experience.

Later in their article (sect. 3.3), the authors argue for the plausibility of color vision in other animals. But surely we cannot attribute propositional content to the representational content of the color experience of other organisms. Assuming that such animals do not have language, how could propositional content ever be a part of their experiences of color? The most satisfying explanation for a reflectance physicalist surely must be that the contents of color experiences for nonhuman animals are real, physical colors (reflectances) in the world, not abstract objects such as propositions, which in any case are not accessible to beings without language. In my view, the authors should embrace their physicalism wholeheartedly, and accept that color contents, and experiences, are real properties of the world in their own rights. They should reject the appeal to propositional contents as a part of the content of color experience, much as they rejected the sense data view, and for similar reasons: both are epiphenomena that serve no causal or explanatory purpose for a physicalist.

Physicalism plus intentionalism equals error theory

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Abstract: Byrne & Hilbert (B&H) combine physicalism about color with intentionalism about color experience. I argue that this combination leads to an "error theory" about color experience, that is, the doctrine that color experience is systematically illusory. But this conflicts with another aspect of B&H's position, namely, the denial of error theory.

Part of what is appealing and distinctive about the position advanced by Byrne & Hilbert (2003; henceforth B&H) is the combination of a certain sort of physicalism about color with intentionalism about color experience. But one can argue that this combination leads naturally to an "error theory" about color experience! If this argument is sound, we may conclude, contrary to the main thrust of B&H's article, that there is an element of truth – I do not say it is the whole truth – in the Gallillean view that color is an illusion.

Suppose I am looking at a tomato in good light. In that case, as B&H make clear, the world seems to me to be a certain way. And, as they also point out, I may go on to ask whether the world is in fact the way that my experience presents it as being. This question implicitly distinguishes two possible worlds, without prejudicing whether we may ultimately identify or differentiate between them: (1) the visual world, the world that is presented to me in visual experience, and (2) the actual world, the world as it really is. From this point of view, the error theory about color experience says that, in a certain systematic sense having to do with color, the visual world is different from the actual world. So when I say that intentionalism and physicalism combine to yield the error theory, I mean that, if these positions are both true, the visual and actual worlds *are* different in this sense – that is, in relation to color (rather than, e.g., shape) and the difference is systematic (rather than haphazard).

But what is the argument for this conclusion? Well, to say that physicalism about color is true, is to say that it is true in the actual world. This gives us our (truistic) first premise:

P1. If physicalism is true, physicalism is true at the actual world.

Of course, to say that physicalism is true in the actual world is not to say that it is true in the visual world, in view of the possibility that the two worlds diverge. This then raises the question: What theory of color is true for/in the visual world? From the perspective of intentionalism, this last question is about which theory of color is best suited to tell the truth, not about the nature of colored objects, but about the phenomenology of color experience.

There would seem to be three possibilities here, corresponding to the three (realist) theories of color distinguished by B&H. (I regard the ecological view as a version of dispositionalism and so will not discuss it explicitly.) The first possibility is that physicalism is true for the visual world. But this is extremely implausible. To say that physicalism is true for or in the visual world is to say that the physical nature of colors – assuming them to have a physical nature – is evident to one simply on the basis of experience; or, at any rate, that it could become evident given only experience and sufficient reflection and suggestion. But even physicalists – those who think physicalism is true in the actual world – don't think that the physical nature of colors is evident in this sense. So physicalism is not true at/for/in the visual world.

The second possibility is that dispositionalism is true for/in the visual world. But this too is implausible, for related reasons. To say that dispositionalism is true for/in the visual world is to say that the dispositional nature of colors – assuming them to have a dispositional nature – is evident to one simply on the basis of experience, or that it could become evident given only experience and sufficient reflection and suggestion. But even dispositionalists –

those who think dispositionalism is true at the actual world – don't think, in general, that the dispostional is evident in this sense. (Langsam 2000 is a counterexample to this generalization; but see Byrne 2001 for criticism.) So dispositionalism too is not true for/in the visual world.

The third possibility is that primitivism is true for/in the visual world. This is in fact an extremely plausible thesis. Even physicalists about color often say things which suggest – in our terms – that primitivism is true at the visual world:

> [it] is surely right that, for example, the sensible quality of redness looks to be an intrinsic (non-relational) property of certain surfaces. Phenomenally, the primary and secondary cannot be separated . . . [T]he secondary qualities appear as lacking in "grain" . . . So much for the way it seems. (Armstrong 1987 in Byrne & Hilbert 1997, pp. 36–37)

If we suppose that primitivism is true at the visual world, we now have our second premise, which is intended to be true on the basis of phenomenology:

> P2. If intentionalism is true, primitivism is true in/for the visual world.

