

What do the colour-blind see?

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1. Colour blindness: a guide and test for theories of normal vision

What do the colour-blind see?¹ Can we tell? Does it matter? The theory of normal human colour vision is one of the triumphs of 19th-century science, emerging in Helmholtz and J. C. Maxwell in the 1850s, out of suggestions from Tobias Mayer and Thomas Young and others half a century and more earlier.² The trichromatic theory—the view that a normal perceiver can, in light mixing, match any given colour with some combination of three suitably-chosen primaries, and that this is a sign of our having three main receptor types at work in human colour vision—has constant confirmation in the fact that colour photography with three emulsions works (cp. Maxwell 1855, 136-7), as does colour television with three phosphors. The psychophysics and theory of colour measurement have been marvellously systematized;³ the 'opponent theory' first proposed by Ewald Hering as a radical alternative to Helmholtz's theory has been modified and in some manner joined up with it—notably by Hurvich and Jameson, who in the mid-1950s took the outline of G. E. Müller's model of colour vision, devised an experimental 'hue cancellation' procedure as a quantitative measure of certain features in the structure of our experienced colour space, and found a physiological counterpart for that structure in the findings of the new electrophysiology of Granit and Hartline (and Kuffler and Svaetichin), and later in the reports of De Valois and De Valois on the macaque visual system. There are other stories to be told about the physiology of the receptor pigments of the eye and the genes that code for them, about the various kinds of cell in the retina and elsewhere in the visual system, and now increasingly about colour processing in the brain. It is a topic on which psychologists, physicists, biologists, and neurophysiologists—not to mention paint manufacturers, dyers, and makers of photographic equipment—have reason to be proud and glad of the convergence of interests and views.

Colour blindness might at first seem just a peripheral abnormality. But it has often been both a guide to the nature of normal colour vision and a test application for theories of it. It promises to provide

cases where the various components of a complex process that are either hard or impossible to separate artificially are found already separated in nature.⁴ The great physicist J. C. Maxwell, having just graduated in mathematics from Cambridge in 1854, did his first research comparing the colour matching of ten normal perceivers with that of two colour-blind people (who we would now call protanopes), in order to identify the sensation that was ‘wanting’ in the colour-blind but present in the normal perceivers (see Maxwell 1855). And the colour-blind are often a test of a particular theory—in both its more empirical and more theoretical or philosophical aspects. If normal vision involves (as Helmholtz and Maxwell thought) three basic sensations, say, of red, green and violet,⁵ produced by three receptor or nerve types, then we might expect a person missing one of these types to have sensations corresponding merely to the other two. Someone missing the ‘red’ receptor or nerve type (‘red-blind’, in Helmholtz’s terms) would have sensations only of green and violet (along with the blue that results from combining them, which would—surprisingly, indeed—be experienced on looking at white things)—all at high saturation, since there would be no third colour sensation ever to desaturate the other two. On the other hand, if (as Hering believed) colour vision involves some kind of distinct red-green, yellow-blue and light-dark processes, then we might expect colour blindness to involve the loss of one of those three dimensions. And we might hope to find some kind of empirical confirmation—though it would no doubt be indirect—of one or other of these views of the colour blind, and hence in turn of some or all of Helmholtz’s or Hering’s general view.

For 150 years and more, the main camps, whatever their many differences, have agreed on one thing: that the main groups of colour-blind people—those who confuse reds and greens and browns, and are today classed as dichromats⁶—have in some sense no perception of red and green at all, but only of yellow and blue.

All colours appear to the colour-blind as if composed of blue and yellow. (Maxwell 1855, 140)⁷

[A red-green blind person] sees as colourless what to others appears in one of the two fundamental colours, red and green; while, in any mixed colour containing red or green, he sees only the yellow or the blue. (Hering 1878, 107; cp. Hering 1880, esp. 102-103)

The two main camps managed, in their different ways, to agree on this. (For Hering, the colour-blind had sensations simply of yellow or blue. For Helmholtz and Maxwell, they had (to take the case of the ‘red-blind’) sensations of green and violet, but *called* them yellow and blue—and were relevantly sensitive only to yellow and blue.) And the dominant view has been the same over the course of the last century. To take just two representative speakers:⁸

The color perceptions of both protanopic and deuteranopic observers are confined to two hues, yellow and blue, closely like those perceived under usual conditions in the spectrum at 575 and 470 mμ. (Judd 1948, 247)

Two types [of dichromat] fail to experience red and green hues, both of which are presumably seen as grays. As a result, the world appears to them in various shades of blues, yellows, and grays, as though the red/green dimension of the color solid had been eliminated. (Palmer 1999, 104)

The deficiency, we are told, is not a local one confined, for example, just to certain reds and certain greens, or to those colours under certain circumstances: it amounts to the complete loss of a whole dimension. Orange and purple must lose their red component, turquoise and chartreuse lose their green, and the whole world of colour collapse from three dimensions to two.

This view might seem to have almost ideal credentials. It fits what some of the colour-blind themselves have reported (e.g. Scott 1778: 'I do not know any green in the world ...; but yellows ... and all degrees of blue, except those very pale ..., I know perfectly well.' (612-3)). In Helmholtz's version it seemed confirmed by some elegant mathematical modelling of the confusion patterns of the colour blind (which we will take up in §3). Hering's view was perhaps phenomenologically more plausible, in allowing the colour-blind to perceive white as white, rather as blue (in the case of the 'red-blind') or purple (for the 'green-blind'). And Hering's view could claim empirical vindication in the 1880s and 1890s with some studies of colour-blindness in just one eye, which reported (as we shall see in §4)—using the good eye to calibrate the experiences of the bad—that a 'red-green colour-blind' eye indeed yielded experience only of yellow and blue. And what is now the standard synthesis (promoted in different ways by Hurvich & Jameson, and others)—combining three cone types in the retina (as in Helmholtz) with some kind of opponency (as in Hering) in the later processing—might seem to inherit all the advantages of each party. And its proponents have eagerly reaffirmed the claim: 'the red/green deficient sees only yellows and blues in addition to white' (Hurvich 1981, 244).

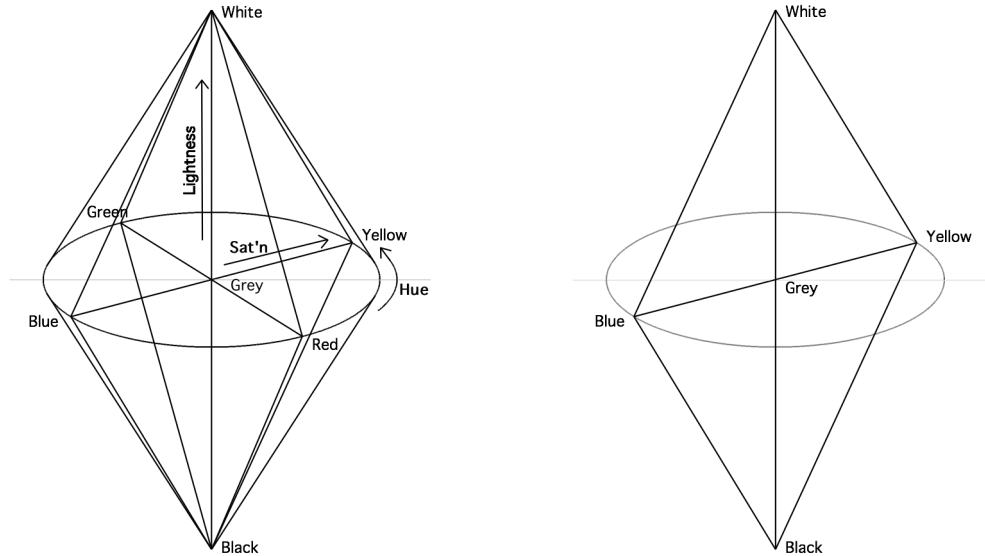


Fig. 1. Collapse of colour space in dichromats according to the standard model. Left: Illustrative representation of the experienced colour-space of normal trichromats: as a double-cone. (See e.g. Ostwald 1931, vol. 1 ch. 7; De Valois & De Valois 1975, 148; Hurvich 1981, 11; Rossotti 1983, 146.) Variation is possible in three dimensions: variation in hue is indicated by circular motion about the central vertical axis, saturation by distance from the central axis,⁹ lightness for surface colours (or brightness for lights) by height along the central axis. Labels mark the position of 'unique' (i.e. phenomenally pure, apparently unmixed) hues, which mark out two intersecting cardinal axes of phenomenal colour-space, Red-Green and Yellow-Blue. (The diagram is intended only as illustrative; for other kinds of colour-order system, see e.g. Zelanski & Fisher 1989 ch. 6; Hunt 1987 ch. 4 / 1998 ch. 7.) **Right:** Illustrative representation of the experienced colour-space of protanopes and deuteranopes, according to the standard model: three dimensions collapse to two. Variation is possible only in lightness and saturation of a single yellow or blue hue. (The circle at middle height (drawn as a fainter line), is no part of the dichromat colour-space—it is included to show the perspective and to indicate something of what has been lost by comparison with the space of normal trichromats.)

1.1 First reasons to be sceptical about the Standard Theory

This view of the colour-blind is, however, I think, very unlikely in the end to be true. I shall mention right away two difficulties. First, many dichromats clearly *talk* of seeing more than just varieties of yellow and blue. One might suspect they were merely aping other people's words without understanding them; but (as we shall see) at least some of them talk as if they both recognize and experience a much larger range of colours. Dalton, who was, it seems, a deuteranope (Hunt et al., 1995), believed that in the spectrum—e.g. looking at light from a prism—he saw only yellow, blue and perhaps purple (Dalton 1798, 90), but he reported seeing plenty of other colours under other conditions, especially by the light of candles or an oil lamp. Crimson by day, he says, has a blue tinge; but by candlelight it 'becomes yellowish *red*'. 'Pink by candle-light seems to be three parts yellow and one *red* or a *reddish yellow*.' (Dalton, 1798, 92, my emphasis) Dalton confuses reds and browns and greens, but he does not talk as if he had, so to speak, just a

single kind of colour experience produced more or less indifferently by all of them (and, in the standard model, by grey or pale yellow). Rather, it seems, reds and browns sometimes look green, greens sometimes look red or brown -- and the experiences in the two cases are different. 'A decoction of Bohea tea, a solution of liver of sulphur, ale, &c. &c., which others call brown, *appear to me green*.' By contrast, 'Green woollen cloth, such as is used to cover tables, *appears to me* a dull, dark, *brownish red* colour.' (1798, 92, my emphasis) Dalton does not talk as though he had just a single pair of hues. Things may, it seems, look red or look green to him, and not just yellow or blue—neither of the first two seeming merely a variety of the latter two (or of grey). He talks as if he had a system of colours much like other people, but with a tendency to get experiences of one colour where a normal person would get experience of another. We shall see reports from other people later that point to a similar condition. The reports are of course not to be taken automatically at face value; but we shall have to investigate whether they might in fact be true.

Secondly: There is experimental evidence from many directions that the colour-blind do not regularly make the kinds of confusion that they are supposed, on the standard model, to make. Dorothea Jameson (Jameson & Hurvich 1978) devised a fascinating experiment with the Farnsworth D-15 test in which two out of three protanopes, having just produced a characteristically confused ordering of the little coloured caps in the Farnsworth test, then go on to give the colour names of the caps no less correctly than a normal trichromat. It seems that many dichromats can often, notwithstanding their failings, in some way recognize red caps as red and green caps as green. (The Farnsworth test involves 15 little caps, each containing a circular patch of colour, about 12.5 mm in diameter, set in a black plastic surround, to be placed in order of colour similarity, starting with a reference cap, which is blue.) And we shall find (in § 5) a stream of other reports of red-green discrimination among certified dichromats. (A selection would include: Nagel 1905, 1908, 1910; Scheibner and Boynton 1968, Smith & Pokorny 1977, Nagy & Boynton 1979, Nagy 1980, Montag and Boynton 1987, Montag 1994, Crognale et al. 1999, and Neitz et al. 1999.) The evidence seems to be that a majority of dichromats who confuse red, green and yellow spectral lights when they are presented in small fields of 1° or so,¹⁰ actually manage to distinguish those colours when they are presented in larger fields, especially of 10° or more. They may do better with surfaces rather than spectral lights, and (when presented with surfaces) with fairly bright rather than dim illumination, and with higher saturation rather than lower. Additional viewing time may also help, along with other factors that I shall be exploring later (in §6)—for example, seeing things in a variety of kinds of illumination, and letting the eyes rove over the object, seeing it both with direct and with peripheral vision.

Discriminating red and green is one thing, actually seeing—or having sensations of—red and green is another. Jameson & Hurvich, having shown that their two out of three protanopes did well at labelling the red and green caps, insist nonetheless that the success is merely a matter of what we might call 'judgment' or 'inference', rather than sensation: the dichromats are using some '*rule ... for correlating red vs green hue names with lightness*', while it remains true that 'only two hues [namely, yellow and blue] and an achromatic locus are *available*' to them (Jameson & Hurvich 1978, 154, 151, my emphasis). (The

protanopes are supposedly using the lightness rule, 'if dark, then red' (1978, 154).) But such rules would not be very effective and the proposed one doesn't fit the subjects' actual performance. (There are greens that are darker than many reds and reds that are lighter than many greens, and Jameson & Hurvich themselves admit that the one cap in that category in the Farnsworth test was still correctly named by their two protanopes.) And other investigators have reached very different conclusions: Nagel concludes that the majority of dichromats have a sensation of red, and some 'remnant' perhaps of green (1908b, 23; 1910). Boynton & Scheibner say: 'We must now agree when [the protanope] says that the sensation of red is not unknown to him.' (1967, 220) Both sides in this debate make claims that need to be investigated and tested, and we need to ask (as I shall in §§7 and 9) what kinds of empirical evidence might succeed in deciding between the rival views. But among the many options, we will need to consider the possibility that, even if the information from a dichromatic eye has in some sense only two colour-dimensions, still the colour-blind may, by using cues within that information stream, succeed, not only in gathering information about the 'missing' dimension of colour, but also in synthesizing 3-dimensional colour-*experience*.

1.2 Methods and accessibility; general importance of the issues

The issue is philosophically important, I believe, rather than being, so to speak, merely a question of straight scientific fact. First, it raises questions about the ways in which one group of human beings can learn about the experiences of another group with a significantly different sensory system. It is of course quite standard to examine and try to learn from other people's discriminatory and classificatory behaviour, but I shall be particularly interested in how *what people say and do* can tell us about the *structure* and *interrelations* among the colours they see—the patterns of similarity and difference, which colours seem to 'contain' others (as we say orange contains red and yellow), which colours are 'opposite' to or contrast strongly with others, and so on. This will be important in investigating whether the colour-space of dichromats really is merely 2-dimensional (cp. §§7 and 9 below). Secondly, the debate provides some interesting cases in which to examine the relation of *information* to *perceptual experience*, and the relation of the complexity of *receptor systems* to the complexity of the *information* that is gathered with them. We will need to consider (in §7), for example, whether the colour-blind who correctly call red things red are really *seeing* them as red, or merely 'working out' or 'judging' that they are. And if, as I shall argue, there is a good case for saying that at least some dichromats see red and green in addition to yellow and blue, then we will need to reflect on how it can be that in some sense a 2-dimensional input can yield something like 3-dimensional information and experience—which is something for which I shall offer some mathematical modelling (in §6). We may have reason to doubt some of the simpler conceptions of the relation of qualia or 'sensations' to their causes.

A third reason for philosophical interest is that we need to keep in mind the question of what in the world colours, primarily, *are*. To be very quick on an issue that I develop a little further (in §8) but must mainly leave for another occasion: it may be that we need both what might be called a more 'dynamic'

conception of *colours*, and a more dynamic conception of our *perception* of them. The case for the latter idea may be an easier one to make: Just as feeling the weight of something (to the extent that we do so at all) often involves going through a process of interacting with it, *weighing* it in the hand or lifting it up and down—seeing what it can do, by testing it out under a variety of slightly different manipulations—, and just as there are things one can identify by touch in the dark but only by *moving* or *rolling them around* in one's hand, so also seeing the shininess of a small piece of paper typically involves not just catching a glimpse of it under a single condition of uniform illumination, but, rather, letting one's eye rove over it, or moving one's head a little to see how it catches the light from different things at various angles. And the perception of colours may often operate in a similar way. The larger debate on these issues is not for the present place.¹¹ But if that is more our picture of perception in general, with us harvesting information over time and space in order to synthesize or update our picture of those portions of the world that we are navigating our way through (not to mention the closer contact of trade and exchange at ports of call), then it will, I think, seem less strange to allow, as I think we will find we must, that a colour blind person—while lacking at any one moment much of the information available to the normal-sighted—may yet succeed over time in compensating in large part for that deficiency. And such a conception of the *perception of colour* may in turn both influence and be influenced by our philosophical conception of what *colours* themselves *are*—whether they are, for example, sensations, or dispositions to produce experiences in us, or dispositions to change incident light, or whatever else.

I might mention a fourth and last question, in the epistemology as well as sociology of scientific thought. If, as I think we shall see, there has been evidence over more than 200 years, often from respected and substantial figures—like Dalton in 1798, George Wilson in 1855, Wilibald Nagel in 1905-1910, Smith & Pokorny in 1977, Robert Boynton and his collaborators repeatedly between 1967 and 1990—that the standard theory of dichromats cannot be correct, at least on what is indiscriminable from what, and perhaps also on what the experience of dichromats is, then we must ask how it is that that standard theory has been repeated again and again, as if it were simply straightforward fact—as if it just *had to be* fundamentally right, even as the best evidence pointed to its being incapable of being strictly true. I find myself wondering whether the situation may not have some of the structure of the early stages of a Kuhnian scientific revolution — where people stand by an established theory while there is countervailing evidence that is in a sense well-known and *noted*, but which is held at a distance as being fundamentally unimportant, because there seems no other 'paradigm' available within which it could be give a more significant place.¹² Whether the present situation is or is not of that kind will depend, of course, on whether any such new picture or theory develops and meets with success.

I appeal on a number of points to the history of work on colour-perception over the last 200 years, much of it published in German. I do so with some delight—partly because I think we benefit from reexamining the fundamental empirical bases of the views that have been dominant since Maxwell and Helmholtz in the 1850s. But I do it also because this is a field in which there is a lot that we would not learn

at all (including matters of, so to speak, straight scientific fact) if we did not learn it from the history. The dominant views since the 1850s have left little room for even envisaging that, with just two colour receptor types, perception of more than two principal hues might be possible at all; the dominant trends in psychology have had little time for debates about the experience of the colour-blind without a firm experimental grounding, and have had limited interest in devoting the ingenuity that would be required to devise experiments that might actually tell us about it. For this reason it is, I think, particularly interesting — though there are risks, of course, too — to go back to descriptions of colour blindness from a time before that model had fully taken hold, for example, to Dalton and George Wilson, and to rare independents like Wilibald Nagel who know the standard theory as well as anyone, but are unashamed to talk of their own evidence and experience as conflicting with it. And, as we shall see from our study (in §4) of Judd's 1948 survey of some 80 years of unilateral cases of colour-blindness, even what look like authoritative and balanced later reviews of a subject may give practically no sign of the variety of conflicting views and evidence that actually exists in the original reports.

2. Some terms and tools

2.1 Types of Colour Blindness; Diagnosis

I present a table from Wyszecki & Stiles 1982, as a summary of the main forms of colour blindness. Normal colour vision in humans is standardly taken to be trichromatic — evidence for which comes from the fact that, with a set of three suitably chosen lights, we seem to be able, by some mixture of different quantities of those three, to match any coloured light presented to us. The main forms of colour-blindness that we will be concerned with are forms of *dichromacy*, where there is such poor discrimination (at least with small fields) that it takes only a pair of suitably-chosen lights (e.g. Red₆₅₀ and Blue₄₆₀), rather than the normal three, to get, by some mixture, an acceptable match for any presented light. The natural explanation is that dichromats lack one of the standard cone types — as is now well-confirmed: *protanopes* (the 'red-blind', in the language of Helmholtz and Maxwell) lack the L-cones, the long-wave receptor type, while *deuteranopes* (or the 'green-blind') lack the M-cones, the medium-wave receptor type. Definitive diagnosis of these conditions depends, as we shall see, on tests with a Nagel Anomaloscope or similar apparatus. But the general symptoms are these: protanopes and deuteranopes typically confuse reds and greens; in a spectrum (i.e. a real spectrum produced, e.g., by a prism) they mostly see only *yellow* (from the red, orange, yellow and yellow-green parts of the spectrum) and *blue* (from the blue-green, blue and violet parts), with the central green zone looking to them more or less colourless, near what is known as the 'neutral point' (at ~490-495 nm for protanopes, ~495-500 or 505 nm for deuteranopes). Protanopes have what is sometimes called a 'shortened spectrum', in that, lacking L-cones, they see much less of the extreme red end of the spectrum than normal trichromats do. There is a third form of dichromacy, tritanopia, where the S-cones

are missing: the tritanope too is said to see only two hues, standardly taken to be red and green. But the condition is extremely rare and I shall say almost nothing more about it here.

There are less extreme forms of colour-blindness known as *anomalous trichromacy*. The two main varieties, *protanomaly* and *deuteranomaly*, involve a person's making similar but milder confusions to those found in the corresponding forms of dichromacy, *protanopia* and *deuteranopia*. Reds and greens are confused in ways resembling the dichromat confusions, but testing reveals that the condition is one of *trichromacy*—in that a set of three lights is typically needed in order to match any given light—though it is *anomalous*, in that the matching is somewhat different from normal. Until recently, there were a number of competing hypotheses about the nature of these conditions (see Wyszecki & Stiles 1982, 462-3; Mollon 1997). The accepted view today (see e.g. Nathans 1999, Neitz & Neitz 2000, Deeb 2005) is that a *protanomalous* person is lacking L-cones (like a protanope), but has two varieties of an M-cone pigment type with slightly different wavelengths of peak sensitivity (we might call these M- and M'-cones), so he can compensate to some extent for the absence of the L-cones. A *deuteranomalous* person has no M-cones, but has two varieties of the L-cone type (L and L'), which, again, allows some compensation for the deficiency. There is much variation in the degree of severity in cases of anomalous trichromacy; the defect is often said (though there are certainly exceptions) to be less severe the larger the separation between the peak absorptions of the two varieties of the remaining L- or M-cone type. There are extremely interesting questions about how we should characterize what anomalous trichromats see—but I shall confine myself here to the more severely affected groups, the dichromats.

Some terms and tools

Characteristic	Prot-anomalous	Deuter-anomalous	Protanope	Deuteranope	Tritanope	Rod-Monochromat
Color discrimination through the spectrum	Materially reduced from red to yellowish-green but to a varying degree in different cases		Absent from the red to about 520 nm	Absent from the red to about 530 nm	Absent in the greenish-blue (445 to 480 nm)	No color discrimination
Neutral point (i.e., wavelength of monochromatic stimulus that matches a fixed “white” stimulus)	None	None	490–495 nm	495–505 nm	568 and 570 nm	All wavelengths
Shortening of the red (i.e. reduced luminous efficiency of long wavelengths)	Yes	No	Yes	No	No	Yes
Wavelength of the maximum of luminous efficiency curve	540 nm	560 nm	540 nm	560 nm	555 nm	507 nm
CIE 1931 chromaticity of the confusion point (dichromats only)	—	—	$x_{pc} = 0.747$ $y_{pc} = 0.253$	$x_{dc} = 1.080$ $y_{dc} = -0.080$	$x_{tc} = 0.171$ $y_{tc} = 0$	—
Percentage frequency of occurrence						
among males	1.0	4.9	1.0	1.1	0.002	0.003
among females	0.02	0.38	0.02	0.01	0.001	0.002
Cone types thought to be present	M, M', S	L, L', S	M, S	L, S	L, M	none
Standard view of Range of Hues experienced	Y, B weak R, G	Y, B weak R, G	Y, B	Y, B	R, G	none

Table 1. Salient Properties of Color Defectives.

From Wyszecki & Stiles 1982, 464 (footnotes omitted). Final two rows added by the present author (following (1) e.g. Nathans 1999, Neitz & Neitz 2000; (2) e.g. Judd 1948 (for Protans and Deutans) and Hurvich 1981, chs. 16-17).

The main tools of diagnosis, from the 1880s onwards, can be divided into two kinds: those for the practising doctor and those for the experimental scientist. For the doctor, there are pseudoisochromatic plates like those of Stilling (Leipzig, 1877-79; rev. 1922, 1952) and Ishihara (1917), which are still in use today—which are like a pointillist painting made up of coloured dots in which certain patterns and shapes are visible or salient for one kind of observer but not for another. (In one Ishihara plate, the normal read the figures 29, and the red-green blind read 70.) The easily-administered confusion tests with wools or coloured papers were by the late 19th century known to be very imperfect—for the telling reason that too many colour-blind people, even dichromats, proved capable of ‘passing’ them.¹³ Improved tests used small coloured caps, to be placed in an order of colour similarity, as with the Farnsworth ‘100-hue’ and D-15 tests (cp. Farnsworth 1943).¹⁴

For the experimental scientist, more precise investigation—both of colour-blindness and of normal colour vision—involves metameric colour-matching: that is, seeing how one colour can be matched with a mixture of other colours that is different in physical composition. In his first researches, Maxwell used spinning disks or ‘tops’ to produce an optical mixture of variable-sized segments of coloured papers that

could be attached to the top (Maxwell 1855). But much more revealing was the ‘spectral apparatus’ that Maxwell soon had made, which displayed two adjacent fields of light: one field of spectral light,¹⁵ to be compared with a second field, containing a mixture of two or more kinds of spectral light in adjustable combinations (see Maxwell 1860, and Fig. 8; cp. Nagel 1914, 63-80). A later version of the same kind of apparatus was the colorimeter of W. D. Wright, which was used in gathering much of the data on which the CIE colorimetric system was based (Wright 1927; 1946, chs. 3-4). In the earlier days, the fields were, for technical reasons, bound to be either small or desaturated or impure—and a virtue was made, in much of the quantitative work, of the near necessity of small fields of 1° or 2° or less. Nagel’s anomaloscope (as we shall see) uses small fields; but in other work Nagel is a rare exception, expressing his delight in 1910 at the new apparatus he has had built in Berlin, which allowed large fields of 20° —and he sometimes even used fields of $\frac{1}{2}$ to $\frac{3}{4}$ m² viewed from a distance of 50-75 cm, which might extend over an angle of 70° or more (Nagel 1910, 6-7).

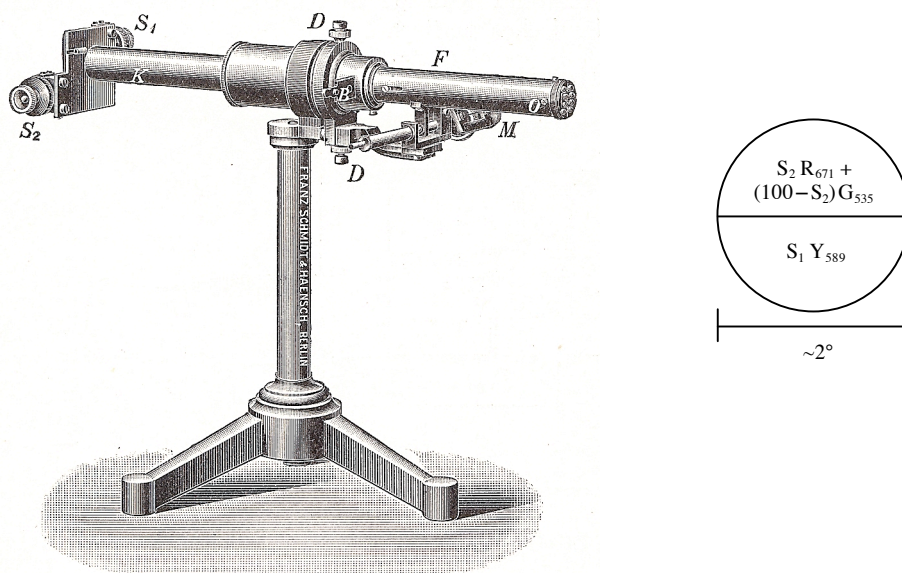


Fig. 2. *Nagel Anomaloscope Model II* (Nagel 1914, 74; cp. 1907c; 1908b, 35-37). When pointed at a light source further to the left (not illustrated), the instrument presents a circular field, the lower half of which is illuminated with spectral yellow light (e.g. 589 nm), and the upper half with spectral red or green (e.g. ~ 671 and ~ 535 nm), or a mixture of the two in any desired proportion. (See diagram on **right**.) The instrument is built on the principle of a spectroscope that uses prisms to create a spectrum from a light source, and slits to select desired portions from that spectrum. The light source is placed at the extreme left on the axis of collimator tube K (*Kollimator*) (—a gas or petrol lamp was typically used, or now an electric bulb, set about 18 cm from the end of the tube—); the prisms are in the centre of the apparatus; the resulting image is viewed through lens O (*Okular*) of the telescope F (*Fernrohr*). At the far end of tube, the left screw (S_2) controls the mixture in the upper half-field (from pure Green₅₃₅ at a setting of 0, to pure Red₆₇₁ at 100); the right screw (S_1) controls the lightness of the Yellow₅₈₉ (from 0 for darkest to 100 for lightest). (For the visual angle, see Pokorny et al. 1981, 25, speaking of Model I anomaloscopes available in the 1970s; Farnsworth had earlier had a Model II with an adjustable diaphragm

(controlled by B (*Blende*) in the illustration) giving visual angles of $1^{\circ} 15'$, $2^{\circ} 10'$ or $3^{\circ} 15'$ (Willis & Farnsworth 1952, 29).)

Some illustrative examples of settings (figures in italics correspond to settings on the screws S_1 and S_2 : they should not be read as exact quantitative measures (cp. Nagel 1908b, 36-37)):

Normals: e.g. $55 R_{671} + 45 G_{535} = 14 Y_{589}$

Protanomals (need more red, and the red light is of relatively low luminance to them): e.g. $70 R_{671} + 30 G_{535} = 4 Y_{589}$

Deuteranomals (need more green): e.g. $42 R_{671} + 58 G_{535} \approx 14 Y_{589}$

Protanopes & Deuteranopes are so indiscriminating as to accept practically *any* ratio of R_{671} and G_{535} as matching the Y_{589} , given suitable adjustments in brightness.

The easily-administered tests, like the Ishihara plates and the Farnsworth D-15 cap test, have the important weakness that they may make clear, for example, that a person has some kind of protan deficiency without determining whether he is a protanope or merely protanomalous. A differential diagnosis depends on the ‘Rayleigh matches’ that he makes—i.e. the proportions of red and green light in a mixture that he accepts as matching a given yellow. (Think of the way yellow is produced on a TV screen, by a normal combination of the red and green phosphors. Then add the fact—first pointed out by the physicist Lord Rayleigh in (1881)—that some colour-blind people (the anomalous trichromats) need abnormal proportions if they are not to find the mixture either ‘too red’ or ‘too green’; while—pretty much as Helmholtz and Maxwell had earlier said—others (the true dichromats) are so indiscriminating that they accept almost any red-green mixture (abnormal or normal) as ‘yellow’, just as long as it is bright enough.) This can be tested in many ways, but for relative simplicity and convenience, the physiologist Wilibald Nagel perfected, in about 1907, an Anomaloscope that it is still a standard tool today, along with more recent designs that operate on similar principles. Nagel designed the apparatus to present one field of $Yellow_{589}$, which could be adjusted in brightness, and a second field containing a mixture of Red_{671} and $Green_{535}$ in adjustable proportions. Anomalous trichromats are identified as those who need either more red or more green than normal, in order to match the yellow; while dichromats are those who accept almost *any* mixture of the red and green, as long as the yellow is suitably adjusted to match it in brightness. (A rough explanation for the dichromat confusions is this: the S-cones have practically no sensitivity in the green-yellow-red part of the spectrum, above ~ 530 nm; so dichromats, presented with the stimuli from that part (namely, 535, 589 and 671 nm) will have only one cone type to perform the task of discriminating them: hence a suitable adjustment in brightness should be able to make any two of these stimuli seem equivalent.) When authors describe someone as a *dichromat*, the standard criterion—the more or less definitive test—is this acceptance of Red_{671} , $Green_{535}$ and $Yellow_{589}$ (or similar lights) as more or less interchangeable and indiscernible in 1° - 2° fields in an anomaloscope, as long as the fields are suitably adjusted in brightness.

2.2 Chromaticity diagrams

The standard model of colour blindness can probably only be understood with a grasp of the principles behind chromaticity diagrams and colorimetry in general; but I shall say just a few things here which, with some examples, will, I hope, be adequate for now.¹⁶ A newcomer who wants to move on to the later discussion of colour blindness may find that it is enough, for a first impression, just to examine the two main diagrams with their captions: it may make most sense to come back to the main text later.

We will later be making much use of the x, y chromaticity diagrams of the CIE (*Commission internationale de l'éclairage*), which provide a very helpful framework for presenting a lot of colour information. The particular form of these diagrams was standardized in 1931, on the basis of colour-matching data that had been gathered in the 1920s by W. D. Wright and others—and they are part of a whole colour measurement system that has mathematical characteristics that it would be inappropriate to explore here. But the basic idea may be understood as an application of the same principles as in a Maxwell triangle. Taking three reference lights (for example, blue, green and red of 460, 530 and 650 nm), we may use a triangle to represent the various proportions in any combination of those three lights. And we can ‘measure’ the colour of any given light by specifying the mixture of our three reference lights that would (for a standard human perceiver) be a match for it; and we can ‘measure’ the colour of a surface by doing the same for the light that it reflects under some particular kind of illumination. The theory and practice of this are a subject in themselves; the relevance of this to colour blindness is that the same diagrams as are used to specify the colours of things to normal trichromats are also often used to specify the patterns of confusion found in dichromats—and they are a promising, though sometimes misleading, guide to what dichromats do and do not see.

Some terms and tools

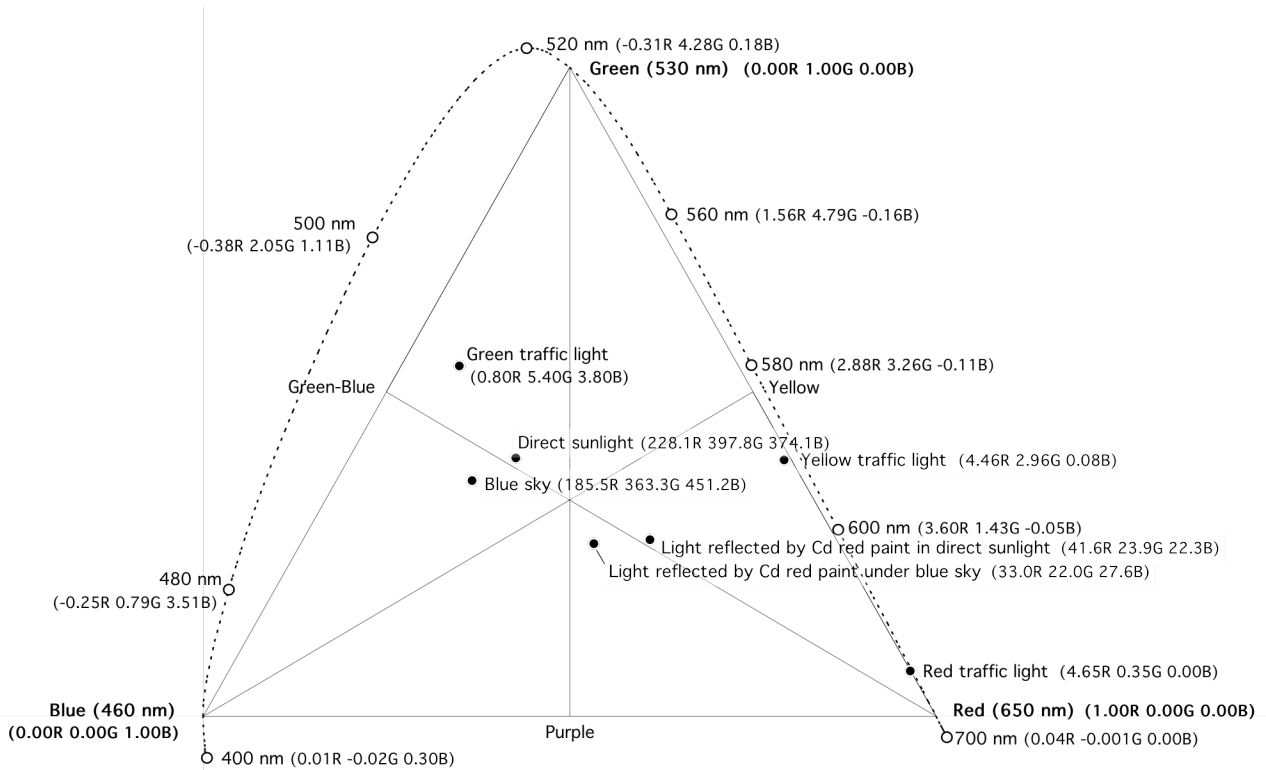


Fig. 3. Maxwell triangle: Mixtures of three 'primaries' in different proportions and the mixtures required to match various standard light-sources. Three reference lights or 'primaries' (Red₆₅₀, Green₅₃₀ and Blue₄₆₀) are represented at the three vertices. Mixtures of those lights in different proportions are represented by points on the plane whose 'closeness' to the three vertices represents the relative amount of the corresponding three kinds of light in the mixture. (A mixture of n units of light A and m units of light B will be represented at a point M on the straight line between them, such that $AM / MB = m / n$. In general, for a mixture of several kinds of light, if we associate with each component a weight proportional to the number of units of that light and place that weight at the point representing the component, then the resulting mixture will be represented at the centre of gravity of the resulting system of weights.) Points mark mixtures found to match various common kinds of light (traffic lights, Blue sky, etc.), and (on the dotted-line curve) light of different kinds through the visible spectrum from 400 nm to 700 nm. (In parentheses are the representative amounts of the three primaries needed to match the various lights. Absolute intensities are only illustrative.) (The three primaries and the relations between units for them are set as in the W. D. Wright system (e.g. Wright 1946, 125-135). White would normally be in the centre of a triangle like this, but it is here displaced to the left and slightly up (to a position roughly between 'Direct sunlight' and 'Blue sky'), because of the relatively large size of Wright's units of Red₆₅₀. The positions of points on the curve (and the tristimulus values in parentheses) are my own recalculation by means of a linear transformation of CIE XYZ functions. Positions of common objects within the triangle (and tristimulus values) are illustrative only: for railroad signals, see Judd 1952, 174; for light reflected from paints, see §6 below. For the general principles of such representations, see Newton 1704/1730, 154-158; Mayer 1775; Maxwell, 1855, 1860; Hunt 1987, 58-60.)

Where do the colours of the spectrum—i.e. the ideally narrow wave-bands of light coming from a prism or diffraction grating—belong in such a diagram? Answer: spectral lights are higher in saturation

than anything that can be produced by a mixture of other lights, so they cannot be mapped within or on the boundaries of a triangle like the one illustrated here. But spectral lights stand in the same kinds of relations to the lights we already have in our triangle as those lights stand in to each other, and they can be given a place on the same plane, but outside the triangle. This will be clearer from an example. To match a spectral Yellow₅₈₀, we can find a mixture of the Red₆₅₀ and Green₅₃₀ primaries that is a good match in hue; but any such mixture will be less saturated than the pure Yellow₅₈₀.¹⁷ If, however, we take a little of the blue primary to desaturate the Yellow₅₈₀, then we can get an apparently perfect match. We discover experimentally an equivalence such as this:

$$10.0 Y_{580} + 0.11 B_{460} = 2.88 R_{650} + 3.26 G_{530},^{18}$$

Then we may rephrase the colour-matching information in the following form, and treat the right-hand side as specifying an equivalent for our 10.0 units of Y₅₈₀:

$$10.0 Y_{580} = 2.88 R_{650} + 3.26 G_{530} - 0.11 B_{460}.$$

And that allows us to fix a position in the diagram for Y₅₈₀, just outside the main triangle, slightly higher and to the right of the R₆₅₀G₅₃₀ line.

An experimenter could go on to do similar tests for the other kinds of light in the spectrum—seeing what quantities of our R, G and B primaries are needed to match light of a range of wavelengths, e.g. 400 nm, 410 nm, and so on, through the visible spectrum (or, more exactly, seeing how each light, if desaturated with one of the primaries, can be matched with some combination of the other two). This is the kind of work that was done by Maxwell (1860), and redone with greater accuracy in the 1920s by J. Guild and W. D. Wright. The kinds of results found are illustrated in Figure 3 above—and with suitable averaging, smoothing and other processing, they became the basis of the standard system of colorimetry adopted and published by the CIE in 1931.

It is a high-level fact of the human visual system, known as Grassmann's Third Law, that (within limits) if lights *a* and *b* match each other, then they will behave indistinguishably when used as constituents in further mixtures: if *a* matches *b*, then *a* mixed with any further light *c* will match *b* mixed with *c*, regardless of what differences there may be in the wavelengths of light composing *a* and *b*.¹⁹ Hence, if we have set up the diagram treating one set of lights (e.g. R₆₅₀, G₅₃₀ and B₄₆₀) as primary, with further lights (e.g. 400, 405, 410 nm) being defined in relation to them, we may go on to recognize that each of the first set of 'primaries' can itself be matched by a combination of other lights (e.g. R₆₄₀, G₅₁₀ and B₄₄₀) in appropriate quantities—and each could be replaced, for any mixing-and-matching purposes, with the equivalent mixture; hence, if we know the amounts of the first primaries needed to match some particular light *L*, we can also calculate what quantities of the second set (e.g. R₆₄₀, G₅₁₀ and B₄₄₀) would be needed

to match it. In effect, we can translate between one set of ‘primaries’ and another. And we may treat a diagram like this as representing a space of *colours* of lights (or strictly, their *hues and saturations*, brightness being a third dimension not here represented): two lights will have the same position on the diagram if and only if they are indistinguishable in hue and saturation; the colour of any mixture of lights will be determined on similar centre-of-gravity principles; and we can jettison any original assumption of any particular set of lights as ‘primaries’.

A Maxwell triangle is standardly an equilateral triangle. But there is nothing compulsory about the exact positioning on the page, so to speak, of the vertices that represent whatever primaries or other reference points we may adopt. In fact, the same set of colour-matching data can be presented in different diagrams, according as we place the vertices in a different layout—stretching and rotating (or indeed reflecting) the triangle in various ways—and changing, if we wish, the size of the units for each of the reference lights. A diagram based directly on our real R, G and B primaries needs, as we have seen, negative quantities to specify certain highly saturated lights; but we can lay out the same data differently with axes placed so as to avoid any negative quantities. This was done in the classic CIE 1931 system, which transformed information originally gathered in terms of Red, Green and Blue primaries in terms instead of new *X*, *Y* and *Z* primaries, which might be thought of as imaginary supersaturated red, green and blue lights.²⁰

Our first Maxwell triangle separated off the *relative* amounts of our three primaries from the absolute amount: our CIE *x*, *y* chromaticity diagram—which can be thought of as a projective transformation of the Maxwell triangle—is similar. It separates off *x* and *y*, which together capture something like the hue and saturation of a light—and which together are said to constitute its *chromaticity*—,²¹ leaving *luminance*, the psycho-physical correlate of brightness, as a third dimension to be specified separately and represented by a third variable, *Y*.²²

It should be remembered that the data behind the classic CIE diagrams are fundamentally simply ‘the average colour matching properties of the eyes of 17 British observers’ (Hunt 1987, 44). Even when the colour-matching functions are now published at up to 12 decimal places, for spectral lights taken at 1 nm intervals (as in Wyszecki & Stiles 1982, 725-35), they derive from a small number of people who do not agree perfectly among themselves. There are some known faults with the colour-matching data (see e.g. Wyszecki & Stiles 1982, 330-32; Stockman & Sharpe 1999) and there are more sophisticated colour-matching functions that have been developed since, by the CIE and others. But the basic CIE 1931 framework is mathematically developed and still serves as a fundamental reference system in colour science. The inaccuracies have been described as ‘negligible in most practical situations’ (Hunt 1987, 44); and I shall develop my own discussion here in terms of the 1931 system. It would obviously be possible to use more exact functions, and for certain purposes it would be better. But it would not be of much significance in the present context: first, I shall be discussing classic conceptions of colour blindness that were developed in accord with just the kind of colour-matching functions that are used in the CIE 1931

system. And, secondly, if we used any of the modified systems that I am aware of, the points of principle that I shall be discussing would arise in much the same way.

So far we have been mostly been representing lights, rather than surfaces; but the system can be applied in a similar way to represent the character of *light reflected* from a surface under a particular kind of illumination: we assign it a point in the diagram that represents the character of a mixture of reference lights that would be (for a normal human perceiver) indiscernible from the light reflected from the relevant surface in that context of illumination.