If we assume in addition that the truth of primitivism in a world excludes the truth of physicalism in that world (and vice versa), it follows from P1 and P2 that the visual world does not coincide with the actual world. But that is simply to say that the error theory is true.

This argument refutes neither physicalism about color nor intentionalism about color experience, or their conjunction. It is open to physicalists and intentionalists to say that color experience is misleading in various ways (e.g., Thau 2002). But B&H are intentionalists and physicalists who say that color experience is not misleading; indeed, for them, color realism is true for that very reason. In sum, their color realism stands in conflict with their physicalism about color and their intentionalism about color experience.

Authors' Response

Color realism revisited

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Abstract: Our reply is in four parts. The first part, R1, addresses objections to our claim that there might be "unknowable" color facts. The second part, R2, discusses the use we make of opponent process theory. The third part, R3, examines the question of whether colors are causes. The fourth part, R4, takes up some issues concerning the content of visual experience.

Our target article had three aims: (1) to explain clearly the structure of the debate about color realism; (2) to introduce an interdisciplinary audience to the way philosophers have thought about the issue; (3) to argue that colors are certain sorts of physical properties ("productances").

We are very grateful to the commentators in this continuing commentary for their criticism and constructive suggestions.

R1. Ordinary intuitions and unknowable color facts

As **Dennett** notes, the target article emphasizes that the problem of color realism is not about color words, or the folk category of color. As we conceive it, the problem concerns certain properties that are represented by the human visual system and those of a variety of other animals. However, Dennett thinks that our methodology does not fit happily with the touted "scientific" nature of the problem: We place too much weight, he claims, on "ordinary intuitions about color."

But, in fact, we do not place any weight on ordinary intuitions about color. (**Dennett** himself seems to think that ordinary intuitions should be accorded *some* weight: An account of color should be "largely consonant with everyday usage." We disagree.) Dennett's one example of our alleged reliance on ordinary intuitions is our rejection of the idea that there are different kinds of colors: surface, volume, and illuminant (see sect. 3.1.2 of the target article). Although the ordinary person might well find this idea odd – tomatoes, glasses of Burgundy, and stoplights are all red! – this was not our reason for rejecting it. Rather, our objection was that surfaces, volumes, and illuminants can all look the same in respect of color – a fact which might be revealed by training an animal to press a bar in the presence of a type of light, and then noting that the animal generalizes the rule to certain surfaces and volumes. This fact is best explained, we said, by supposing that there is a common property visually represented. Whether or not we are right about this, the argument certainly does not make any appeal to ordinary intuitions.

Dennett suggests that our claim that it may be unknowable whether a chip is unique green is "counterintuitive." He might mean by this that ordinary intuition rules out such a possibility (thus implicitly accusing us of arbitrarily picking and choosing between intuitions), but we doubt it, because surely ordinary intuition has no clear opinion on the matter. In any case, whether or not Dennett himself is resting any weight on ordinary intuition here, he offers another consideration entirely. Dennett claims that the conclusion that there are no unknowable color facts follows from the "coevolutionary coordination of color vision and reflectance properties," and obviously this argument does not appeal to everyday usage or the opinions of the folk.

Dennett does not spell out his argument in any detail; still, it is instructive to set out an argument that is naturally suggested by his remarks. (We emphasize that we are not attributing this argument to Dennett. See also Dennett 1991, pp. 375–83, which we lack space to discuss.) (1) Evolution fine-tuned the colors of certain fruits so they were readily visible to certain fruit eaters, and evolution also fine-tuned the fruit eaters' color vision (in particular, the spectral sensitivities of their cone pigments) to detect the colors of the fruits (see Regan et al. 2001). Hence: (2) in normal conditions the fruit eaters will correctly perceive the colors of fruits. Hence: (3) in normal conditions the fruit eaters will correctly perceive the colors of things generally. Hence: (4) our claim that most people misperceive unique green chips in normal conditions is incorrect.