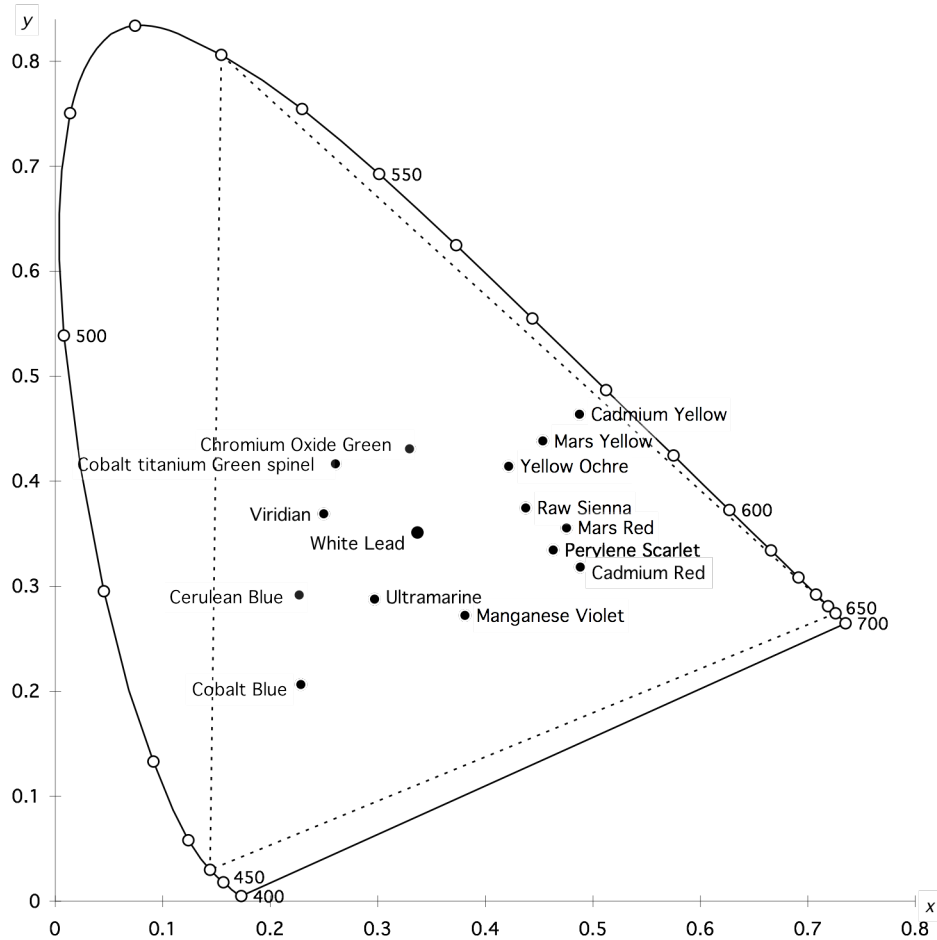


Fig. 4. CIE 1931 x, y Chromaticity diagram, showing locus of spectral hues (on the plectrum-shaped curve) and chromaticity coordinates for 15 standard pigments when illuminated by direct sunlight (of Correlated colour temperature 5500 K²³). The diagram can be thought of as an alternative mode of presentation of the same kind of colour-matching information as in Fig. 3, where B₄₆₀, G₅₃₀ and R₆₅₀ were our 'primaries'. The transform of our original Maxwell triangle appears here in dotted lines; and the geometry of the plectrum-like curve indicates what proportions of such 'primaries' would be needed to match light of the various wavelengths that make up the visible spectrum. Similarly, the position of each of the 15 pigments (or, strictly, of light reflected from them when illuminated with Standard Illuminant D₅₅, approximating direct sunlight (5500 K)) represents the proportions of such 'primaries' in a mixture that would match light from that pigment. —One may also think of the new representation as specifying the character of any given light, by giving the proportions of some new X, Y and Z 'primaries' that would be needed to match it, where X, Y and Z can be

thought of as imaginary supersaturated red, green and blue primaries, that would be represented respectively at (0, 1), (1, 0) and (0, 0) in the present diagram. See text for further details. (CIE diagram: e.g. Wright 1969, ch. 4; Hunt 1987, chs. 2-3. Chromaticity of 15 pigments: Author's calculation, from pigment reflectance functions in Mayer 1991, 65-134, and relative spectral power distribution function for CIE Illuminant D₅₅ (e.g. Wyszecki & Stiles 1982, 8-9), sampled at 20 nm intervals from 400 to 700 nm.)

It is worth asking precisely what is represented by position in a chromaticity diagram. People are sometimes tempted to say: 'How the object looks'. But strictly, the diagram represents not the sensations of subjects, but stimuli in the world—though grouped according to a relation of phenomenal indiscernibility (to standard human perceivers). A light stimulus is given a particular position in the diagram according to the proportions of red, green and blue primaries (or any equivalent to them) that would be needed for an apparent match or substitute for it (for standard human perceivers).

One might be tempted to say, then, 'The chromaticity diagrams tell us *which things look the same*, though not how those things look.' But that is only true in a limited way. *In any one context*, two light stimuli with the same position in the diagram (and the same luminance) will look the same, and can be substituted for one another without the colour appearance changing; but stimuli with the same chromaticity and luminance may look quite different, if they are in different contexts—because of adaptation, and 'reading' of the general scene, and other factors yet.

An example should make this clear. A blue card in yellow light may be reflecting light to the eye that matches a certain mixture of our primaries—a mixture that is (we may suppose) in fact white, more or less equivalent to CIE Standard Illuminant C. If so, it will be mapped at position $x = 0.310$, $y = 0.316$. Similarly, a yellow card in blue light may also be reflecting light that matches that same combination of primaries (and, similarly, Illuminant C); it too will be mapped at $x = 0.310$, $y = 0.316$. And yet, it may be (if a viewer adapts to the two different illumination conditions) that the first card *looks blue* (as indeed it is); and the second card *looks yellow* (as it too is). They are mapped at the same position in a CIE diagram because the light reaching the eye from the two of them would be matched with the same combination of reference lights. But that fact does not fix the things' respective appearance: one card looks blue and the other looks yellow. And two such lighting environments may even be both present at one time in different parts of one person's visual field: think of sitting inside by window, reading by reflected light that is mostly sky-blue, while things on the window-ledge outside can be seen bathed in a pool of yellow sun-light.

This will be of some importance. It is sometimes tempting to think that if the *chromaticity space* of an observer is restricted in some special way, then her *experience* is automatically restricted in a corresponding way. (So, if we found that the chromaticity space of dichromats reduced to a line, we might think their experienced colour space (aside from brightness) would similarly reduce to a line.) But there is no need in general for this to be true—given the remarkable adjustments that the visual system makes to adapt or compensate for abnormal conditions and 'restriction' or 'bias' in its input—; and in some cases it is definitely false. Colour constancy is one well-known form of such adjustment. But it may be worth

mentioning some more extreme cases too—and in particular the impressive demonstrations of Edwin Land (see e.g. Land 1959a, b), where—to take just one example—if we have two suitable ‘colour-separation’ photographic transparencies of the same scene (one taken through a red filter, the other through a green filter, both on black-and-white film) and projects them superimposed, one with red light and the other with white, then viewers report seeing the original scene, not just (as we might expect) in varieties of red, white and black, but in virtually all colour-categories, including red, yellow, green and (though less successfully) blue. (Land’s scenes included a bowl of fruit on a patterned tablecloth, and a Still Life with packs of Kleenex, Shredded Wheat, Jell-o and Campbell’s Tomato Soup.) The basic phenomenon—indeed Land’s original observation in 1955 (Bollo, 1959)—was that, starting with a standard projection of red, green and blue ‘separations’ onto a screen, one can turn the blue projector off and still (with some adjustment of the brightness levels) get an impressive range of colour impressions; and he found that the two projector lights may even be varied considerably in colour and brightness from the original red and green, without the effects being lost.²⁴

Land explored further varieties of such phenomena, using a ‘dual monochromator’ that allowed the two ‘colour-separations’ to be illuminated (or transluminated) with narrow wavebands of almost monochromatic light. With, for example, Orange₆₁₅ and Green-Yellow₅₆₀ as illuminants, one might expect people to see nothing but Orange and Greenish-Yellow and the yellowish hues in between, all at virtually maximum saturation since there would be nothing to desaturate the other two. But people actually report seeing things in virtually every colour category, including blue and white. The phenomenon is even more remarkable if illustrated in a chromaticity diagram (see Fig. 5). One short *straight line* in chromaticity space, on either side of Yellow—from YellowRed₆₁₅ to GreenYellow₅₆₀—, ends up producing a range of experience (including white) which is almost wholly outside what is usually associated with that line in the diagram.

Some terms and tools

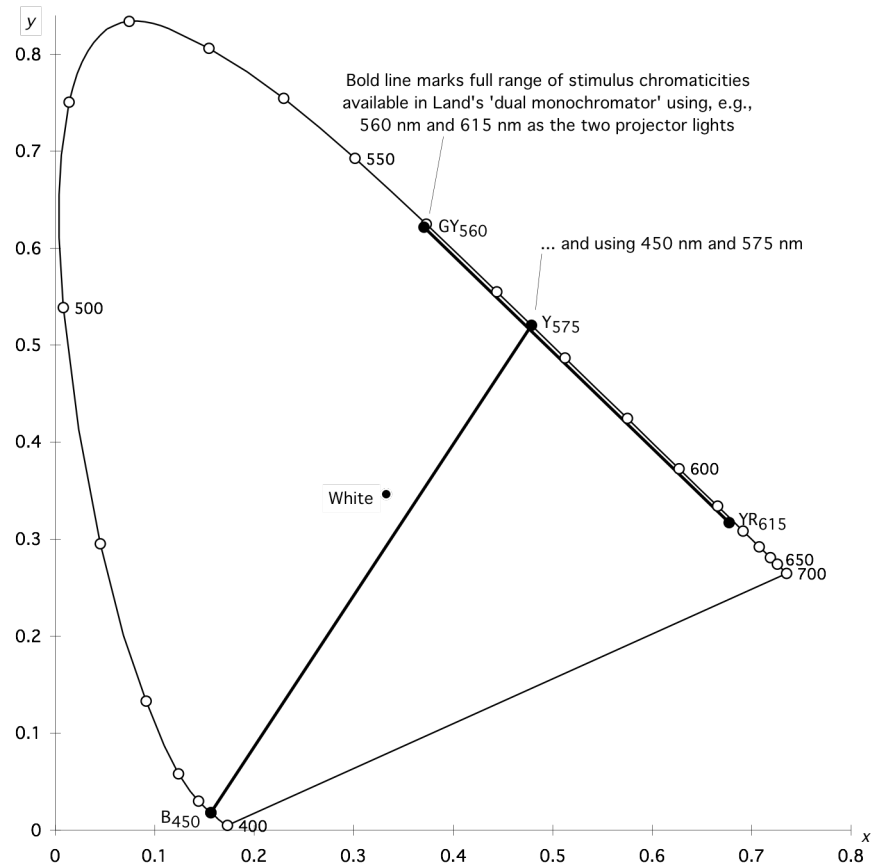


Fig. 5. Chromaticities of stimuli available in Land's 2-projector experiments: Another Collapse to a Line. When viewers are presented with two black-and-white colour-separation transparencies, projected in Land's 'dual monochromator' with YellowRed₆₁₅ light and GreenYellow₅₆₀, respectively, the chromaticities available fall merely on a straight line between those two projector colours; but the viewers report seeing not just varieties of yellow, but red, green, grey, white and even some blue—colours wholly outside the range of experiences normally associated with that line in chromaticity space. With Blue₄₅₀ and Yellow₅₇₅, the stimuli range 'objectively' from that Blue to that Yellow, via a slightly pinkish white; but viewers report seeing not merely White, Yellow, Gray and Blue, but also Red, Orange, Brown and Green. (One might note the similarity between Land's Yellow₅₇₅-Blue₄₅₀ line and the Yellow₅₇₅-Blue₄₇₀ line that Judd 1948 thinks captures the full range of colour supposedly seen by dichromats.) (Present author's diagram to illustrate experiments reported in Land 1959b, 641. The representative White is D₅₅.)

I mention this partly because, if it turns out that some dichromats experience more than just varieties of yellow and blue, then their condition will be structurally parallel to that of normal trichromats presented with Land's demonstrations: whatever differences there may be in the exact causal factors at work, it would seem that in both cases, 2D colour input is producing more than the standardly-expected 2D range of experience. Land's subjects are in effect turned into temporary dichromats by the loss of a third colour-signal; but they evidently do not end up with experience of merely two main hues—and it would be nice if this could guide us in understanding the experience of dichromacy. Unfortunately, I do not think we have much understanding of the Land phenomena themselves: they are certainly not explained by Land's

own retinex theory (Land 1977) nor, I think, by the critics (Judd 1960 and Walls 1960) who have invoked low-level mechanisms of adaptation and contrast, and the Helson-Judd theory of ‘color conversion’ (Helson 1938, Judd 1940). (Those accounts have implausibilities in themselves, and they make no sense of the great increase in vividness and range of colour seen when the two images are exactly in register.²⁵) I suspect we will not have a good understanding of the colours seen by dichromats until we have a greater understanding of the colours seen with Land phenomena too—and the latter, I think, remain extremely puzzling. Some of the factors I shall model later (in §6)—such as those involving the macula—are of no relevance to the Land phenomena; but others may have a role in both kinds of case—like the fact of individual surfaces being simultaneously presented under the modulating effects of a variety of kinds of illumination and spatial orientation—though we are only at the beginning of an understanding, I suspect, of how such factors actually operate.

There are limitations to chromaticity diagrams like these. They can be accurate only to the extent that normal human colour vision is genuinely trichromatic. If instead, for example, rods—which are usually thought to be involved mainly in vision at night—play a significant role also at higher light-levels, then two samples that stimulate the cones in similar ways, and are (on the basis of colorimetric data) assigned the same position in x, y, Y space may in fact (even in the very same context) look different from each other. I shall not be considering that fundamental issue here, though it is certainly not to be dismissed.²⁶ Equally, if (as is the case) in the population there are variants of the various cone pigment types, then the colour-matching functions (and hence the layout of spectral hues and other stimuli in a chromaticity diagram) will not be exactly the same for the various groups with the various types of receptor. And if there are individuals who not only have L-, M- and S-cones, but also more than one variant of some of the cone pigment types, then their colour-matching may perhaps be impossible to represent solely in a three-dimensional space. I shall, however, try to abstract from such issues here. I shall be arguing that the evidence suggests that the people standardly classified as dichromats do not, as the standard model implies, see merely two principal hues: they seem to see more colours than they ‘should’. And I shall investigate ways in which this might be achieved, even if those subjects are indeed strictly—as far as the number of retinal receptor types is concerned—dichromats.

2.3 Brief statement of the standard theory

There are two main claims to what I shall be calling the standard theory of dichromat perception: (1) that dichromats make confusions among stimuli that lie along confusion lines like those marked in the diagrams that follow. For the relevant form of dichromat, (i) all the lights represented on one confusion line are indiscernible (except perhaps in brightness), and (ii) they produce only a single kind of colour experience (with a similar proviso). One might say a whole confusion line collapses to a *point*, representing a single colour perceived by the dichromat (with variation only in brightness) in place of the whole variety of colours that a normal trichromats would perceive given the variety of stimuli represented on the line.²⁷

What is more, it is thought that the single colour-points for each of the many confusion lines all line up neatly on a straight line (rather than, for example, forming an arc or curve): thus, (2) dichromats see colours lying on a single straight colour-axis, which is standardly taken to run from Yellow₅₇₅ to Blue₄₇₀, via neutral.²⁸ The full range of coloured things, both lights and surfaces, looks either yellow or blue, in varying degrees of (perhaps vanishing) saturation and brightness.

The two parts of the theory can be held separately.²⁹ But the main claims go naturally together and might easily seem absolutely secure achievements of colour science. The main support for (1) lies in some theoretical considerations in Helmholtz and some empirical work in Pitt (1935). Support for (2) comes partly from general theoretical considerations about opponency in the visual system, but above all from reports of some rare cases of colour-blindness in one eye, where the other eye is near-enough normal for it to be used as a reference point in identifying the perceptions of the weaker eye. The combination of these views—which is shared by Judd, Le Grand, Hurvich and Jameson and many others—is illustrated in the diagrams below. My own view is that we have strong reason to doubt the foundations of both parts of the theory. But I shall only go relatively briefly over those reasons (in §§3 and 4), since the core of the present article (in §6) is, rather, to propose some hypotheses about *how*, if dichromats do escape the consequences of these arguments, they may be achieving their success.

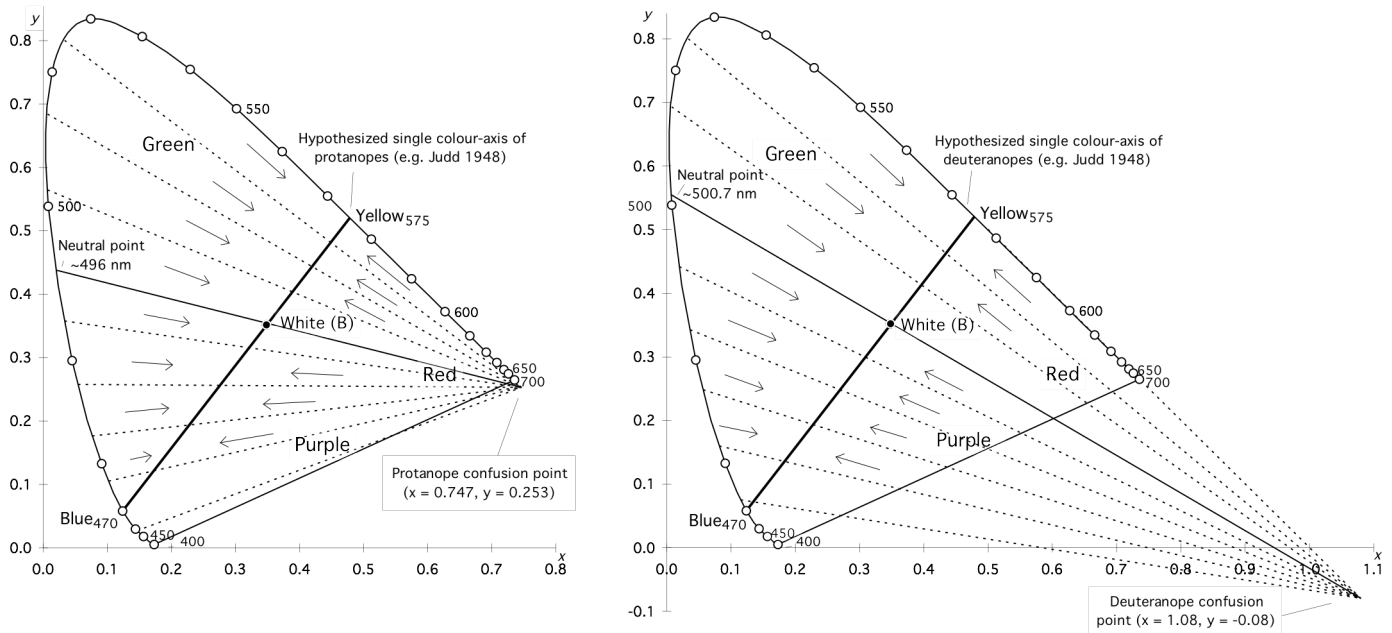


Fig. 6. Standard theory of Dichromacy: Confusion lines (dotted), and how stimuli in CIE 1931 chromaticity space are supposed to look to Protanopes (left) and Deuteranopes (right). These diagrams combine two ideas: (1) the use of confusion lines to indicate sets of stimuli supposedly confused and (2) the claim that each confusion class is experienced by a dichromat as merely a more or less desaturated variety of a unique Yellow and Blue. Dotted lines indicate sets of stimuli confused by the relevant kind of dichromat. The ‘confusion point’ (or ‘convergence point’) can be interpreted (for followers of Maxwell and Helmholtz) as corresponding to the ‘primary sensation’ (red for protanopes (left), green for

deuteranopes (*right*)) that is present in normal trichromats and absent in the relevant form of dichromat. (More abstractly, it represents an imaginary supersaturated red (or green) stimulus that, if presented to a normal trichromat, would stimulate exclusively the cone type that the relevant type of dichromat lacks—a red (or green) that the dichromat is wholly ‘blind’ to.) The ‘neutral point’ is the point in the spectrum that looks neutral to the relevant type of dichromat, or that can be matched with *white* (here taken as CIE Illuminant B). The dichromat’s colour-experiences are supposedly merely those that a normal trichromat would typically get from colours lying on the line joining Blue₄₇₀ and Yellow₅₇₅ (marked as a bold line). The normal chromaticity plane is divided into two zones by the solid line running between the ‘confusion point’ and the ‘neutral point’. Stimuli on the line look neutral; those above (and to the right) look yellow, those below (and to the left) look blue. We may think of all the colours represented in the diagram as ‘collapsing’ onto the Yellow-Blue axis along the confusion lines, as indicated by the arrows: the relevant dichromat is supposed to see merely the colour represented at the projection onto that single axis. (For Confusion diagrams like these, see e.g. Le Grand 1957, 332; Hsia & Graham 1965, fig. 4; Hurvich 1981, 194; Wyszecki & Stiles 1982, 464 & 466; they all derive from Pitt 1935, 47 & 49, transposed into CIE *x*, *y* diagram form by Judd 1943, 305 and 1945, 202: cf. also Sharpe et al., 1999, 28; and see §3 below. For an earlier version of the idea, see Maxwell 1855 Fig. 2, and 1860 esp. Figs. 10 & 11; for a fine diagram with one ‘fan’ of confusion lines for protanopes and a second ‘fan’ for deuteranopes, see von Kries 1882, 141. For the specification of the yellow-blue colour-axis, see Judd 1948 and §4 below; again, there is an ancestor of the idea in Maxwell: 1855, esp. 139; 1860, 440. The arrow representation is my own.)

3. The confusion diagrams and their experimental basis

Where do confusion diagrams like this come from? There is an extremely interesting theoretical argument in Helmholtz that I can only touch on here;³⁰ for practical empirical support, Pitt (1935) is usually cited as giving something like definitive evidence. Unfortunately we shall find that it is not quite what it appears to be.

The confusion diagrams like those at the end of §2.3 are extremely attractive and powerful. They systematize a huge amount of material (whether that material is strictly accurate or not), and allow us to make good sense of e.g. the neutral point and patterns of common confusion. The diagrams seem to show particularly clearly the relation of dichromatic systems to trichromatic systems (as ‘reduction’ systems, rather than (as with anomalous trichromacy) ‘transformation’ or modification systems (for these terms, see Nagel 1908a)). And if a person asks why the loss of a long-wave receptor should affect perception of green as well as red, the relevant diagram offers a helpful answer: things lying on any protanope confusion line are discriminated by normal trichromats only thanks to their possession of the L-cones (which effectively tells them *how close* (on the diagram) a stimulus is to the *protan confusion point* (i.e. to the point representing the L-cone)): hence, if a green and a red lie at opposite ends of a single confusion line, someone lacking an L-receptor cannot be expected to be able to discriminate them. In general, we should expect loss of ‘red-’cones to affect the recognition of greens no less than of reds—which is one reason why ‘L-cone’ is a better term than ‘red-cone’, and why von Kries’s term *protanopia* is better than the older term ‘red-blindness’.

But are the diagrams in fact correct? How much are they based, so to speak, on relatively direct evidence, and how much on elaborate theory? The main source is Pitt's 1935 study, which presents diagrams that (when converted into the CIE system) are equivalent to those given above in Fig. 6 (except for the labelling of 'yellow' and 'blue' regions and the hue-line that I have inserted, following Judd 1948). Pitt's diagrams even include the chromaticities for dozens of paint-colours in some standard illuminant. And one might easily be tempted to read off from them such claims as that (except perhaps for a difference in lightness) a deuteranope would find indistinguishable the Purple and Light Green paints in Pitt's confusion class 19, or the Yellow, Orange and Red in class 27.

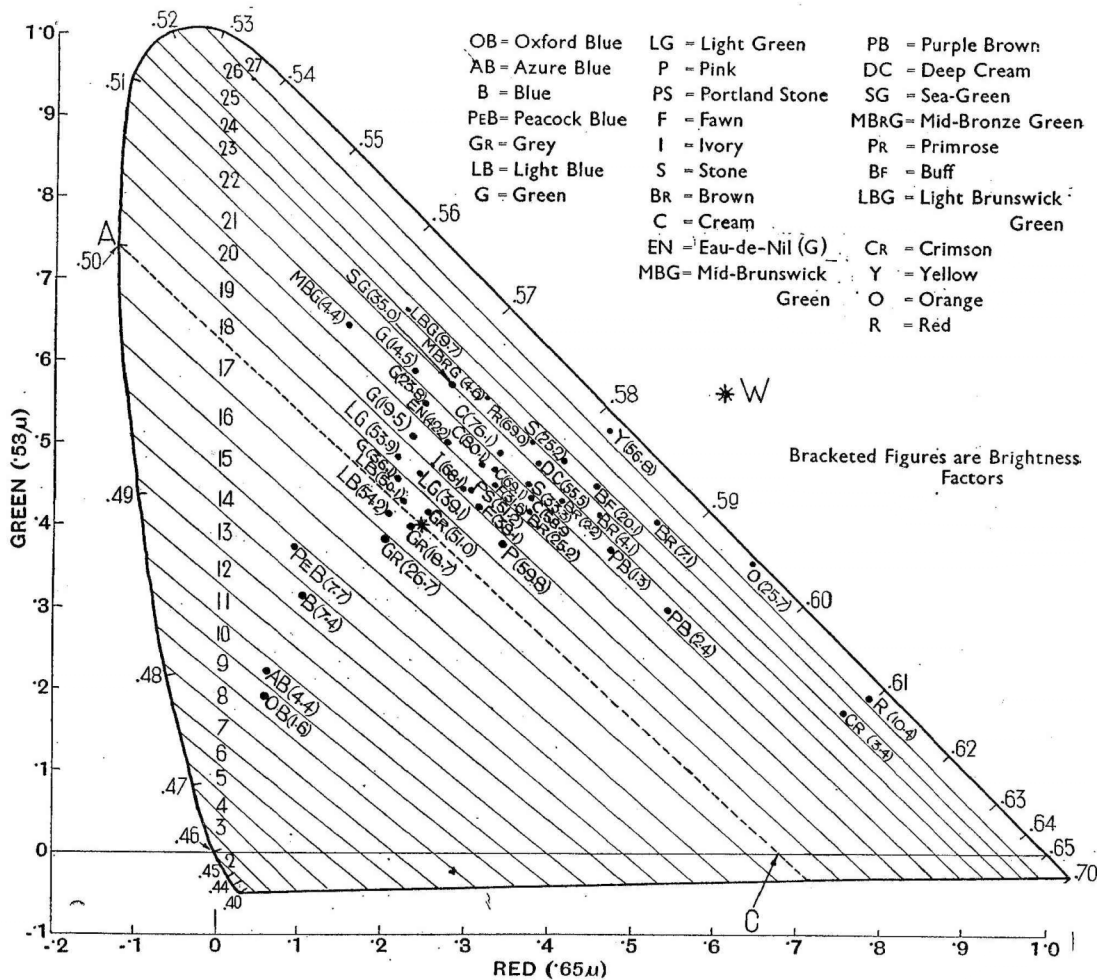


Fig. 7. Trichromat chromaticities and Deuteranope confusions: Pitt 1935, 47. The diagram can be thought of as specifying (in W. D. Wright's system) the proportions of three lights, Red₆₅₀, Green₅₃₀ and Blue₄₆₀ (represented at (1, 0), (0, 1) and (0, 0) respectively), needed in a mixture to match any given light stimulus (for a normal trichromat). The plectrum-like curve shows the mixtures required to match pure monochromatic light of various wavelengths through the spectrum (and, along the straight line at the bottom, running from 400 to 700 nm, the purples produced by mixing violet and red from the extremes of the spectrum). (Cp. Wright 1946, chs. 3 & 4; cp. Fig. 3 above.) Each of the many 'confusion lines' is supposed to join stimuli that (except perhaps for brightness) would look the same to a deuteranope: Pitt has spaced them at appropriate distances to yield 27 confusion classes. The experimental data behind the diagram are the deuteranope matching of spectral hues (on the plectrum curve) with particular mixtures of

Blue₄₆₀ and Red₆₅₀ (on the horizontal axis, Green₅₃₀ = 0). For example, 480 nm is matched (by Pitt's deuteranopes) with a mixture in the proportions 0.205 R₆₅₀ 0.795 B₄₆₀; 494 nm is matched with 0.50 R₆₅₀ 0.50 B₄₆₀; and 500 nm is matched with 0.68 R₆₅₀ 0.32 B₄₆₀ — and the dotted line joining those last two points also goes through the point for White (CIE Standard Illuminant B, marked with an asterisk * near the centre of the diagram): 500 nm is a 'neutral point' for Pitt's deuteranopes, a part of the spectrum that looks white. Letters (decoded in the list at the top right) mark the chromaticities of some 44 paints (as identified for normal trichromats: figures supplied to Pitt by the UK National Physical Laboratory). The confusion lines converge at about $r = 3.15$, $g = -2.28$ in this representation. Transformed into the CIE 1931 system, the diagram takes the form of the x, y diagram for deuteranopes given at the end of §2.3, and the lines converge at about $x = 1.08$, $y = -0.08$. (Pitt 1935, 49 gives a similar diagram for protanopes, which corresponds, in the CIE system, to the diagram for protanopes at the end of §2.3.)

The fact is that Pitt's own experimental data on dichromat confusions involved only lights, not surface colours; and among lights, only two rather restricted sets. What Pitt tested was simply which Blue₄₆₀-and-Red₆₅₀ mixtures could be found to match (for a relevant dichromat) each of a series of spectral lights, about 30 in number, selected at intervals through the spectrum (1935, 14-17, esp. Fig. 4)—as viewed in the Wright colorimeter, in which two adjacent fields of light are compared, each being a rectangle of about 1° by 2° (Pitt 1935, 8; Wright 1946 chs. 3 and 4). The data all concern small fields, not large, and *lights*, not surface colours, and (among lights) only two particular ranges. And at relatively low levels of brightness.³¹ To draw any conclusions about confusions of paints—or even larger or brighter fields of light—would be to draw consequences from Pitt's data only in conjunction with large theoretical assumptions (e.g. that real dichromats fit the Helmholtz-Maxwell model): it is not an experimental finding. And (as we shall see in §5) there is plenty of evidence that those theoretical assumptions are actually untrue of many, indeed most, of the dichromats of the real world. Whatever the explanation may be, it seems that the confusions represented in these diagrams are just not usually made if the fields are larger than about 4°, or under other more favourable conditions. Research like Pitt's has given us genuine guidance on the patterns of insensitivity found in dichromats under rather extreme and special conditions—which in turn are a guide to the sensitivities of the various types of cone. But it would be a mistake to take that as telling us anything very firm, either about the discriminatory capacities of dichromats under more relaxed and ordinary conditions, or about the character and limits of the full range of their colour-experience.

There is a second problem—small in itself, but indicative of larger difficulties of principle. There is a decent level of agreement among experimenters on the position of the protanope confusion point—usually placed at about $x = 0.747$, $y = 0.253$, just beyond the red end of the spectrum.³² But when it comes to a deuteranope confusion point, researchers seem only to come up with data that imply quite different positions—disagreeing with each other and with themselves so much, indeed, that one might wonder if there is any such point to be found at all. Here is a small selection of values from different sources.³³

Deuteranope confusion point

The confusion diagrams and their experimental basis

What Pitt 1944 takes his 1935 data to indicate ³⁴	$x = 1.28, y = -0.25$ (i.e. $r = 9.5, g = -8.5$)
What Pitt 1944 would like his 1935 data to indicate	$x = 1.413, y = -0.413$ (i.e. $r = \infty, g = \infty$)
What Pitt's data actually indicate (JB's calculation from 6 pairs of data points)	$x = \sim 1.06, y = \sim -0.075$ (i.e. $r = \sim 3.15, g = \sim -2.28$)
Some other researchers:	
Farnsworth 1954, using colour chips (as reported by Hsia & Graham 1965, 209) ³⁵	$x = 0.90, y = 0.00$
Farnsworth, for a range of subjects (as reported by von Schelling 1960 ³⁶)	<i>varying on a line between</i> $x = 1.35, y = -0.35$ and $x = 1.75, y = -0.75$
Nuberg & Yustova (1958, ii.480) ³⁷	$x = 1.70, y = -0.70$
Vos 1978, Walraven 1974; Smith & Pokorny 1972 ³⁸ (cf. Wyszecki & Stiles 1982, 614-15)	$x = 1.4000, y = -0.4000$
Estévez 1979 (see Hunt 1987, 62-63; 1998, 61-62)	$x = 2.306, y = -1.305$ ($u' = -0.534, v' = 0.680$)
Wyszecki & Stiles 1982, 464	$x = 1.080, y = -0.080$

If we want any provisional or illustrative figures—and we will need some for the modelling in §6—then the figures from Wyszecki & Stiles in the final row are probably as good as any. (The figures are derived from Pitt's data, via Judd 1945, 202;³⁹ my own analysis of Pitt's data, in row 3, agrees well with these figures.) But it would be a mistake to attach any great reliability to them.

Given that good experimenters seem only to come up with such conflicting views, we might wonder if there is really a well-defined question to which they are giving such different answers. The five deuteranopes in Scheibner & Boynton 1968, for example—to mention yet another available guide—agree well among themselves on their 'neutral point', which implies (by my calculation) a deutan confusion point at about $x = 1.62, y = -0.62$; but they agree dreadfully with Pitt's deuteranopes. One possibility is that in such disagreements the conditions or the task are in some way different in the various studies. Maybe the subjects' macular pigmentation is different; maybe the adaptation level is different, the size or brightness of the fields, the amount of time the fields are exposed, the manner in which the two fields were presented (side-by-side or successively; with a surround or not). And there are different mathematical methods: e.g. most theorists, like Pitt, ignore brightness and concentrate only on the chromaticities of lights confused and therefore need two confusion lines to get a confusion point; but Yustova employs a method of Maxwell's that makes inferences from a single confusion line, taking into account the exact brightness levels in a match too, calculating the position on that single line of a stimulus that would have zero luminance (to the

dichromat). But if the variations in experimental and mathematical method can move the deuteranope ‘confusion point’ from $x = 1.08$, $y = -0.08$ to $x = 1.70$, $y = -0.70$ and again to $x = 2.306$, $y = -1.305$, then we must ask whether there is even a well-defined topic that is the subject of their disagreement. The implied differences over which light stimuli a deuteranope will in practice confuse are often relatively small, but the placing of the M-fundamental is importantly different. We must ask whether single and definite confusion points are there to be found at all: in view of the genetic variation within the class of ‘normal’ observers, we should be aware that the ‘normal’ chromaticity diagram may need to be different for one subgroup and for another; among dichromats, we may be able to find a definite confusion point only for a subgroup of people that all have the same receptor types; and, to the extent that additional factors are at work (e.g. the macula, and perhaps—though, as we shall see, the candidates are not all equally likely—rods or additional cone-types, or other retinal inhomogeneities), we can only expect people to be unhappy with a match that they ‘ought’ to find acceptable, when those additional factors allow a pair of supposedly matching fields actually to be differentiated. And, above all, we must remember that—if the evidence that I have mentioned before is correct—the kinds of confusion illustrated in these diagrams are indeed made with small light fields of 1° or so, but they simply do not usually occur with larger fields—or even, reliably, with smallish fields of surface colour. So these diagrams are going to be of little use in telling us the *full range* of ‘what a dichromat sees’. This is not to object to the attempt scientifically to separate out different factors and study them separately; it is to say that the component factors need much more careful separation than they have received so far, and that we cannot expect that, when we put all the factors together, the confusions of dichromats will be very thoroughly captured by a traditional confusion diagram. I leave for a footnote a similar cry of protest from a distinguished researcher of nearly 50 years ago.⁴⁰

4. The Unilateral Cases—and Nagel’s remarkable reports

Supposing we read the confusion diagrams as implying the collapse of the range of hues to just two, how would we know which hues they were? Studies of colour-blindness in one eye are supposed to tell us. In those rare cases where a person has one colour-blind eye and another that is normal or near-normal, the hope is that the more normal eye can be used to calibrate or specify the perceptions had with the other eye: the person might for example, *point* to colour-samples (e.g. on a colour-chart) that look to the normal eye the same in colour as certain controlled stimuli presented to the colour-blind eye; or he might simply *tell* us what colour particular things look when he sees them with his colour-blind eye—and the fact that he has one good eye could serve as evidence that he means by the colour terms pretty much the same as normal perceivers do. As it happens, the classic survey of such cases by Deane Judd (1948) reaches the firm conclusion that the main groups of dichromats see just a unique yellow and a unique blue, corresponding to 575 and 470 nm.⁴¹ As a report selecting, out of 40 original articles, spanning 89 years and several

languages, a core group of ten for detailed evaluation, Judd's article might seem a paradigm of diligent judgment. On deeper investigation, it turns out, I think, to be nothing of the kind.

It is important to acknowledge immediately the remarkable fact that there are dichromats who themselves talk of seeing only yellow and blue (and neutral) in the spectrum, or indeed under any circumstances at all. (Scott 1778 and Pole 1859 are, respectively, perhaps the first and the most remarkable among them.⁴²) But we have already seen evidence from dichromats who believe they see more than those two hues (like Dalton 1798 referring to what he saw in candlelight: §1.1), and we will have to consider whether they might in fact be right. And we must remember that we cannot assume that whatever is true of *some* protanopes or deuteranopes is also true of all: there may be individual differences, or important subgroups that have not yet been properly distinguished—varying in genetic endowment, or in the ways they may have developed means to 'compensate' in part for their receptor loss.

Here is a version of Judd's table of the ten most reliable reports. Judd gives the impression that—with only three small deviations that he can deal with (marked in my table with an asterisk), the original articles all point in the direction of what I shall call the 'yellow-and-blue view' of dichromat experience. I shall save a fuller evaluation for another occasion. But if we go back to the original reports, we get a very different impression. One author in the group, Samuel Hayes, reaches—from a review of the literature and the investigation of his own subjects—a conclusion diametrically opposite to Judd's:

The statement that sensations of blue and of yellow alone are possible to the partially color-blind *cannot be reconciled* with our findings. (Hayes 1911, 402, my emphasis)

In fact, the main point of Hayes's study is to claim that his own unilateral colour-blind subject (a unilateral protanope, he believes) sees green in addition to yellow and blue, as also do many other protanopes: 'green (as a specific color quality different from yellow and grey) is included in the color system of her protanopic (right) eye' (380).⁴³ As for the reports in the earlier literature, Hayes says:

Our review of the monocular cases ... showed that, with the exception of von Hippel's, they reveal nothing but meagre experimentation, glaring contradiction, and theoretical bias. (1911, 404)

My own view is that Hayes is much too generous to treat von Hippel's case as an exception to this general judgment. We shall come back to this in a moment.

The Unilateral Cases—and Nagel’s remarkable reports

Author, date	Classification of the defect		Indicated perceptions	
	Author's own	Present [i.e. Judd's]		
a. Unilateral dichromacy				
Hippel 1880	Deuteranopia	Protanopia	Yellow	Blue
Holmgren 1881	Protanopia	Protanopia	Greenish yellow	Violet blue
*Hippel 1881	Deuteranopia	Protanopia	*Like 589 nm	Blue
Hayes 1911	Protanopia	Protanomaly	Yellow	Blue
*Sloan 1947	Deuteranopia	Deuteranopia	Like 5Y 5/5 [coloured paper with dominant wavelength 575 nm], also [spectral light of] *584 nm	Like 3PB 5/5 [coloured paper with dominant wavelength 478 nm], also [spectral light of] *452 nm
b. Dichromatic Fovea, with near-normal periphery				
Nagel 1905	Deuteranopia	Deuteranopia	Yellow	Blue
c. Unilateral anomalous trichromacy				
von Kries 1919	Deuteranomaly	Deuteranomaly	Like 573 nm	Like 464 to 480 nm
d. Unilateral acquired deficits				
Hering 1890	Approach to deuteranopia	Approach to deuteranopia	Yellow	Blue
Hess 1890	Approach to deuteranopia	Approach to deuteranopia	Like 575 nm	Like 471 nm
*Goldschmidt 1919	Protanomaly	Protanomaly	*Orange, yellow	Blue

Table 2. ‘Cases of unilateral defect of vision giving information regarding protanopic and deuteranopic color perceptions’ (Judd 1948, 254, with modifications⁴⁴). Judd wants us to conclude that deuteranopes and protanopes see only a yellow of about 575 nm and a blue of about 470 nm. I have marked with an asterisk the authors and relevant claims in three reports that Judd describes as ‘exceptions’ to his hypothesis.

The cases surveyed in Judd’s review are actually of four very different types. The last group, (d) the acquired deficits, is, I think, of absolutely no value to us. Even if we knew in those cases what colour-sensations were or were not had with the affected eye, it would be entirely unclear what implications that might have for ordinary cases of congenital dichromacy. In two of the three cases that Judd studies, the deficits derive apparently from some kind of optic nerve atrophy or neuropathy (and in the third case, from a gunshot wound): those are certainly not fundamentally similar to a lack of L- or M-receptors in the retina, and we would need independent reasons to be justified in taking the former kind of pathology to tell us anything much about the precise limits of experience in the latter kind of deficiency. What is more, even if there were a physiological similarity, an *acquired* deficit might or might not tell us about experience in a *congenital* case.⁴⁵

What might really help us are (a) the supposed cases of unilateral dichromacy. The best evidence Judd has for his 'yellow-and-blue' view is probably the case of a 17-year old man who was the subject of three reports, by Hoppel (1880), Holmgren (1881) and again Hoppel (1881). His right eye suffered from a marked 'red-green blindness' (and a bad squint uncorrected by several operations⁴⁶), while the other eye was apparently normal. But there are disagreements between Hoppel and Holmgren (and between the two of them and Judd)—and both original authors show the strain of making the case conform to their theoretical predilections. They both say that the man sees only two hues with his weak eye, but do not agree on which hues they are. Hoppel, as a supporter of Hering, wants the hues to be yellow and blue; Holmgren, as a supporter of broadly Helmholtzian ideas, expects them to be a variety of green and of violet. Hoppel (1881, 50) reports that the colours experienced correspond to 589 nm and 450-460 nm (which are actually a slightly *orange* yellow and a violet blue). Holmgren (1881, 306) claims instead that they are a *greenish* yellow and an 'indigo-violet'. Judd, some 65 years later, seems to think he can, so to speak, split the difference on the yellows and bleach any violet tinge out of the blues—and declares the colours to be unique yellow and unique blue. I think it is doubtful, on closer investigation, that the boy saw only two hues at all with his dichromatic eye. The best evidence that Hoppel presents is simply that 'yellow' and 'blue' are the only colours that the subject reports seeing, when presented, under controlled conditions, with particular stimuli of various colours. Spectral lights, when presented in isolation in a spectroscope in 1 nm widths, are described only as 'Yellow' or 'Blue' (1880, 180). Certain annular portions of rotating disks that look green and red to the normal eye are described as merely looking yellow and blue to the dichromatic eye (182). There are similar results with Dor's plates and some pieces of red and green glass.

But there is counter-evidence in the article that suggests an opposite conclusion. Under other conditions, the man reports plenty of other colours with his supposedly dichromatic eye. Looking at a full spectrum, he reports '*red*', 'yellow or *green*', and 'blue' (Hoppel 1880, 180, my emphasis). When asked to name the colours in the Index to Radde's Scale (a colour-order system of the late 19th century), he performs the task 'fairly correctly' when presented with the whole Index page of 42 different samples (which he certainly cannot have done using just the terms 'yellow' and 'blue'). When the 30 colours in the higher saturation series in Radde's Index are presented to his colour-blind eye, separately and out of sequence, he uses the terms 'red', 'green' or 'greenish', 'yellow' and 'blue' pretty much for red, green, yellow and blue and calls none of the greens red or reds green.⁴⁷ He makes some serious mistakes: he calls the orange-red and orange samples 'yellow' and calls some of the greens 'yellow'. His colour-vision is obviously seriously deficient. But he still seems to have a far from negligible capacity for red-green discrimination and his use of the terms is surely *prima facie* evidence that things sometimes looked red or greenish to him with his colour-blind eye. (Remember the premise of the whole project: that the subject knew from his good eye what the standard meaning of these terms was.) There are many further problems that I shall not go into here (especially with the tests with a spectroscope that Hoppel reports), but it should be obvious that the claims of Hoppel and Holmgren are nowhere near being established by the evidence

they give. They are conclusions they can have felt entitled to draw only given a commitment to their own general *theories*—though the theories were incompatible with each other and neither was in any position to claim evidence-trumping authority.

Perhaps the most interesting detail here, however, is in the man's performance with the low saturation samples (nos. 31.-42.) in Radde's Index: when they are presented individually and separately, he fails to recognize any red or green tinge in any of them. But there is one exception: he describes 31. Neutral Grey (along with the pale orange-red 32) as 'light green'. If this is true, it may well be a case of a kind of colour *induction* and it may be a sign of processes at work more generally: it may be, not that the relevant aspect of colour is *bleached out* of the dichromat's experience, but rather that it is, so to speak, *painted in*, and sometimes painted in incorrectly. It would be a mistake to draw any firm conclusion from the fragmentary information in the present case; but the hypothesis would fit quite well with what we know of other operations of our other sensory systems. The partially deaf sometimes become hypersensitive to sound, the wholly deaf may hallucinate sounds; there are, it seems, something like automatic gain controls in some of our sensory systems that turn up the amplification (as well as working to harness ancillary sources of information) where there is low amplitude or reduced variation in sensory signal input. In cases of anomalous trichromacy, there is a phenomenon of *exaggerated* contrast noted by Nagel (1908b, 24-25); and recently—with normal trichromats—Richard Brown and Don MacLeod have talked of 'gamut expansion' in cases where pale colour patches are seen against a uniform grey background. (Colour patches that look pale when seen against a brightly coloured background, for example, look richer in colour when seen against a neutral grey. (Brown & MacLeod 1997, 845; for some caveats, see also Faul et al., 2007.) This may be a by-product of constancy mechanisms for dealing with fog and similar conditions that dilute colours generally, or of central adaptations to a general weakening of peripheral sensory input.) The message of the cases of anomalous trichromacy may be not how much is *lost* (the implication therefore being, as Judd believes, a total loss of red-green perception in cases of full dichromacy) but rather, how much is *saved*—as the visual system works to compensate for loss of information, sometimes (as when pale grey is reported as looking green) inaccurately.

I shall pass quickly over the Hayes (1911) case—which may or may not be a genuine case of unilateral dichromacy. The subject is clearly reported to see green with the affected eye, so if the eye is (as Hayes tells us) protanopic, then it seems a counter-example to Judd. On the other hand there is certainly a possibility that the eye was merely protanomalous. (Hayes seems to have a slightly impressionistic grasp of the distinction between protanomaly and protanopia. And he was certainly in no position to make a firm differential diagnosis between the two conditions with the equipment he had available—spinning tops, but no anomaloscope or spectral mixing apparatus.) If the eye is only protanomalous, then it belongs with category (c)—the cases of anomalous trichromacy—and, as we shall see, under that heading too, it lends no

support to the yellow-and-blue view. One can conclude very little from the case, but either way, it gives no support to Judd.