There are three main problems with this argument, apart from the fact that the coevolution hypothesis is not an established fact. First, and perhaps most seriously, on any remotely defensible elaboration of (1), it does not imply (2). The most (1) could imply is that the fruit eaters are by and large correct: any evolutionary fine-tuning of fruits and cone pigments would leave plenty of room for minor misperceptions of determinate shades, and minor variations between individuals (see sect. R2.6 of our Response to the

first round of commentaries, Byrne & Hilbert 2003). Second, (2) does not imply (3). A system specialized for infallible detection of the colors of ripe fruits might well make mistakes with other colors. (In this connection, it is worth pointing out that the coevolution hypothesis just concerns the red-green opponent system, not the yellow-blue system.) Third, (3) does not imply (4). The main empirical evidence for coevolution concerns certain species of Old World monkeys, not humans. Granting for the sake of the argument that some remote trichromatic ancestor of humans never misperceived colors in normal conditions, it by no means follows that humans have retained this capacity.

Ross offers a "pragmatic account of veridical color" which, he claims, has the "advantage" of avoiding "unknowable color facts." As explained in our first Response, we think this is no advantage at all. But, in any case, we do not think Ross's account is of the right sort. Ross holds that "an object's veridical color is relative to favored conditions of perceptual access" – these favored conditions being a matter of our interests and purposes (Ross 2000). This form of relativism is inconsistent, however, with our commitment to intentionalism (see sect. R4 below). To apply his theory to visual experience, Ross needs to assume that the phenomenal character of color experiences is independent of their content. Without such an assumption his theory, if it can be applied at all, results either in the conclusion that the apparent colors of things vary with the choice of pragmatic standards, or that we never see the true color of anything. Ross's pragmatic physicalism may be adequate as an account of some parts of color language, but it is inadequate as an account of what we see.

R2. Opponent-process theory

In his interesting commentary, **Broackes** raises a number of issues concerning opponent-process theory and its relation to our thesis that colors are represented in visual experience as proportions of hue magnitudes (sect. 3.2 of the target article). He points out – as did the previous commentators Kuehni, Jakab & McLaughlin, and Pautz in *BBS* 26(1), 2003 – that the very simple physiological model we use for illustration is empirically inadequate. As Broackes recognizes, however, we do not endorse this model; our discussion of it was merely intended to show how the hue magnitudes might be given a rough-and-ready characterization in physical terms.

Broackes thinks that our theory presupposes some version of opponent-process theory. In particular, he thinks that we "claim a precise parallelism between the structure of phenomenal color space and that of the physiological processes underlying it." There are two questions here. (1) Does our theory require the kind of straightforward mapping that Broackes describes? (2) Is there reason to doubt the existence of such mappings? Broackes rightly holds that the answer to the second question is "yes." There seems to be no evidence at present for the existence of distinct populations of cortical neurons with the characteristics required to support the various psychophysically identified color processes. In particular, there are no identified populations of cortical neurons that have the tuning required by a simple implementation of the psychophysically determined chromatic response curves (Lennie 1999).

So everything hangs on the first question, and fortunately for us this has a negative answer. **Broackes** may have been misled by the details of the last paragraph of sect. 3.2 of the target article. We chose to give an approximate specification of the magnitudes in terms of physiology, partly to make vivid the compatibility between our theory and the physiology, not because we require the connection to be made at this level. Our claim that colors are represented as proportions of hue magnitudes is simply a claim about the content of color experience, not about physiology.

R3. Color and causation

It has seemed obvious to many philosophers that color perception involves some sort of causal transaction between the colors and our visual systems. We agree – otherwise it is difficult to explain how visual systems could recover color information. (This widely held causal claim, according to us, is simply a plausible empirical assumption, not a conceptual truth about perception.) **Botterell** argues that this assumption is inconsistent with our view that the perceivable colors are to be identified with reflectance-types rather than determinate reflectances. According to Botterell, it is the determinate reflectances that are causally efficacious, not the perceivable colors themselves.

We have two points to make about **Botterell**'s argument. First, we often do in practice give causal explanations that appeal to determinables rather than determinates, even though the determinates are individually sufficient for the effect. Why did the bridge collapse? Because a truck weighing more than 10 tons was driven across it. This explanation appeals to a determinable, weighing more than 10 tons, rather than to its determinates, like weighing 10.13 tons. According to Botterell, this "explanation" must be understood as gesturing towards the genuine explanation that appeals to the precise weight of the truck. But – and this is our second point – Botterell's insistence that only the determinate property is causally efficacious is not well motivated. What matters is that the weight of the truck was too large, not which determinate weight the truck possessed (Yablo 1992). Any weight greater than 10 tons would have produced the same effect. Similarly, in the case of color, it is the type which is causally relevant to the visual effect rather than the determinate reflectance (Jackson 1996; Jackson & Pargetter 1987; Smart 1975).