The third and last case of unilateral dichromacy that Judd mentions might seem more helpful and more rigorous. Sloan & Wollach⁴⁸ (1947 and 1948) announce that their experiments show that ‘the color perceptions of the [subject’s] dichromatic right eye include only blues, yellow, and the white-black series’ (1948, 508)—which sounds like support for Judd. Unfortunately, the experiments show no such thing. To compare the perceptions of the subject’s two eyes, the investigators run two main series of experiments, one with Munsell papers (i.e. pieces of coloured paper meeting specifications in the Munsell system) and the other with spectral lights in a photometer. The tests using coloured Munsell papers that the investigators report involve only four narrow ranges of colour samples: there are four blues, three yellows (or rather, browns), three bluish-greens and three purples—selected with the aim of discovering which of the yellows and blues looked the same to the two eyes, and which of the blue-greens and purples look more or less neutral to the dichromat eye. With spectral light stimuli—selected for the same purposes—, the main tests involve only four blues, three yellows and three bluish-greens. How on earth is that supposed to tell us the *full range* of the subject’s colour-experience with the eye in question? The authors are not *testing* whether the experience with the bad eye included merely two hues; they are *assuming* that it included only two hues, and are simply trying to identify *which* blue and *which* yellow the two supposed hues might be.

The authors do tell us that ‘[1] In the spectrum and [2] in the Munsell 100-hue series [i.e. the colours of the Farnsworth-Munsell 100-hue test], *the subject reported that with the right eye he saw only blues and yellows*’ (1948, 509, my emphasis). But we are not told of any effort being put into making an experimental test of whether that very general report from the subject was actually true. Even if it was true, it would not establish the conclusion the authors want. We know (1) that some people (like Dalton himself) report seeing only yellow and blue (and maybe purple) *in the spectrum*, but report other hues *in other circumstances*. What is more, (2) the Munsell papers that Sloan & Wollach used in their experiments were all are of medium-low saturation and lightness: they are (approximately) the colours that Farnsworth used in his ‘100-hue test’. But those colours were chosen by Farnsworth not for bringing out the maximum range of colour-sensation in a dichromat, but as samples that would present colour-blind people with maximum difficulty, in order to facilitate quick diagnosis of their weaknesses. So even if it were true that the subject saw only yellows and blues (1) from the spectrum and (2) from the Munsell colours presented to him, that would not tell us that he *never* saw any other hues under more favourable conditions. The Munsell papers presented were, to repeat, of medium-low lightness and saturation⁴⁹ (as is evident from the fact that with the good eye, some of them were described as ‘pinkish-blue’ and ‘brown’). Where were the bright reds and vivid deep greens?! We know from other sources (that we will review in §5) that, if there is any residual perception of red and green among dichromats, then these medium-low saturation colours are among the kinds of sample *least* likely to reveal it. The Munsell paper samples were 2-inch squares—which at 40-60 cm would subtend about 5°–7° of visual angle: whereas we know that any residual red-green discrimination

is better when the samples are larger. Finally, we are told that the illuminant for the papers was 'artificial daylight (Macbeth daylight lamp)' (Sloan & Wollach 1948, 507). One should not be misled by the label. The light of a Macbeth daylight lamp is not much like ordinary daylight: it is a small desk lamp with a metal dish reflector behind the tungsten filament bulb, and a blue filter in front, which cuts out nearly all of the red, orange, and yellow light, and much of the green, so as to transmit a spectral profile like that of daylight of ~6000 K. The result is a hard and dim light compared with that of a 100-watt bulb, or with real daylight; and it shines directly upon the samples to be viewed, with minimal reflection from walls, table etc. It has, for diagnostic purposes, the significant advantage, in comparison with redder and brighter lights, of increasing the number of errors made by colour-blind people on standard tests (Schmidt 1952). Precisely for that reason, it is not a good light in which to investigate any residual but weak capacities for colour perception.⁵⁰ If Sloan & Wollach's subject had had any perception of red and green, the method of investigation could hardly have been better designed to *avoid* finding it.

I shall say little about (c) the cases of unilateral protanomaly and deuteranomaly—except that it is completely obscure what consequences one might draw from them about the experience of protanopes and deuteranopes. Judd assumes that if in anomalous trichomacy the experience of certain hues is *impaired*, then in full dichromacy the experience of those same hues will be entirely *absent*: but this link proposition is a theoretical claim that may or may not be true. We know indeed that certain models—derived from Helmholtz and Hering—imply this; what we need to know is whether those theoretical models are in fact dependable and empirically secure. As it happens, in the only clear report of a unilateral anomalous trichomat that Judd takes up—von Kries's 1919 report on a man with one deuteranomalous eye—, it was, it seems, *not* the sensations of red and green that were maximally impaired, but rather those of green and yellow, followed by red and blue, in that order. What is more, the reduction in the intensity of sensation of both red and green was (so von Kries says) 'surprisingly slight' (150). On Judd's own principles, the conclusion to draw from the case would be that dichromats, if they experienced merely two hues, would experience red and blue! But von Kries's report might also be taken as evidence that there is no neat segmentation of hues into pairs, one of which would totally disappear in cases of dichromacy at all.

As for Hayes's case: if the affected eye is after all merely protanomalous (rather than protanopic, as Hayes had thought), then it is unsurprising that some, perhaps weakened, sensation of green is maintained; but we could only conclude that this sensation of green would be wholly *absent* in cases of dichromacy matter given the link proposition I mentioned earlier—and that is a claim of high theory that is put in doubt by much of the evidence. In the absence of that link idea, Hayes constitutes not the slightest support for Judd's main thesis; and either with or without that idea, von Kries constitutes counter-evidence for that thesis.

This leaves (b) the remarkable case in Nagel (1905) of the train driver 'Sch.', who is described as having a dichromatic fovea and trichromatic periphery. (This is a case, therefore, not of one dichromatic

eye and one normal eye, but of two eyes, each apparently with one dichromatic region—centrally—and another nearly normal region, in the periphery.) It is something of an outrage that Judd presents this as being a report of a person who has sensations of *only blue and yellow* with the dichromatic part of his eye. There are three things to say about the report and its evidential value.

1. The article in no sense reports Yellow and Blue as the sole 'indicated perceptions' for the dichromatic part of the eye. There is indeed a comment on the train-driver's perceptions of yellow, which we shall come to in a moment, but the article contains not the slightest mention at any point of the presence or absence of perceptions of blue—indeed it contains no occurrence of the word 'blue' (or *blau* or any suitable equivalent) at all.⁵¹

2. On the subject of yellow, the one relevant comment is Nagel's remark that it is 'theoretically important ... that Sch. calls spectral red and yellow-green, in foveal vision, yellow' (1905, 100). But this must be taken with Nagel's report that Sch., on other occasions, also called those same spectral stimuli either *red* or *green* (1905, 94).⁵² It is perfectly possible (Nagel's report is not entirely explicit) that the man had a variety of sensations from stimuli in the long-wave part of the spectrum—of yellow, red and green, depending upon context, size, relative lightness and other factors.

3. Most importantly, Nagel's article actually describes not one case but two—the train-driver Sch., and Nagel himself, presented as an ordinary case of deuteranopia—though one that is far from fitting the standard theoretical conceptions about that condition. Let me explain. Having finally been forced to conclude that Sch.'s periphery must be normal or near-normal (on the basis of his excellent practical performance), Nagel then comments: 'It must be said that in establishing such a diagnosis [of a normal or near-normal periphery] the most extreme caution is called for, since one might be tempted to contemplate something similar in case of a very large number of dichromats, especially deuteranopes.' (Nagel 1905, 97) What Nagel means is that many dichromats perform excellently when tested on more than just a small foveal region of vision—without that being a reason to say they have a trichromatic periphery after all. And here Nagel presents the most remarkable finding of the whole article: that dichromats like himself can in large fields recognize red, in addition to yellow and blue.

The claim is of great importance, and it leads to some yet more important further statements, that get not the slightest attention from Judd. Nagel says that, for a green light, he can always produce in the laboratory—though it is not easy—another colour that a dichromat will confuse with it: namely, some kind of yellow (or brown) or white (or grey). (Fig. 6 above may make the expected equivalences clearer.) On the other hand:

Not so with Red. I find it absolutely impossible to get a satisfactory match between vivid Red and Green or Brown, as long as the ... field size [is] of at least 10°. (Nagel 1905, 97)

A few years later, he has additional evidence:

Among 30 dichromats of both types (protanopes and deuteranopes) that I have examined since [a few years ago], I have found *not one* who displayed a dichromatic system also in *large-field vision*. Without exception they were able with large areas to *recognize the colour red with complete certainty* in all nuances, even at quite low saturation. With areas of a size of, say, 1/2 to 3/4 square metres, observed from a distance of 1/2 to 3/4 m, the so-called dichromat recognizes such low degrees of saturation of red (even when mixed with unsaturated yellow or blue), that he seems at first to do only slightly worse than the normal. (Nagel 1910, 6, my emphasis)

If red is recognized at a variety of levels of brightness and saturation—and is not matchable with any mixtures of yellow and blue—the obvious implication is that it is not producing merely some variety of sensation of yellow or blue. And Nagel says so explicitly in the same article: he knows very well what it is like to see the world only in yellow and blue. But that is not at all his normal experience. He can bring about that extraordinary reduced condition, by wearing red filter capsules for half-an-hour or so: having flooded the eye with so much red, he finds, on taking off the filters, that his sensation of red is 'exhausted' for about a minute:

It is a most remarkable experience, to see the environment thus in the way in which the dichromat is supposedly bound to see it, so others say, but in which I have *never seen it before*: totally without the colour red. Red objects appear, even when they are quite large, exactly as they otherwise look only in foveal view—brown [i.e. dark yellow], yellow, grey or blue, according to the exact shade of the red. The effect lasts for about a minute after wearing the filter capsule for half an hour, and two minutes after exhaustion for one hour. The red sensation then returns slowly afterwards and is normal again after about five minutes. (Nagel 1910, 10, quoting from Nagel 1907d, my emphasis)

It is puzzling that this second article ('Farbenumstimmung beim Dichromaten', *Zeitschrift für Sinnesphysiologie* 1910) is mentioned in the bibliography to Judd's 1948 paper. One might be tempted to say that Judd shows not the slightest sign of having understood it (whether or not he might ultimately accept it). Judd talks in all innocence of the unlikelihood that his yellow-and-blue view might even ever be *challenged*.⁵³ In fact, the existence of a challenge is not an unlikely possibility, but, rather, an actual and evident fact—staring him in the face from the pages of one of the articles that he makes a pretence of disinterestedly surveying. He says not one word about these challenges: it is not that he mentions them and answers them casually—he simply shows no sign of even seeing them.

Judd is a serious author who made very substantial contributions to colour science. A Cornell Ph. D., he spent most of his career at the U.S. National Bureau of Standards, during a period when much of the most exciting work on colour vision arose out of technical demands and practical needs; he was one of main developers of the colorimetric systems of the *Commission internationale de l'éclairage* (CIE) and may have been the first to plot an (x, y) diagram (Judd 1952, 108n.).⁵⁴ But there is something bizarre about

his treatment of this topic. It is serious in style, the work of a person of authority, and clearly the result of a lot of hard work. But it is full of misunderstandings.⁵⁵ It has the air of presenting a range of rival views—acknowledging, for example, that Holmgren and Hippel disagree on which two hues their 17-year old subject may have seen; but it simply never takes seriously the no less present disagreement on the prior question of whether dichromats can be expected to see merely two hues at all. On that, Nagel and Hayes were already explicit opponents, and von Kries had presented evidence (in the 1919 article) that could be used to raise doubt. Judd talks of them as if they either already were or could easily be turned into supporters of his own cause.⁵⁶ They were not, and to pretend that they were is to treat them with something like indifference or disdain. The condescension is puzzling given that Nagel and von Kries, at least, were figures of intelligence, distinction and scientific independence of mind, with medical training as well as many of the instincts of a physicist, who had spent years of their lives working with actual cases of colour-blindness. And in neither technical nor theoretical domains had there been any particularly radical advance between their day and Judd's. The anomaloscope that Nagel had invented was still at least as good as any of its rivals forty years later (Willis and Farnsworth 1952). And the 'zone' theory, a later version of which Judd himself supported, had itself been proposed by von Kries in 1882, with not merely the general idea of a stage of neural processing subsequent to the (largely chemical) activity of the retina, but even the precise and prescient conception of quotients or log differences to capture the structure of subjective colour—with something like (L / M) or $\beta (\log L - \log M)$ to correspond to the opposition of red and green (von Kries 1882, 62, 163-4; 1905, 269).

Let us look again at Judd's table and ask what would be put in the last two columns if we recorded not '*Indicated* perceptions' but '*Reported* perceptions'. ('Indicated' seems in Judd to mean: 'either reported, or implied by something (perhaps minimal) in the report taken together with my own theory'.) Any sign of support for Judd's yellow-and-blue view, I think, disappears. To take (a) the three cases that have a claim to be unilateral dichromats: the Hippel/Holmgren/Hippel patient certainly reported things as 'red', 'greenish' and 'light green'; Hayes's subject G.W. certainly reported things as looking green (though she may or may not have been truly protanopic); and Sloan & Wollach in their investigations employed methods that, if their subject had had any sensation of red and green with his dichromat eye, might almost have been designed to *fail* to reveal it. Meanwhile, (b) Nagel's report is completely silent about the general range of sensation of his train-driver; and when he does make general claims about dichromats (himself included), he actually says they recognize and have sensations of red (Nagel 1905, 1907, 1910). We might add that (c) von Kries's anomalous trichromat retained the sensation of red, it seems, more strongly than that of yellow, and in any case lost sensation of both red and green less than one might expect. And we might remember that (d) the acquired deficits are of no primary evidential value for us on congenital dichromacy. Altogether, these reports seem to me to constitute a case not of moderate or *weak* evidence for the yellow-and-blue view, but of *no* evidence for it at all. What we have instead is a number of authors firmly wedded to a yellow-and-blue theory—Hering, and his followers Hess, and Hippel, and later, Sloan

& Wollach and Judd himself—, sometimes to the point of stamping out the evidence that they themselves had let slip, that it could not quite be correct.⁵⁷ There may indeed be some truth in that view; but I think it is entirely unclear from the evidence we have seen what truth exactly it might be.

I know of only three main cases of unilateral colour blindness from later years, and they are worth examining in more detail than I can give here. The Graham & Hsia case (e.g. Graham et al. 1961) certainly seems to involve the perception of merely yellow and blue; but it is not a case (in the affected eye) of any of the recognized forms of congenital dichromacy.⁵⁸ It is in fact entirely obscure quite what the underlying nature or cause of the subject's colour-blindness may have been—and how it might be a guide to the experience of people with any of the more usual kinds of dichromacy.

The unilateral case reported by MacLeod & Lennie 1976 is extremely interesting: it seems to be a case of a deuteranopic eye that provides sensation of only two principal colours, but the colours reported are a *greenish* blue and a *reddish* yellow (corresponding to 473–474 nm and 610 nm)—which fits the theories of neither Helmholtz nor Hering (nor Judd, who wanted Blue₄₇₀ and Yellow₅₇₅). And, more remarkably, the authors find that the nearest thing to a 'neutral point' for the subject's dichromatic eye is probably greenish, rather than pure white: so (with some other evidence too) they conclude that, the subject's sensations using that eye are reduced to the colours represented on a line in normal chromaticity space running between 473–474 nm and 610 nm, but it is not a straight line but an arc 'bowed toward the green corner' (698). (The line would therefore include green-blue, a variety of greens—some very desaturated—, yellow, orange, and orange-red.) This is good evidence against standard views. But when the authors hold that the full range of sensations with the dichromat eye is confined merely to this line in chromaticity space, they go beyond, I believe, what the investigations they report can establish. There is the familiar shortcoming (and similar limitations affect the other two recent reports): that the main investigation involved spectral (or narrow-band) lights in small fields—and it is not at all clear whether, with nonspectral (i.e., mixed) lights, a wider range of test intensities, surface colours, larger fields, and (a very different but important issue) colour presentations involving more complex and varied contexts and 'natural' environments, a wider range of colour experiences might not have been had.

The third case (Alpern, Kitahara & Krantz 1983b) is an acquired tritanopia in one eye. As an acquired tritanopia (following a serous chorio-retinopathy two years earlier), the case is unlikely to tell us much about cases of congenital protanopia or deuteranopia (at least, in advance of our having a much better theoretical understanding of the full variety of cases). But it does have two particularly interesting features, which may alert us to possibilities to test for in other cases. One is that the sensations had with the tritanopic eye (when identified by means of stimuli presented to the normal eye) follow, it seems, not a straight line in chromaticity space between red and green (as the traditional theories claim), but a curve that bends back on itself—there are not just two principal hues experienced, but a whole range, including red, orange, yellow and blue, though, it seems, no green at all.⁵⁹ Secondly, the case is described as one where

change of *brightness* in a stimulus by itself produces change in *hue* sensation. Violet₄₂₂ and Green₅₂₃, we are told, lie on a single confusion line—i.e. have the same chromaticity—for the tritanope eye. But, at the intensities used in the actual testing, the latter looked a *greener* blue than the former—thanks, the authors tell us, to its being, as it happened, brighter.⁶⁰

Though Alpern et al. imply that *level of brightness* can by itself affect hue sensation, they unfortunately do not study brightness as a separate variable in itself. (They notice it only as an incidental by-product of the different brightnesses of the stimuli produced with their apparatus.) Other investigators have given evidence that suggests such effects may be more widely found. Pitt says, of his dichromats: ‘various decreasing intensities of a green (.53μ) were called yellow, yellow, reddish-yellow, red, red’ (1935, 33). Pitt himself does not take the reports at face value: he thinks that, when the dichromat *says* he is seeing yellow, reddish-yellow and red (as the intensity of a Green₅₃₀ stimulus is progressively reduced), all that can be happening is that he is experiencing the same pure yellow hue, at progressively lower levels of brightness—this one hue being successively ‘called by different colour names’ (33). But in the absence of any special theoretical precommitment, it would seem equally open to think that in Pitt’s experimental situation, the Green₅₃₀ stimulus actually produced (when bright) sensations of *yellow*, and (when less bright) sensations of *orange* and finally (at lowest luminance) sensations of *red*. This is just the kind of thing that Alpern et al. provide firm evidence of with their own tritanopic subject, and it might well occur more widely. It is something that we should test for further.

It may be worth adding that the studies of unilateral dichromats suffer from the same kinds of weaknesses as have vitiated much experimentation on dichromats in general. I shall discuss these more fully later (in §9) in the light of some of the more adventurous empirical findings (that we survey in §5), but some of the more general points are these: we need more evidence on performance with large fields as well as small, surfaces as well as lights, at varying levels of saturation, and with such ‘dynamic’ factors as those of seeing a single surface in a variety of kinds of illumination. With such factors ignored, it can only be hazardous to make any claim about the limits within which a unilateral dichromat’s experience is (as Judd puts it: 1948, 247) ‘confined’.

I have already mentioned that we need to know more about the *cause* and nature of these unilateral conditions before we can take a view on how similar the underlying physiology might be to ordinary cases of congenital dichromacy. But, even if we had an ideally good account of the perceptual experience of a unilateral dichromat and had reason to think the underlying physiology was reasonably close to that of an ordinary bilateral dichromat, there would remain important questions about how much the former could tell us about the experience of the latter. The reason is that the presence of a second, normal or near-normal eye may itself have an effect on the kind of experience yielded by a dichromatic eye: there may be influences on neural development and processing in a unilateral case (e.g., to minimize disparity in the experiences associated with the two eyes) that are simply not present in an ordinary bilateral case.

We can be said to have found *not one case* of a representative unilateral dichromat who we can be confident had sensations only of yellow and blue. (Two cases might seem to come close. But the Sloan & Wollach investigation involved only smallish fields and medium-low saturation surfaces, chosen from a tiny range of hues, seen under dim (indeed mesopic) conditions—and it can be no guide to the subject's full range of sensation. And the Graham & Hsia case resembles no known variety of congenital dichromacy at all.) But even if we had found such a case, or came to find one in the future, it would be quite unclear—in the absence of a much better general understanding than we actually have of the nature of both unilateral and bilateral deficits—quite what conclusions we might draw from it about the experience of ordinary bilateral dichromats.

5. The many reports of residual red/green discrimination among dichromats

I have already mentioned the evidence in Jameson & Hurvich 1978 that dichromats make distinctions that, on the standard theory of dichromacy, they should not be able to do. The evidence is not at all isolated; there is evidence from the 1850s and from the early years of the 20th century that was rather shockingly neglected thereafter. And there is recent evidence too, from the last forty years. In fact, odd as it may sound, it is about as close as anything can be to being an established fact in this subject, that dichromats—that is, people classified as such on the basis of, above all, the Nagel anomaloscope—are often quite capable of making distinctions between colours that the standard theory implies should be indistinguishable for them (and that, in the Nagel anomaloscope, genuinely are indistinguishable for them). I shall here just mention a few of the findings. What is striking is, first, how much disagreement there is among experimenters on what might account for these abilities, and, secondly, the extent to which authoritative writing on the subject of colour-blindness still perpetuates the standard theory of it.

I shall here survey just a handful of the most important studies, made by experimenters in the distinguished laboratories of Robert Boynton at UC San Diego (and earlier at the University of Rochester) and of Joel Pokorny and Vivianne Smith at Chicago. The experiments study performance at three main tasks: the colour naming of spectral lights (Scheibner & Boynton 1968, Nagy & Boynton 1979), the colour naming of surface colour samples (Montag & Boynton 1987, Montag 1994), and the matching of spectral lights with mixtures of other lights, as e.g. in Rayleigh matches (Smith & Pokorny 1977, Nagy 1980). The colour-naming studies are all unanimous on the phenomena, though they disagree on the explanation of them: as long as the stimuli are larger fields (e.g. 8° rather than 1°), presented for longer periods of time (e.g. 5 seconds rather than 30 ms), then nearly all ‘dichromats’ prove able to name the colours with some success in the red-green dimension as well as in the yellow-blue dimension—not with the same accuracy as normal trichromats, but with a degree of success that should be totally lacking if the standard theory were right. The colour-matching experiments tell a similar story: while ‘dichromats’ (almost by definition) accept a Rayleigh match of Y_{589} with R_{670} or with G_{545} as long as the fields are small (e.g. 1° in size), it seems (as e.g. in Smith & Pokorny 1977) that they actually reject any such match when the fields are larger, e.g. 8° (which is still only the equivalent of about an 8 cm. diameter circle viewed at 60 cm). That is, the ‘dichromats’ are behaving under these conditions in the same way as some form of trichromat.

How do they manage it? There is no agreement among the authors of these studies, and indeed the evidence does not suggest a single explanation. Rod intrusion? The suggestion is rejected in Scheibner & Boynton 1968, supported after all in Nagy 1980 (at least for certain conditions) and in Montag & Boynton 1987, but rejected again in Montag 1994—for the fairly impressive reason that (in surface colour naming tasks) ‘Using high light levels so that the rods are saturated does not impair performance’ (Montag 1994, 2137).

The other explanation that the experimenters usually turn to instead is that the dichromat may after all have some cones of the type usually supposed to be missing, or perhaps a variant like those found in

anomalous trichromats. Scheibner & Boynton 1968 attribute their subjects' success in naming of 3° spectral fields to cones 'of the type usually considered missing' (1968, 1158). Nagy & Boynton talk of a 'weak residual third cone mechanism' (1979, 1259; cp. Nagy 1980), having ruled out a rod explanation on the ground that their subjects do only slightly worse at giving the colour names of 12° spectral fields five minutes or so after a light bleach than when they have not had a light bleach at all—whereas rods would be expected still to be out of service at that point. (When you 'bleach' the pigments of the eye by exposing it to an extremely bright stimulus (in this case, 4 minutes of white light), the cones recover faster than rods: the authors expect the cones to be back in action 5 to 8 minutes later, while the rods would still be saturated.) Thus, Nagy & Boynton (1979) conclude, many people standardly classified as dichromats are, in a sense, not dichromats at all. Montag & Boynton 1987, however, examining dichromats' colour naming of surface samples, take precisely the opposite view, having found that their one protanope subject did rather badly after a bleach after all. But then Montag 1994 explicitly retracts his 1987 conclusion and goes back once again to the hypothesis of some cones of the supposedly 'missing' type—on the ground that increased light levels (not involving a bleach, but just ordinarily bright lighting) do not significantly impair dichromats' performance.

I shall make just a couple of comments. First, it is a little odd that rods and cones are mostly treated as rival and mutually exclusive hypotheses. There seems no *a priori* reason why we should expect just one factor to be *the* explanation of all the phenomena in question. Rods and cones might both play a role, perhaps under different conditions, or perhaps sometimes operating together. More importantly, it is odd to suppose that rods and cones exhaust the options for possible explanations here: and among the further options, we might also consider the possibility that what counts is not the number of types of receptor, so much as the range of ways they are *used* in. Secondly, we should note how very indirect most of the evidence here is, especially for the presence of a third cone type: the argument is usually simply that, if it isn't rods, then it is bound to be another set of cones. And that is hardly persuasive if there may be further options yet. Thirdly, we should note some very striking individual differences among the subjects. Among the 'dichromats' of these experiments, most show much better performance than would be traditionally expected, when given larger fields and more time; but not all do: Protanope GR in Nagy 1980 is unique among his group in accepting just *any* mix of R₆₆₀ and G₅₂₃ as matching Y₅₈₈, even in 12° annular fields; it may be that he is a perfect, or true, dichromat, while the others have a different retinal endowment. But it could also be that the difference lies in what the subject has *learned to do* with that retinal endowment—e.g. some dichromats may over years have made efforts to 'compensate' for the effects of a missing cone-receptor type, which have left them permanently able to gather and utilize relevant information successfully, while other dichromats may have simply given up or not bothered to try. There is clearly a huge amount of further exploration to be done here.

We need, I believe, not only more investigation of the cone- and rod-hypotheses; we need *more hypotheses* in the field too. (Crognale et al. 1999, 718 mention some other ideas—including the promising

idea of variations in the optical density of one and the same photopigment in different parts of the retina.) In the present context, I shall concentrate on a further hypothesis of a quite different kind—that, dichromats, even if they only receive *two-dimensional colour information* from any one part of the retina, may yet, from such two-dimensional input, be able to synthesize *three-dimensional colour information* and even colour *experience*. This hypothesis too should not be understood as exclusive of the other hypotheses: the colour perception system may well be complex enough that several factors of different kinds feed in to the production of a single kind of colour experience—and in the absence of one factor, the others may in some degree compensate or operate to the same end instead.⁶¹ Nor is the hypothesis meant to be, with the other possible factors already mentioned, exhaustive of the field: on the contrary, I suspect we will need further hypotheses yet.

6. Red and Green from Yellow and Blue: Modelling recovery of the ‘missing’ dimension

Let me start with an analogy. We might be tempted to think that people using only one eye could in a sense never see things as being at a distance: with merely one 2-dimensional retinal image, they could not be aware of the third spatial dimension that people with binocular vision experience. We might perhaps stretch to granting these people illusions or suppositions of distance, but they could surely not—so this line of thought runs—have knowledge or genuine perception of that ‘missing’ dimension. A little reflection and the empirical facts show how wrong this view is. In many ordinary situations, the information that is available to *two eyes at one time* is equally available to *one eye at two times*—if one simply moves one’s head. And it is not just that the information is available but the phenomenology of depth (however that might be analyzed) is missing: there is indeed a sense in which, standing still and closing one eye, one may find that things in front seem in some sense to ‘lose their depth’; but as soon as one starts moving around, they rather remarkably seem to regain it and fall into place as looking laid out in a three-dimensional space. And practical performance on tasks that involve depth perception may survive the loss of an eye extraordinarily well. Mansur Ali Khan, Nawab of Pataudi, had already lost the sight of his right eye in a car accident when he became Captain of the Indian cricket team in 1962 (at the age of 21), and he continued to be a powerful medium pace bowler.⁶² There are, we know, many factors that in different ways contribute to the many varieties of depth perception. (Descartes and Berkeley argued over some of them, like the angles of optic axes and sensations from changes of focus; other factors—like retinal disparity—emerge in Wheatstone and later writers.⁶³) It would obviously be a mistake to imagine that there was just one kind of input that was ‘the’ source of ‘sensations of depth’. And though any 2D image will in a sense present depth-ambiguities, nonetheless, with even just a single 2D image like a photograph, as soon as we recognize in it solid physical objects of various familiar kinds, we will also see many of those objects as standing in various more or less determinate depth relations to each other and to the apparent vantage point of the camera. Strictly unambiguous depth information may need to be gathered over time; but practically unambiguous depth information may—given the structures and regularities of our world—be available

even from a single 2D presentation, if it is complex enough in itself and we can recognize in it things of familiar types.

The view that dichromats, if they have only two kinds of colour receptor, can see only two dimensions of colour, has the same kind of simple attractiveness as the view that people with a single two-dimensional retinal image can see only two spatial dimensions. And it may be no less incorrect. Might it not be a mistake to think there is just one kind of input (say, a suitable variation in an (L-M) opponent channel) that is ‘the’ source of experience of, e.g. the red-green colour-dimension—and that in its absence people must simply have no experience of that dimension? Depth perception is not solely a matter of binocular vision, and maybe seeing three dimensions of colour is not just a matter of having three kinds of cone.

Supposing the dichromat to have only two kinds of cone, how might he be able to use that 2D colour-information over time, so as to recover information about the third dimension of colour that he is (or appears to be) ‘missing’? Here is one clue. Many dichromats, knowing their weakness, take care, when trying to tell the colour of something, to look at it in a variety of different kinds of light, or from a variety of points of view—seeing how one and the same surface catches the different kinds of light in the environment in various different ways. They test the surface out—putting it through its paces, so to speak, to see what it can do. So we might enquire: might seeing a surface-colour *under two different illuminants* actually provide a dichromat, who at first seemed to lack information on the red-green dimension, with information about that third dimension, rather in the way that seeing a scene *from two different points of view* provides a person who is looking with only one eye with information on the third spatial dimension? And might some such process even cause some of the things around to *look* red, for example—rather than merely being *judged* to be red—, just as the right kind of looking from different directions, even with just one eye, seems actually to cause some things in the environment to *look* nearer, rather than merely being *judged* to be nearer? And might it even be the case that a single stimulus or input that, in a sense, contained only 2 dimensions of colour-variation might be taken or ‘read’ by people—given its own complexity, and the structure and regularities of our world—as a presentation of things varying in 3 colour-dimensions? We shall find reason for thinking that the answer might be yes to all these questions—though whether the cues that I investigate here are cues that are actually *used*, as well being *usable*, is a further question I shall have to leave for later experimental investigation.

6.1 Fifteen Pigments, in two different illuminants: Normal Trichromats & Protanopes

Let us suppose the standard account of the mechanics of vision in the dichromat: suppose that protanopes, for example, lack L-cones and receive information only from M- and S-cones. We will expect them to confuse coloured lights according to the standard confusion lines (see Fig. 11, cp. Figs. 6, 7). They should confuse light corresponding to Cadmium Red and Viridian, or to Mars Red and Cobalt titanate Green Spinel. In practice, however, we find they are surprisingly successful at identifying the colours of things

that they are supposed to confuse. So how do they do it? What might be the methods or mechanisms by which they achieve what limited success they actually achieve?

The clue that I shall follow up is the fact that a red and a green thing that look indistinguishable in one kind of illumination may behave differently as the illumination changes. As we shall see, light sources differ substantially in physical character: Fig. 8 shows how different, for example, the direct light of the sun and the light of the remainder of the sky typically are—the former is yellowish, the latter bluish. For illustrative purposes I shall in some mathematical modelling concentrate on two particular kinds of light, labelled D₅₅ and D₇₅ by the CIE, which approximate to direct sunlight (of correlated colour temperature 5500 K) and to the sky without sun (7500 K). They are far enough apart for interesting effects to show up, but much greater natural differences do occur in our environment, especially if we include the light of a fire or an incandescent light bulb.

When there is a change in illumination, it may happen that some of the objects around respond to that change differently from others, and that this is a sign of differences in colour among those objects. For example, suppose the light changes from the yellow of direct sunlight (D₅₅) to the blue of the remainder of the sky (D₇₅)—perhaps a cloud covers the sun, or a person picks up an object and turns it so it catches the light from a different part of the sky. Then it may happen that a red and a green thing that were indistinguishable earlier will come to look different: the red thing will have darkened in relation to other things, while the green will not. (Green is more or less equally ‘close’ to a blue illuminant and to a yellow; red is always ‘further’ from a blue than from a yellow.) If a pair of red and green things initially looked equal in lightness, then the red will now look darker. This is, as we shall see, a general phenomenon for normal trichromats. And this might be a dichromat’s clue to recognizing or ‘recovering’ the red-green dimension that he is standardly supposed to be ‘missing’. This is the hypothesis that I shall be modelling here. Of course we do not usually see the illumination in the way that we see objects that are reflecting that light (though we should not forget that we can look at the sky, and, even if not for long, at the sun and other sources). But, provided there is a good range of coloured things in the environment, we will have good guides to the colour of the illuminant; and it is a fact of everyday life that, over a wide range of changes of illumination, we manage quite decently to adjust to, and keep track of, the changes—not merely ‘discounting’ or ‘eliminating’ the colour of the illuminant (on the model of either Helmholtz or Hering), but, rather, *keeping track of* and recognizing, in some general way, *both* the colours of things *and* the colours of the light.⁶⁴

To test the hypothesis—that difference in behaviour of red and green things under changes in illumination contains information that could be a clue to their colour for protanopes and deuteranopes—I shall briefly examine such changes of illumination for normal trichromats, and then focus in detail on the case of protanopes; deuteranopes will appear in § 6.6.

Red and Green from Yellow and Blue

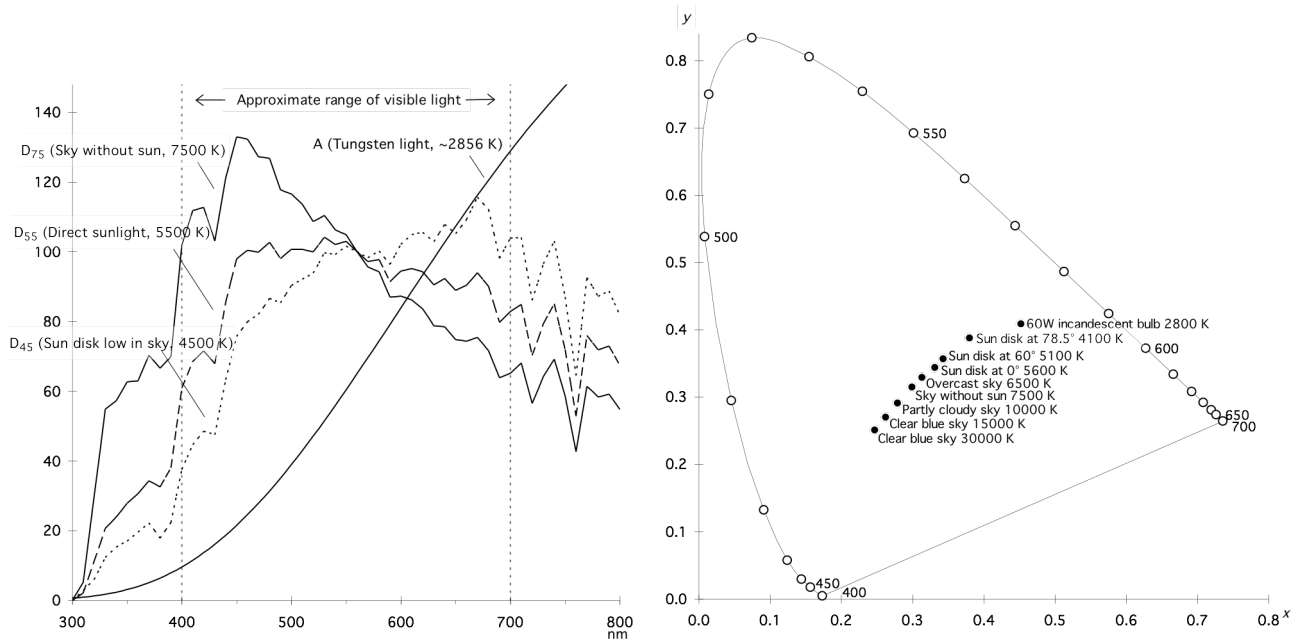


Fig. 8. Representative natural illuminants. Left: Spectral power distribution of some representative illuminants, approximating daylight of correlated colour temperature 4500 K, 5500 K and 7500 K (CIE Standard Illuminants D₄₅, D₅₅ and D₇₅), and the light of a tungsten lamp of about 2856 K (Standard Illuminant A). Note how much red is in the sun's light when it is low in the sky, and how much blue is in the light of the sky, and how the differences between the curves are all the more striking if we concentrate on the visible part of the spectrum, from 400 to 700 nm. (My graph from standard CIE basis functions; see e.g. Hunt 1987, 186-195.)

Right: Chromaticity of representative light sources (mapped from data in Wyszecki & Stiles 1982, 28 & 8-9). Note that natural light (except for low temperature sources) varies mostly along something very close to a straight line running between approximately unique blue and unique yellow (~470 nm and ~575 nm). The direction of that line, incidentally, is very similar to that of the variation produced by a yellow filter such as the macula (see §6.5 below).

One remarkable fact that may simplify the task of recovering the 'missing' dimension of colour for dichromats is that the main changes in illumination in our environment lie pretty much on a single axis, running between Yellow and Blue: it is only at quite low temperatures that we find such departures as the relatively orange light of a 60 watt light bulb, for example. Purple and green ambient light are simply not much found in our environment; if there were any need for a constant reminder of the position of unique blue and unique yellow to recalibrate our visual systems as we and the environment change, indeed, one could take the main axis of change in natural lighting as a pretty good guide.⁶⁵ (I think of the significant fact that people place unique hues pretty constantly throughout their lives, unaffected by changes in the colour of the lens of the eye.) As we will see later, once we have taken account of changes in illuminant along the YB axis, there will be residual changes in retinal stimulation that we can attribute either to the change in the illuminant or to the nature of the objects seen.

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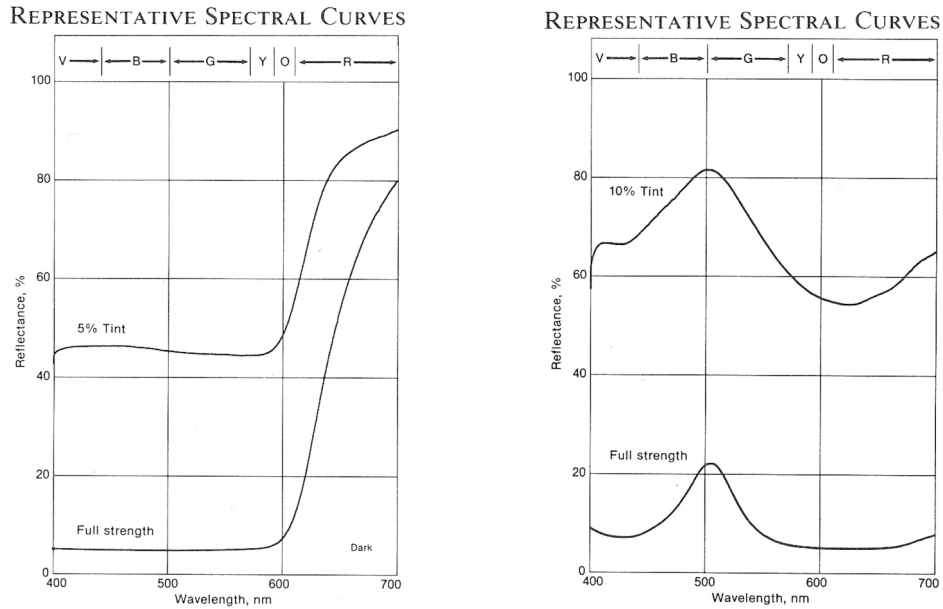


Fig. 9. Spectral reflectance curves for two sample pigments, Pure cadmium red (PR108) (left) and Viridian (PG18) (right). (Mayer 1940/1991, 80 & 98. In each graph, the lower line gives the reflectance function for the pigment at full strength, the upper one for the pigment in a 'tint' or mixture with white. The present study considers the pigments only at full strength.

For colour-samples, I have taken 15 pigments, using their spectral reflectance curves as in Mayer 1940/1991 (see e.g. Fig. 9). For the spectral composition of the two kinds of illuminant, I use CIE functions for Standard Illuminants D₅₅ and D₇₅, corresponding to direct sunlight of 5500 K and sky without sun of about 7500 K (cp. Hunt 1987 186-88, Wyszecki & Stiles 1982 8-9). Multiplying these together (I sample each function at 20 nm intervals from 400 to 700 nm), we can calculate the spectral character of light reaching an observer's eye. Using the standard CIE colour-matching functions we can calculate X , Y , Z values for the light stimulus component at each sampled wavelength at the eye, and sum these to get X , Y , Z values for the complex total stimulus, and thereby get x , y values for it. (See e.g. Hunt 1987 ch. 2 for the method.) And we can do these calculations for each pigment, first as seen in direct sunlight, and then as seen in sky without sun. The shift (as the light changes from D₅₅ to D₇₅) may in general be characterized as a blue-ening: thus in the accompanying figure, a line marks the shift in the x , y values for each pigment type, and it points roughly towards the bottom left, signalling an increase in the blueness of the stimulus at the eye as the illuminant changes in that way.

What is more remarkable than the direction of these general chromaticity shifts is the pattern of changes in *luminance* that take place at the same time. To separate out the effects of the colour of the illuminant from those of its total luminance, I shall assume we are dealing with a case where the D₅₅ and the D₇₅ are matched in luminance: some things get lighter in appearance, while others get darker, but the

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average is constant. The change from D_{55} to D_{75} is a blue-ening; unsurprisingly therefore, blue things benefit from the change more than others: the blue pigments increase in luminance under the change, while the yellow ones decrease (Cobalt Blue gains 4.1% in luminance, Cadmium Yellow loses 3.4%). But more important for us is the fact that *green* things also tend to get lighter, being relatively close to the blue illuminant, while *red* things get darker, being relatively distant from it. The effects are illustrated below (in Fig. 10) for normal trichromats. The proportionate increase in luminance as the light changes from D_{55} to D_{75} is, for the three green pigments, positive: +3.8%, +3.2% and +0.9% (for Viridian, Cobalt titanate Green Spinel, and Chromium oxide green). The proportionate increase in luminance for the red pigments is negative: -5.9%, -4.6%, -4.5% (for Cadmium Red, Mars Red and Perylene Scarlet).⁶⁶ It is indeed the case that red and green things interact differently with the light as the sun goes in and we have sky without sun, or when we tip the things so they catch the light of a different part of the sky.

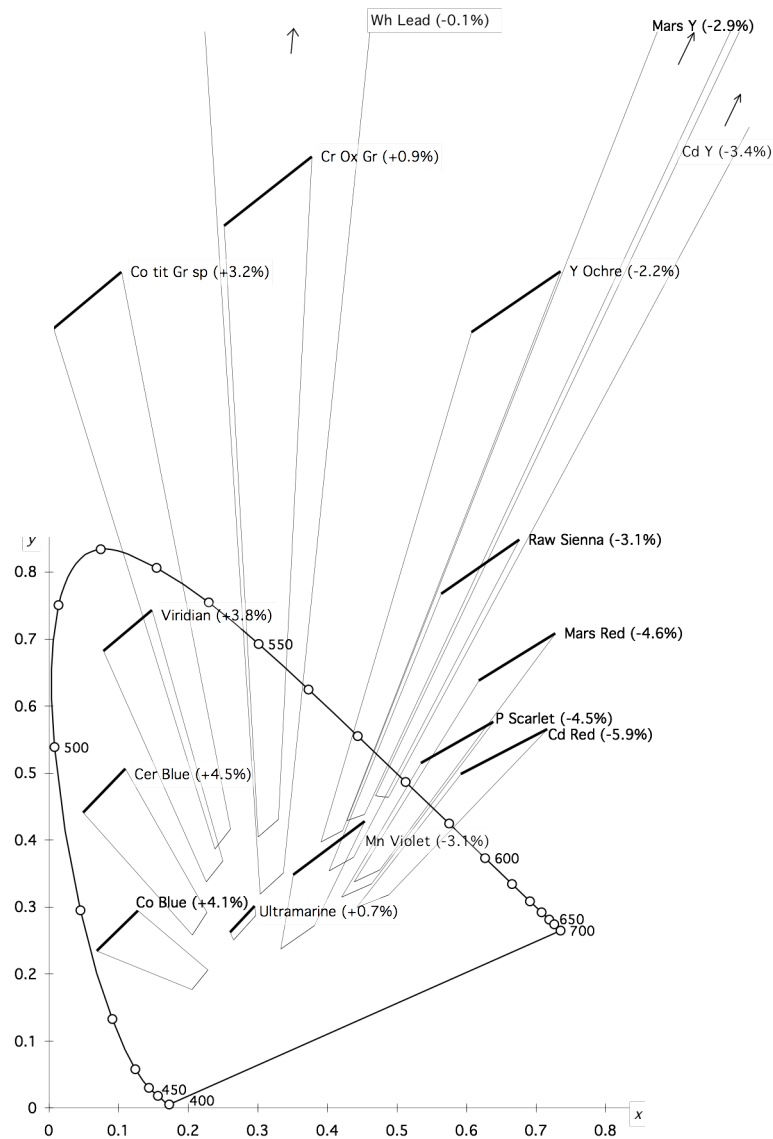


Fig. 10. Changes in Chromaticity and Luminance of 15 pigments, as illuminant changes from D_{55} to D_{75} : Normal Trichromats. An approximately perspectival view of a 3D representation of tristimulus values: the (x, y) plane can be read as horizontal, with points on it representing the chromaticity of the pigments; from each point there is a line rising approximately vertically out of the plane, the height of which represents the luminance of the pigment above a certain 'floor' represented by the (x, y) plane (and set here at about 4% of the luminance of white). Each 'skyscraper' represents a pigment first in D_{55} (with the right-hand vertical wall) and then in D_{75} (with the left-hand vertical wall). Note how the roofs of the 'skyscrapers' slope in different directions in different parts of the diagram: in the bottom left and upper middle (for blue and green pigments) the left-hand wall of each 'skyscraper' is taller than the right-hand wall: the pigments have higher luminance in D_{75} than in D_{55} . But the 'skyscrapers' in the right and lower right of the diagram (for yellow and red pigments) have the opposite slope: the pigments have higher luminance in D_{55} than D_{75} . As the light becomes bluer, there is a surge in luminance for pigments close in colour to the new illuminant. Values for the proportionate luminance increase are given in parentheses: Green pigments have a positive increase in luminance (3.8%, 3.2%, 0.9%); the red pigments have a negative increase (-5.9%, -4.5%, -4.6%).⁶⁷

So far we have been talking of these changes as they affect the normal trichromat; but the luminance functions for dichromats are different (particularly in the case of protanopes: lacking the L-cones, they see much less brightness in the red end of the spectrum). So let us move now to the case of the protanope, and calculate the change in luminance under the same change of illumination.⁶⁸ The results appear in Fig. 11: there is a clear difference in the behaviour of green and red things under the change. I have again modelled a case where the overall average luminance is the same before and after the change. It turns out, just as in the trichromat case, that blue pigments gain most in luminance, followed by green, while the yellow and red lose luminance. If we suppose a protanope with samples of Cadmium Red and of Viridian that initially look indistinguishable to him in sunlight such as D_{55} ; then, if he can bring them into bluer (but similarly bright) skylight such as D_{75} , then the Viridian will lighten by 2.9%, whereas the Cadmium Red will *darken* by a similar percentage, 2.3%. The difference is not huge, but it would be a good clue for a protanope to use: among reds and greens that are initially indistinguishable, those things are *greenish* which become relatively light as the incident light becomes bluer, while those things are *reddish* which become relatively dark.