R4. The content of visual experience

In the target article we used the philosophical apparatus of propositions to elaborate on the fact that color experiences can be accurate or inaccurate, depending on whether objects have the colors they visually appear to have. For example, it might visually appear to someone that there is an orange sphere on top of a blue cube. The subject's experience represents that there is an orange sphere on top of a blue cube, and this representation can be assessed for truth or falsity. In other words, the experience has propositional content – namely, the proposition that there is an orange sphere on top of a blue cube.

Skokowski challenges our claim that color experiences have propositional content, on the ground that nonlinguistic animals have color experiences but not experiences with propositional content. But we see no reason to accept Skokowski's unargued assumption that possession of a lan-

guage is a necessary condition for undergoing experiences with propositional content, or for being in mental states with propositional content. (As we read Tye & Dretske – whom Skokowski cites as fellow-travellers – they do not accept this assumption either [Dretske 1995, pp. 25–27; Tye 2000].)

Skokowski's positive view is that the color properties, not propositions, will serve as the contents of color experiences: "when I experience the yellow of the lemon, I experience the physical property yellow (reflectance)." But what is the content of the experience described two paragraphs back? According to us, it is the proposition that there is an orange sphere on top of a blue cube; according to Skokowski (apparently) the content is a set of properties and relations: {the property orange, the property blue, the property of being a sphere, . . .}. If that is right, then Skokowski cannot distinguish the experience of an orange sphere on top of blue cube from the experience of a blue sphere on top of an orange cube: both experiences have the same Skokowski-content. And since the experiences evidently do differ representationally, Skokowski's alternative account of content is inadequate.

Stoljar agrees with us that experiences have propositional content, and uses this fact as part of his ingenious argument that we have an unstable position. We endorse physicalism, intentionalism (or representationalism: see sect. 3.5 of the target article), and deny, as Stoljar puts it, "that color experience is systematically illusory." Stoljar argues that these first two claims imply that color experience *is* systematically illusory.

Intentionalism is the thesis that the phenomenology of color experience cannot vary independently of its content: If two experiences are alike in their representational content, they are alike in their phenomenology. Thus, according to the intentionalist, if you and I are "spectrally inverted" with respect to each other, the representational contents of our experiences must differ; according to the anti-intentionalist (notably Block 1990) this is not so. Although intentionalism was to a large extent left on the sidelines in the target article, it is in our view an important component of our overall theory, not least because many erroneous accounts of color are motivated by anti-intentionalism (see our discussion of **Ross** in section R1 above).

The crucial step of **Stoljar**'s argument is from intentionalism to the claim that color experience purports to inform us that colors are *sui generis* properties (*a fortiori* not reflectances or productances). Granted this step, the rest of the argument goes through: If intentionalism is true, then color experience represents that physicalism is false; hence, if intentionalism and physicalism are true, then "color experience is systematically illusory."

How troubling is this conclusion? Perhaps not very, a fact that is slightly obscured by **Stoljar**'s use of the expression "error theory" to characterize the conclusion. The error theory is usually taken to be the view that objects do not have the colors that they appear to have (see sect. 2.1 of the target article). But on this way of speaking, the error theory is not the conclusion of the argument: the "error" is that physicalism is false, not that lemons are yellow. We could live with the view that experience misleads us as to the true nature of color.

In any event, we question the crucial step: We don't see how intentionalism supports the view that color experience represents that the colors are sui generis, and so, that physicalism is false. In order for experience to represent this, it would have to represent the color properties themselves as having properties (in particular the property of being unanalysable or sui generis), and we don't think experience has this sort of content: *lemons* look yellow, but *yellowness* does not look any way at all (Byrne 2003).

There is a distinction between not representing F and representing not-F, which Armstrong illustrates by the "headless woman illusion" (Armstrong 1968; 1987). (The "headless woman" merely does not look to have a head, as opposed to looking to have no head.) The important question **Stoljar** raises is whether experience tells us that physicalism is false, as opposed to not telling us that it is true (Hilbert 1987, p. 37). Color experience, we think, is in this respect like the headless woman illusion.

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[Note: The letter 'r' before author's initials stand for CC Response Article references.]

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