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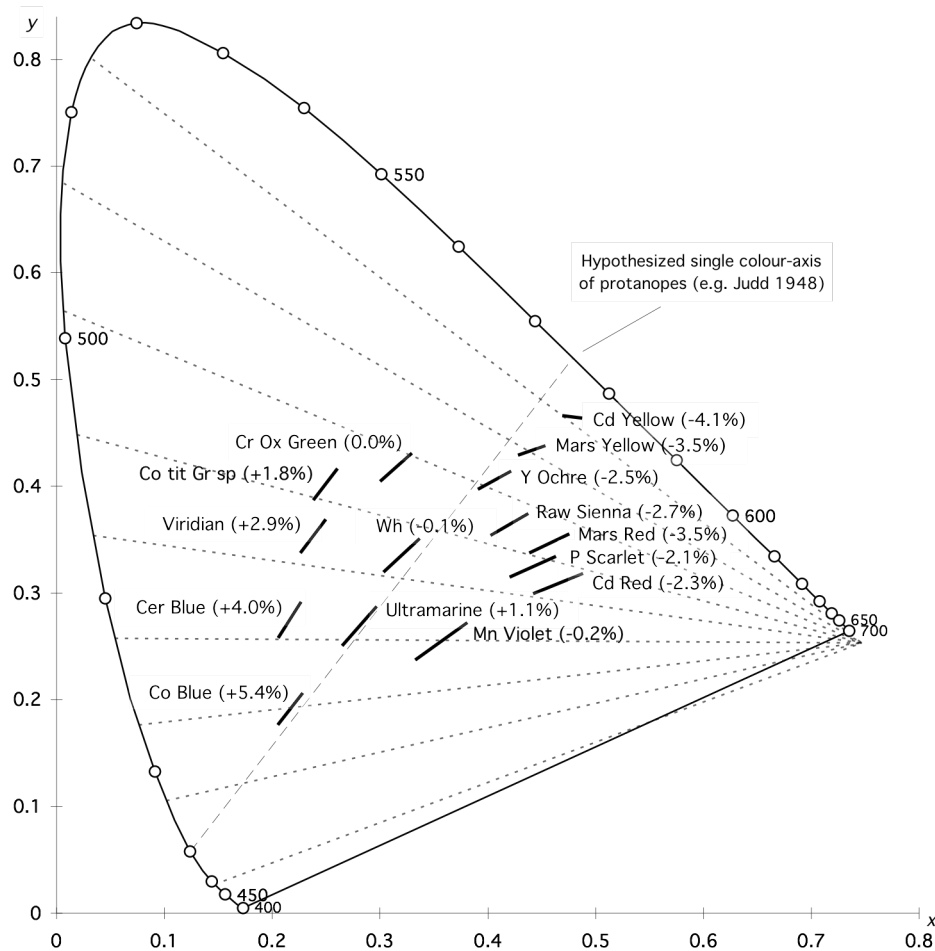


Fig. 11. CIE 1931 Chromaticity coordinates for 15 pigments, with & without direct sunlight: Protanopes. For each pigment a line indicates the shift in chromaticity between the pigment in D_{55} (marked at the right or upper-right end of the line, marked with the colour-name) and the pigment in D_{75} (at the left or lower-left end). Proportionate increase in luminance under the change is given in parentheses. As the light changes from D_{55} to D_{75} , the green pigments show an increase in luminance of 2.9%, 1.8%, 0.0%, the red pigments show an increase of -2.3%, -2.1%, -3.5%. Of red and green things that seemed confusable, it is green things that lighten most, while red things darken.

I have made the proposal so far under a supposition that is really unnecessary. On this supposition, the information needed to tell red and green things apart is not available to the dichromat at one instant; but it is available *over time*, when there are suitable changes in illumination. But there is, of course, no necessity that changes like this should be successive in the world, rather than (so to speak) just successive in our experience of the world: the viewer may (while the illuminants in the environment remain unchanged) simply move the object or move her head so that the light reflected by it to the eye comes from, say, the blue sky rather than the sun directly—or (to take another case) from the window rather than a light bulb. And it is not necessary even that the changes be successive at all: if, for example, there is a single

uniform surface, different parts of which (as seen from the present point of view) catch light from different sources in different ways, then the kinds of variation we have been talking of may manifest themselves even at one single moment. A single large expanse of green, seen by the light of a window, may in one region have a bright patch of direct sunlight on it, while another expanse of red, in the same scene, has a corresponding patch of sunlight falling on it—the difference between the variegation in the one expanse and the variegation in the other providing at one instant the same information as we initially thought of as being gathered over time. And of course curved surfaces often provide more such variation than flat ones, as they face light coming from a wider range of directions. Our ability to use this kind of variation as a guide to colour depends, of course, on our being able to recognize uniformly coloured expanses as precisely that, and to recognize or take account of their shapes: we need, with the red and green expanses, to be able to take the appearance of a highlight as the effect of sunlight falling on a uniform surface in a non-uniform manner (i.e. being reflected from it at a variety of angles to the present viewpoint), rather than being instead, say, the effect of a nonuniform surface colouration being seen under exceptionally uniform illumination. But we do manage to recognize and keep track of such things quite well in our environment; and—though the topic is one for another occasion—we live in a world to which we are generally well-enough attuned that what we take to be the case in such circumstances is mostly not too far from what is actually the case.

6.2 Recovering 3D Colour from 2D receptor information: A first Projection

Let us work with the idea that at any one time from any one region the protanope receives only 2D colour information; and assume that the information can be mapped in terms of luminance and the one-dimensional line that runs from 470 nm to 575 nm on the standard CIE diagram. (I don't believe for a moment that the protanope actually has experience only of the two hues of yellow and blue that that line spans; but it will do no harm to accept the proposition that one dimension of the protanope's input data can be mapped on such a line.)

We may picture the information available as having collapsed from a solid to a plane; abstracting from luminance, the hue stimulus space has collapsed from a plane to a line. The result of the collapse can be pictured like this:

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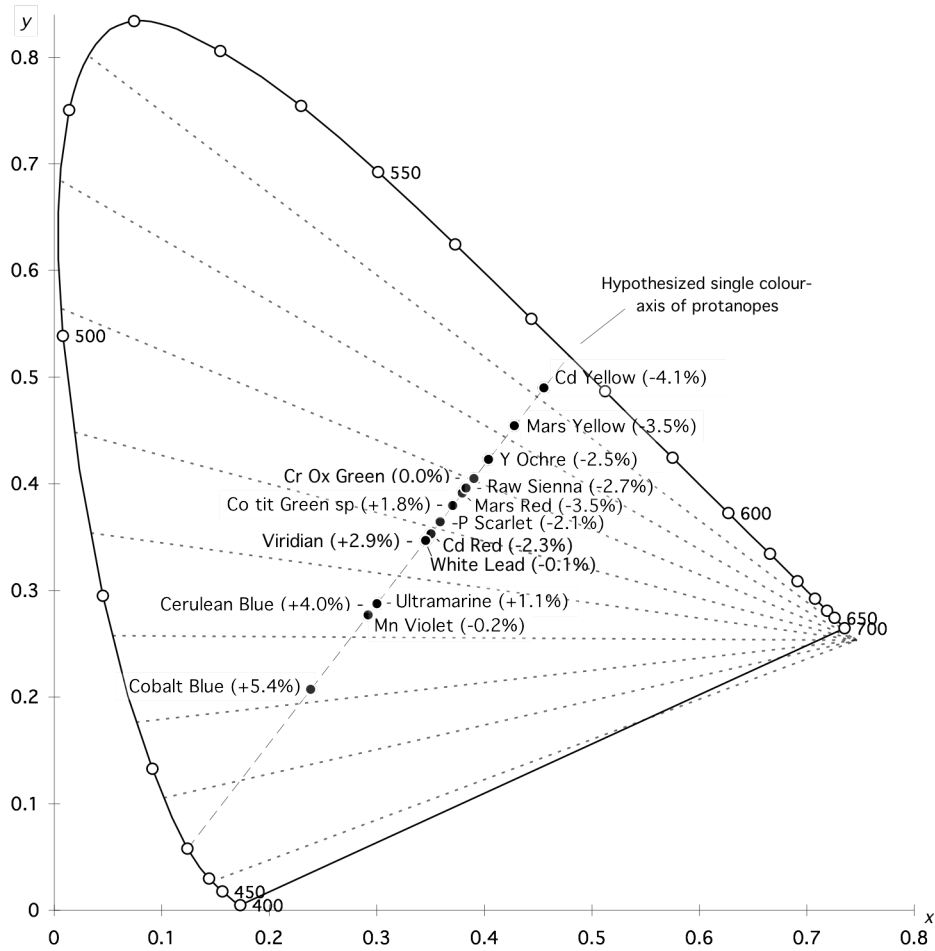


Fig. 12. CIE 1931 Chromaticity coordinates for 15 pigments, in D_{55} direct sunlight: Standard Model collapse for Protanopes. In parentheses are given the percentage increases in protanope luminance for each pigment under a change of illuminant from D_{55} to D_{75} (of equal total luminance): greens generally get lighter, reds get darker.

Thus, aside from lightness, Cadmium Red would for the protanope be a near-metamer for Viridian, and Cobalt Titanate Green Spinel would apparently lie in hue (or, rather, just saturation) between Mars Red and Perylene Scarlet. And, perhaps most remarkably, the saturated pigments Viridian and Cadmium Red would be supposedly almost indistinguishable from some achromatic grey. And as the light changed from D_{55} (Yellow) to D_{75} (Blue), apart from varying in relative lightness, the samples could change only by moving to other projections along that same axis, exhibiting more varieties of the unique Blue and Yellow that supposedly fix the limits of the dichromat's colour world.

From this kind of input, how could one recover information about the 'missing' red-green colour dimension? Here is a first attempt. Noticing the way the reds become darker and the greens lighter under our change of illuminant, we might try taking the magnitude of the luminance change under the change of

illuminant as a direct sign of the missing colour dimension. This can be done in a variety of ways, but it only takes one example to show the problems in principle with all of them:

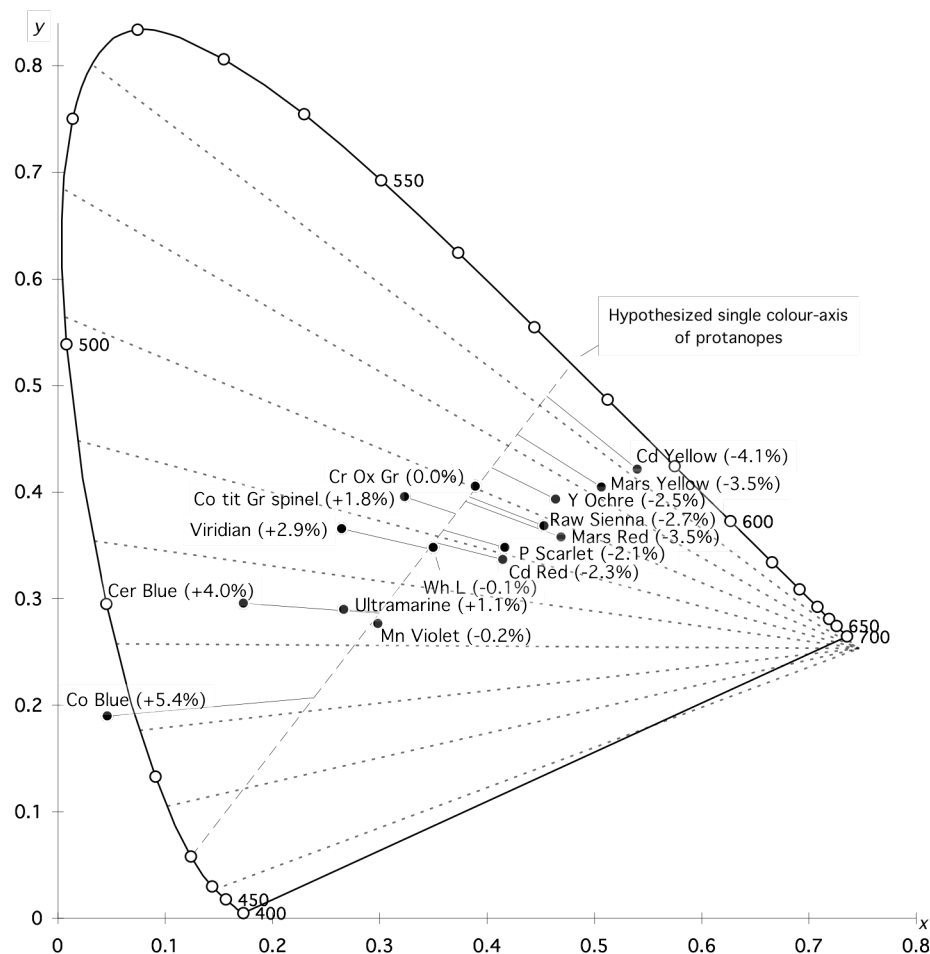


Fig. 13. One crude Projection into CIE 1931 space: 15 pigments, in direct sunlight (D_{55}): Protanopes. Hue-axis values have been projected away from Confusion point in proportion to increase of luminance (under the change from D_{55} to D_{75}). The problem is that the projection makes the Yellows come out too red and the Blues too green—and both kinds come out as too saturated.

The difficulty is that the yellows are shifted too far towards the red, and the blues are shifted too far away from it: the projection had taken no account of the fact that as the light shifts from yellow to blue (e.g. from D_{75} to D_{55}), this affects not just reds and greens, but also yellows and blues. Yellows will darken, and blues lighten⁶⁹ -- and these variations are nothing to do with the missing RG axis. We should not be taking the luminance increase of Cobalt Blue as a sign of greenness in it, and the lack of such increase in Cadmium Yellow as a sign of redness. Rather, we should discount the overall variation in lightness that goes with

position on the YB axis, and then see how much further variation in lightness remains. This is what is attempted in the Second Projection.

6.3 A better Projection

We need to distinguish the change in luminance due to the Yellow-Blueness of a sample from the change in luminance due to its Red-Greenness. This is not particularly difficult conceptually, but I have adopted a relatively simple mathematical method that fits the phenomena reasonably well, rather than exploring more sophisticated methods that might fit it better.

We first calculate the distance in the YB dimension of each sample from N (imaginary pure white). (N corresponds also to the colour of the illuminant. If there is a reasonable range of coloured objects in the environment, then it should not be particularly hard to identify.) We may suppose in general that a pigment's position alone on this single YB axis is itself partly responsible for a change in luminance under the kind of change of illuminant that we have been considering; but to what extent? One reasonable suggestion is this: Almost completely, in the case of colours near the extremes of this YB scale, for a reason I shall explain. In our particular case: take the two samples with the most extreme values on the one input colour-dimension, namely (in our data) Cobalt Blue and Cadmium Yellow. Assuming that the samples have been chosen from an environment reasonably rich in samples that differ fairly broadly within colour space and that fall (in CIE x, y chromaticity space) within a closed curve such as a circle, we may suppose that if in x, y chromaticity space the two samples are at extremes on the YB axis, then they also lie fairly close to the YB-axis—i.e. have relatively small values on the 'missing' hue-axis orthogonal to this one.⁷⁰ This enables us to calculate a factor for the percentage increase in luminance attributable to the Yellow-Blueness of samples (which I will call S').

If we set up a scale on the YB dimension, taking white as the origin of the scale and measuring distance in the units of the CIE x, y chromaticity diagram, then in the single YB dimension, Cadmium Yellow (illuminated by D₅₅) lies at + 0.1727 from the position of pure white; and, as the illuminant changes (to D₇₅), it increases in luminance by -4.1%. Cobalt Blue lies on the axis at -0.1838 and increases in luminance by +5.4%. If we attributed all of the increase in Cadmium Yellow's luminance (under the illuminant change) to its position on the YB axis, we could calculate the *increase in luminance* (under the change of illuminant from D₅₅ to D₇₅) *attributable per unit of YB-ness* as: $-4.10\% / 0.1727$, i.e. -23.74% (treating a -4.10% increase in luminance as due solely to a value of 0.1727 on the YB axis). And for Cobalt Blue, we could calculate $5.4\% / -0.1838$, i.e. -29.38% (treating a 5.4% increase in luminance as due solely to a value of -0.1838 on the YB axis). I propose (though I would not claim any particular theoretical reason) that we attribute not quite all of the increase in luminance of Cd Yellow to its YB-ness—and therefore try out -23% as the factor we are seeking. Taking the factor as -23%, we attribute a -3.97% increase in luminance for Cd Yellow as due simply to its position near the Y end of the axis ($0.1727 \times -23\% = -3.97\%$); similarly we attribute a 4.23% increase in luminance for Cobalt Blue to its position near

the Blue end of the axis ($-0.1838 \times -23\% = 4.23\%$). This of course leaves some residual lightness variation to be attributed to other factors—i.e., in our rather simple model, to position in the Red-Green dimension, which the dichromat is ‘missing’: this is partly a good thing, given that the Cadmium Yellow and Cobalt Blue samples are (in CIE x, y space for normal trichromats) actually not *on* the YB axis -- and it will be good to allow some of the loss of luminance in the Cadmium Yellow to be due to some orange-redness in its hue; and some of the gain in luminance in Cobalt Blue to be due to some greenness in it. No doubt other and better functions could be devised for discounting the change in lightness attributable solely to YB-ness in any sample.

Alternatively, we might propose saying that, within any one confusion class, the average increase in luminance (under the kind of change of illuminant we have been examining) can be put down to position on the YB axis; and the deviation from that average can be put down to position on the ‘missing’ RG axis.

If we then use this factor (-23% multiplied by the value on this YB axis) as the degree of change attributable simply to YBness, then—in general, for any sample—when we have discounted this element, there remains some further change in lightness attributable to its RGness. Thus: when the first factor has been applied, the three reds have residual increases in lightness of -2.3% , -1.8% and -2.4% ; the three greens have residual increases of $+2.7\%$, $+2.6\%$ and $+1.6\%$. Such residual differences can be attributed to the redness vs. greenness of the samples and they can be used as a guide for recovering this ‘missing’ dimension.

We may then project our results into the ‘missing’ dimension in the CIE x, y space. I take the standard protanope confusion point $x = 0.747$, $y = 0.253$ as the origin (cp. Wyszecki & Stiles 1982, 464, cf. §3 above); points on the YB axis are projected by different amounts according to the degree of residual increase in lightness attributable to the ‘missing’ dimension; those with positive values are projected beyond the YB axis (i.e. out into the upper left of our diagram); those with negative values (for residual lightness increase) are projected backwards (or negatively) from the YB axis (i.e. into the lower right of the diagram). But how are we to calculate the amounts of the projection? One attractive idea is to suppose there is a constant factor that is the increase in luminance for any pigment per unit of RG-ness; given such a factor, we may calculate from the residual increase in luminance attributed to any pigment its degree of RG-ness. But what is a suitable factor?

One reasonable hypothesis is to suppose that the *Luminance increase per unit in the other dimension (RG-ness)* (we may call this S'') is equal to the *Luminance increase per unit of YB-ness* (which we called S' , and which we have taken to be -23%). And this turns out to be a pretty good hypothesis. Given the Residual luminance increase attributable to RG-ness, we calculate a coordinate for something like RGness, and use that as the distance by which a point on the YB axis should be projected along a line towards the confusion point. Thus, for example, Cadmium Red has a residual luminance increase of -2.3% ; this implies a value in the missing RG-dimension of ~ 0.10 (which is $-2.3\% / -23\%$), and if we then project the point representing Cadmium Red on the YB axis by a distance of 0.10 (in the units of CIE x, y space) in

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the direction of the protanope confusion point, then it moves from about (0.35, 0.35) to (0.45, 0.33).

Conversely, Viridian, with a residual luminance increase of 2.7%, is given a value of -0.12 in the ‘missing’ RG-dimension (since $-0.12 = 2.7\% / -23\%$), and it is projected a slightly greater distance in the opposite direction.

The results can be seen in the diagram below.

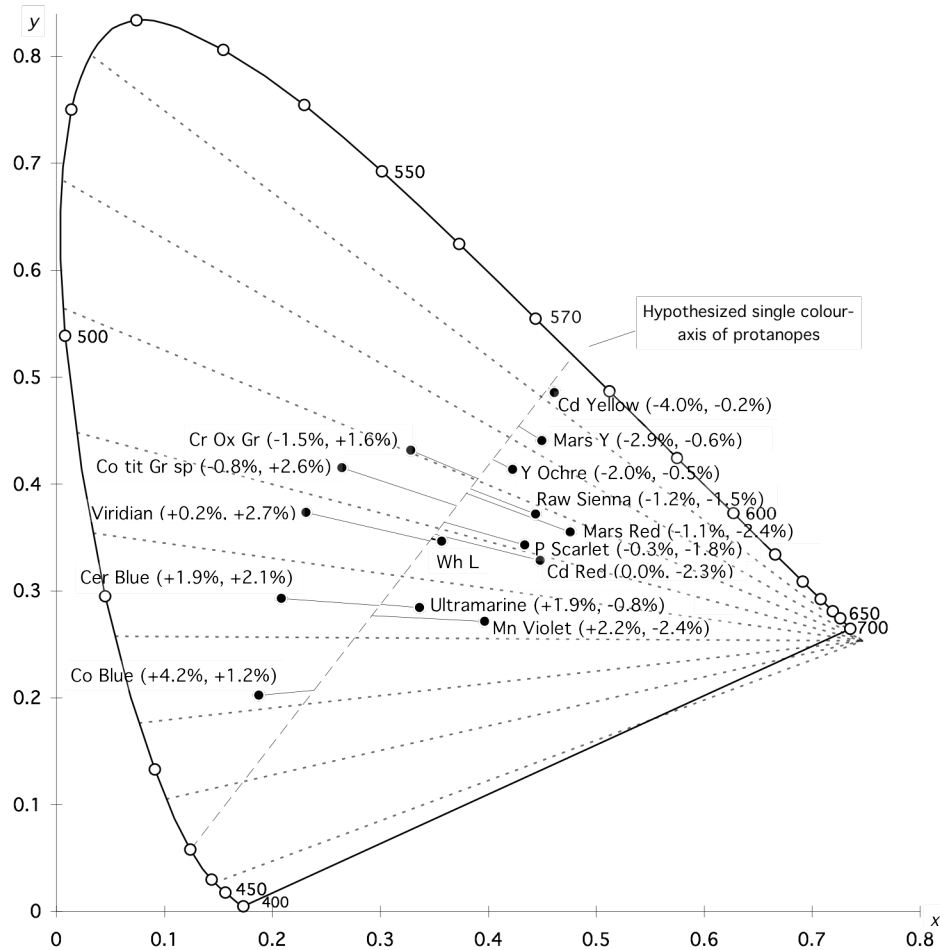


Fig. 14. Recovering the ‘missing’ dimension of Red-Greenness for Protanopes: Improved Projection into CIE x, y space: 15 pigments. Points on YB axis (for pigments in D₅₅) are projected towards (or away from) the Confusion point by a distance given as a multiple ($1/S''$, i.e. $1/-23\%$) of Residual Luminance increase attributable to RG-ness. In parentheses, for each pigment, are given two values, % Increase in Luminance attributed to YB-ness and % Increase in Luminance attributed to RG-ness: these can be thought of as coordinates for Blueness-Yellowness and for the recovered dimension of Greenness-Redness. Values of the first coordinate run from +4.2% for Cobalt Blue in the bottom left to -4.0% for Cadmium Yellow in the top right. Values of the second coordinate run from +2.7% for Viridian in the top left to -2.4% for Mars Red (and Manganese Violet) in the bottom right. (The label “Wh L” marks the position of White Lead (0.1%, -0.2%).)

This is really pretty good as recovery of colour information: it is not far from being the converse of the original ‘collapse’ of the chromaticity plane to a single line. And it shows that the protanope has the information necessary to recover pretty much the ‘missing’ Red-Green dimension of colour space, even if it is not accessible to him in the normal way. The mean discrepancy between the recovered coordinates and the original coordinates for the 15 different pigments in D₅₅ is about 0.0186 (in the units of the CIE *x, y* space), which is less than two-thirds of the distance between the two closest pigments in our selection, Cadmium Red and Perylene Scarlet in D₅₅.

There could obviously be other hypotheses for the recovery of the missing dimension, and we could make small adjustments to the various constants employed in the model. It turns out that our value of -23% was a good one to choose for the *Luminance increase per unit of YB-ness and per unit of RG-ness*. (One can try out making small changes in the values for *S'* and *S''*, but they produce only very marginal improvements in the recovery.⁷¹) There are of course more radical ways in which one might probe the proposal and make improvements. That 0.1 (in CIE *x, y* units) of displacement from the YB axis along any protanope confusion line should count as representing the same degree of Redness or Greenness—regardless of which part of the *x, y* diagram one is in—is surely not correct. That we should be talking of Red-Green-ness, rather than, say, Red-Turquoise-ness, is debatable. (The complementary to the red of the protanope confusion point is not green but a blue-green of about 493-495 nm.) The most evident practical weakness of the algorithm is in the treatment of the blues: Cerulean Blue and Cobalt Blue are 0.02 and 0.04 too far to the left, and Ultramarine and Manganese Violet 0.04 and 0.02 too far to the right: one might say the projection in three of these cases had gone too far. (On the other hand, Ultramarine has crossed to the wrong side of the YB line, so there’s another problem too.) Meanwhile, at the other end of the YB scale, Cadmium Yellow is 0.03 too far to the left and 0.02 too high: one might say the projection had not gone far enough. (The other yellows, on the other hand, have come out very well.) There may be some well-motivated general improvements that could reduce those problems.

But such issues are, I suspect, of secondary importance. There are going to be limits in principle to the perfectibility of any recovery method like the one employed here. Information on the missing dimension of, e.g., RG-ness is no doubt only imperfectly captured in the kinds of cue I am studying.⁷² The claim is not that perfect recovery of the missing colour dimension is available with the relatively slight information from a single illuminant change. It is that we can see that an approximate recovery is possible, and that it is not implausible that the dichromat visual system might in fact operate on some parallel or similar principles—if, that is, the more central parts of the dichromat system are in some way set up or primed for some kind of trichromatic experience. It might even be a reasonable speculation that if this happens at all, then it is thanks to the operation in the dichromat of visual information processing systems that operate similarly in the *trichromat* too—the suggestion would be that there are many ways in which colour constancy mechanisms operate in normal trichromats (some of which show up, I think, most remarkably in Land’s red-and-white projections), and dichromat recovery of colour might be a by-product

of such systems. Except that that no doubt puts the evolution of this the wrong way round: trichromacy in the retina emerged out of genetic variations in dichromatic systems (see e.g. Regan et al. 2001); but, for receptor variations (in what had previously been a dichromatic system) to be of any value, there needed to be already some readiness in the remainder of the system to benefit from the new variety of inputs (rather than simply regarding it as ‘noise’ or inaccuracy to have a deviant variety of a standard receptor type). So perhaps the dichromat, recovering 3D colour, corresponds to a rather interesting moment: that of the evolution of trichromacy where the remainder of the visual system has already in some way developed to the point of tracking, though imperfectly, more than simply two dimensions of colour, though it lacks the third receptor type that would provide a huge improvement for this process.

The recovery in the present model is not perfect; and whether its weaknesses and strengths match the weaknesses and strengths of those protanopes who succeed best in recognizing the colours of things is matter for further investigation. But we see already that Cadmium Red and Viridian, which at a single glance might seem indistinguishable to the protanope, can be distinguished. The information is available in the protanope’s perceptions to allow a pretty good recovery of colour in something like an RG dimension as well as the YB dimension. Whether this method—seeing how things behave under variations in illuminant—is a method that is actually used by those protanopes who prove successful at identifying colours is a matter for further empirical work. Of course, we have also seen that there are some protanopes who seem to remain bad at red-green discrimination even when given large-field samples and good amounts of time: the individual differences will surely be worth investigating.

6.4 Other kinds of lighting change

I shall not examine other kinds of lighting change in detail, but it may be worth mentioning just one change in the opposite direction: from direct sunlight (D₅₅) to a yellower (rather than bluer) illuminant, like CIE Standard Illuminant A, which represents tungsten light of ~2856 K—approximately, the light of a 100W incandescent tungsten bulb. It turns out that the behaviour of the colours under the change is similar to that which we observed with the change from D₅₅ to D₇₅—only, as one would expect, in the opposite direction: as the light becomes yellower, the greens in general have negative increases in protanope luminance (-6.1%, -2.2%, +4.0%), while the reds have positive increases (8.5%, 9.7%, 17.9%). And a similar approximate recovery of the ‘missing’ RG dimension could be achieved by similar methods to those we have already used.

6.5 Seeing with and without the Macula

The last case that we should look at may be the most important: the effect of the macula. The fovea, the ~2° central area in the retina with the greatest density of cones, is covered with the *macula lutea* (or ‘yellow spot’), a yellow filter that extends over a little more than ~4°. (See e.g. Le Grand 1957, 351-4 and Fig. 15 below.) When we look at an object and then at something else a little to the side of it, we see the object first

through the macular filter, and then with para- (or peri-) foveal vision unfiltered by the macula. We are usually unaware of the effects of the filtering;⁷³ but the fact of seeing both with and without this filter gives us a constant stream of additional information on the colours of things. We might almost say that, as their eyes rove over objects, normal trichromats are getting 5 or 6 dimensional colour information all the time. (Filtered and unfiltered S cones have very different response patterns, with peak sensitivity wavelengths (λ_{\max}) separated by about 10 nm; filtered and unfiltered M cones differ less, but still significantly; and the difference is very small in the case of L-cones. See Fig. 16.) Of course we must remember that there are no S cones at the very centre of vision, but they become most common at about 1° of eccentricity (Stockman & Sharpe, 1999, 63)—which is still well within the macular region. So deuteranopes (who have L- and S-cones) might be thought of as having at least one extra dimension of information, and protanopes (who have M- and S-cones), two extra.

The difference between seeing the same thing with and without the yellow macular pigmentation is remarkably similar to the difference between seeing it in yellowish and then bluish light. It is unsurprising, therefore, to find that the effects are similar to those we discovered with our change in illuminant from D55 to D75. One might even be struck by how similar is the direction of change, at an average angle of 234° in CIE x, y space (to compare with 224° for the change from D55 to D75).

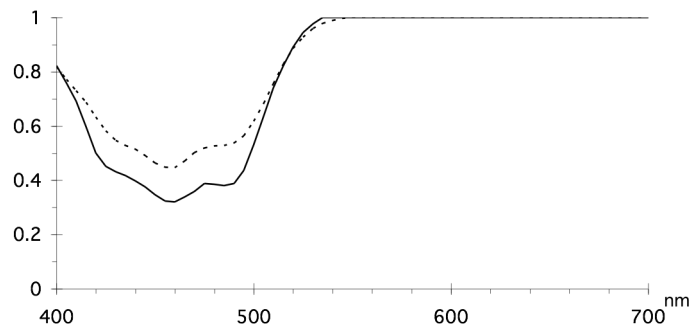


Fig. 15. Transmittance of the macular pigment. Full line gives the transmittance function according to Wyszecki & Stiles 1982, 719-21. Dotted line gives the transmittance function according to Bone, Landrum, & Cains (1992) (my calculation from the log density function available at e.g. <http://cvrl.ucl.ac.uk/database/data>). For the modelling later in this section, I use the Wyszecki & Stiles function.

Red and Green from Yellow and Blue

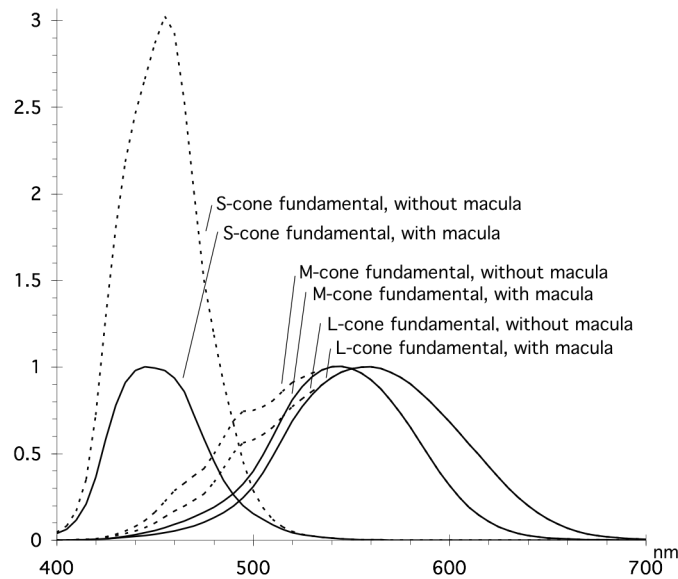


Fig. 16. Hypothesized response-functions for L, M & S cones with and without the macula: König fundamentals derived from the 1931 colour matching functions (Wyszecki & Stiles 1982, 607); and calculated responses of such L, M & S cones in the absence of macular pigment (broken lines; my calculation from macular transmittance function in Wyszecki & Stiles 1982, 721). The calculated responses of the S cones without the macula must of course be interpreted with discretion; but short-wave light is a much stronger stimulus for unfiltered S-cones than for the central S-cones behind the macula, and their maximum response is at ~455 nm rather than ~445 nm.

The effect of removing the macular filter (e.g. looking at an object with parafoveal vision) is substantial. But it is in some ways hard to model and evaluate: we are obviously in no position to remove and replace the macula at will; and there are large interpersonal variations in the degree of pigmentation of the macula. But we have functions at least for the general profile of filtration it provides, and we can model the effect of seeing something without the macula by treating the macula as if it were a filter in front of the eye, and calculating the effects of removing that filter. (The ordinary CIE 1931 2° colour matching functions can be taken to deal principally with foveal vision, which is mediated by the macula; to model the effect of a stimulus seen without that filter, mathematically, we treat the stimulus as if it were multiplied by the reciprocal of the transmittance of that filter.)

Red and Green from Yellow and Blue

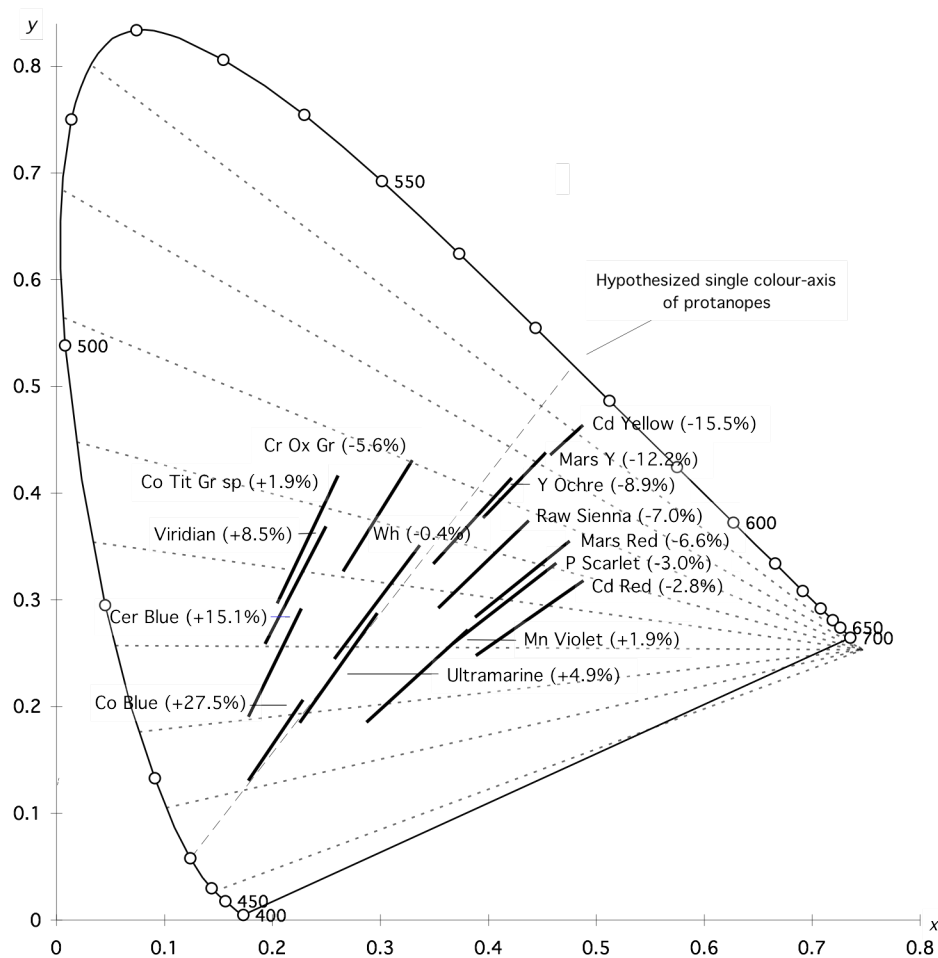


Fig. 17. CIE 1931 Chromaticity coordinates for 15 pigments, with and without macular filter. Figures in parentheses give percentage increase in Protanope luminance. The effect of absence of the macula on protanope luminance is substantial: with removal of the macular filter, the greens increase in luminance (by 8.5%, 1.9%, and -5.6%), more than the corresponding reds (which increase by -2.8%, -3.0%, -6.6%).

Obviously, with the removal of any filter, the total luminance of the light stimulus will increase, and (since the macula is a yellow filter) in our case, the luminance of the blues will increase more than that of the yellows, and the luminance of the greens more than of the reds. To clarify the differential variations among the different hues, I shall pretend that the total average luminance is the same with the macula and without. (If you like, we can read this as modelling the phenomenological fact that constancy mechanisms actually result in more-or-less equal apparent luminance across much of the visual field: there is no question of a peripheral view of an object being phenomenally brighter than a foveal view of it.)

Modelling in that way the effects of removing the macula, we find that for normal trichromats, our green pigments generally have a positive increase in luminance (+9.2%, +3.9%, -2.8%), while the reds have a negative increase (-5.1%, -4.4%, -6.2%) That is, as we saw before with the change from D₅₅ to D₇₅, the greens gain in relative luminance (except for Chromium Oxide Green, which has a large yellow

component), while the reds lose. For protanopes the pattern of increase in luminance is similar: the greens gain by +8.5%, +1.9%, and -5.6%, the reds by -2.8%, -3.0%, -6.6%.

There may seem one puzzle or exception to the general rule I have proposed: while in general the greens *increase* in luminance in the absence of macular pigment, Chromium Oxide Green actually *decreases* in luminance (the increase is -2.8% for normal trichromats, -5.6% for protanopes and -3.3% for deuteranopes). But this is easily understood: Chromium Oxide Green is a relatively yellowish green and (unlike our other green pigments, but like, e.g. grass) it actually reflects a large proportion of red light; hence, as the macular filter is removed (and the light loses proportionately in its red and yellow components), it decreases in luminance. But still, if we consider reds and greens *within a single standard dichromat confusion class* (i.e. a red and a green of roughly equal YB-component), then it is clear that our greens always have a larger luminance increase than our reds under a blue-ening change in illuminant, or the kind of removal of macula that we have been considering. We may still say: a relatively large rise in luminance when an object is seen without macular filtering is a good sign of *greenness* in it.

Our model for the presence and absence of macular filtering delivers a very similar pattern of luminance changes to what we found with the lighting change from D₅₅ to D₇₅. The same methods should therefore work, whereby a dichromat might in large part recover 3-D colour information from 2-D stimuli—first discounting the luminance increase that is due to its position on the YB-axis, and then treating the residual luminance increase as due to its position on a RG-axis. The task is slightly harder, however, and we cannot expect the recovery to be as accurate as with the previous case of D₅₅ and D₇₅. The reason is that, though in gross terms a higher increase in luminance under the modelled change is a decent sign of relative greenness (or lack of redness), there are plenty of exceptions in detail. From Fig. 17 we see that Raw Sienna is less red (or more desaturated) than Mars Red (while being more or less equal to it in yellow-blueness), but its increase in protan luminance under the envisaged change is not higher but lower (-7.0% for raw sienna, -6.6% for mars red). Perylene Scarlet, while less red than Cadmium Red, does not have a higher increase in protan luminance under the change, but a lower one (-3.0% compared with -2.8%). Recovery of the ‘missing’ colour-dimension from the kind of information about macular filtration that we have considered here is therefore almost bound to have inaccuracies of detail.

It is worth remembering, however, that we are not aiming to find cues or information sources that would make the colour-blind *infallible* in ‘recovering’ or identifying the colours of things: they obviously have no such ability. And it could yet be that the strengths and weaknesses of the kind of recovery system that I have been modeling might turn out to coincide very well with the strengths and weaknesses that dichromats in practice display. If so, it would be strong support for the hypothesis that change in luminance as the eye roves over objects is indeed an operative sign of red-greenness for the colour-blind.

Mathematical modeling, of course, cannot decide whether the kinds of cues that I have modeled are the ones actually used by dichromats to recover the ‘missing’ dimension where they do so: it can show

that the relevant information cues are available, but not that they are in practice used. To decide on whether they are used, and in what ways, is a matter for further empirical research.

6.6 Deuteranopes

There are no new points of principle raised for the case of deuteranopes; the same pattern of change in response to the illuminant change emerges. For deuteranopes, as for protanopes, within any one confusion class, it is the greens that have the highest percentage increase in luminance under the shift from D₅₅ to D₇₅, while the reds have the lowest—and this relative luminance increase or lack of it can be used as a sign of the greenness or redness of things seen. (Increases in deuteranope luminance under the change from D₅₅ to D₇₅ are: 3.7%, 3.1%, 0.7% for the greens, -6.2%, -4.8%, -4.9% for the reds.) And something similar occurs again with the presence and absence of the macula: the greens increase by 9.0%, 3.6%, and -3.3%, the reds by -5.7%, -5.0%, and -6.8%.

7. Is this just ‘inference’ to ‘judgments’, not real perceptual experience? Colour and ‘Aspect-shift’.

Some would say that the kinds of factors I have been studying might be in play, but they could only result in *judgments*, not in real *sensation* or *perception* of the troublesome colours. To use the factors I’ve mentioned in identifying colours might seem no better than the old idea in Maxwell (and earlier, Seebeck (1837)) of giving the colour-blind a pair of glasses, with one green lens and one red lens, to enable them to distinguish the things they otherwise confuse. Maxwell claimed that the method yielded perfect discrimination, but no *sensations* of red and green:

By furnishing Mr X. with a red and a green glass, which he could distinguish only by their shape, I enabled him to *make judgments* in previously doubtful cases of a colour *with perfect certainty*. I have since had a pair of spectacles constructed with one eye-glass red and the other green. These Mr X. intends to use for a length of time, and he hopes to acquire the habit of discriminating red from green tints by their different effects on the two eyes. *Though he can never acquire our sensation of red*, he may then discern for himself what things are red, and the mental process may become so familiar to him as to act *unconsciously like a new sense*. (Maxwell, 1855, 141, my emphasis).

The effects I have been investigating are very much the equivalent of seeing something with and without a filtered glass—only my filter is either permanently at work already inside the eye (in the case of the macula), or continually at work outside it (if changes in lighting can be thought of as changes in the filtration of our main illuminant, the sun). Whatever the consequences may be of merely putting on and taking off glasses, however, it seems to me that we should be slower to dismiss the idea that the factors

earlier mentioned might ever have the effect of yielding real experience of red and green. They have of course been in action from birth, and they feed into a brain that is, we may suppose, normally predisposed toward three-dimensional colour-experience. But what we really need is some good evidence on the issue from subjects’ behaviour and reports.

How exactly could we get it? If (as I would agree) even perfect accuracy in the classification of the colours of things at sight is not always enough to show that a person has sensations of red and green, what additional indications might establish this? There are, I think, many kinds of evidence that can help (some of which I’ll touch on in §9), but I shall here consider one particular phenomenon that is, I suspect, extremely significant, though I have never seen it reported in the psychological literature.⁷⁴

I hope I can be forgiven for describing my own case. Of course there are reasons sometimes to be sceptical of such reports; but there are reasons also for thinking that a reflective colour-blind person may know things about his own case that a normal-sighted experimenter will not know unless he is told. In any case, there is a fine tradition represented in Dalton (1798), Nagel (1905, etc.) and Koffka (1909) writing on their own colour-blindness, not to mention William Pole (1856, 1859), Friedrich Schumann (1904) and others.⁷⁵

I confuse certain reds and greens and am protanomalous.⁷⁶ Unsurprisingly (being an anomalous trichromat) I have on occasion quite definite sensations of red (e.g. from fire engines) and of green (e.g. from grass). More surprisingly, however, there are things that look definitely red to me at one time and definitely green at another. And there is a particularly remarkable way in which this can occur. I may see an object and take it to be red (this has happened to me with the dark painted walls of a dining room, with a Lederjacke seen in the window of a shop in Salzburg, and with a multitude of things large and small). A moment later, I may realize I cannot be sure after all—with reds (or greens) like this, I know I sometimes make mistakes. I may then lift up the object and move it around, or (if I can’t move it) move my head to see it from different angles in different lighting; I may take it over to the window (or look at another portion of that same wall, closer to the window); and then suddenly, perhaps, I realize that it is green. At that point, if not just before, the object comes to *look* green. Earlier, it had looked red; now it looks green; and these are two quite different ways a thing may look. And once I recognize that the object is, so to speak, ambiguous for me, I find (and this is the really remarkable phenomenon) that I have some limited ability to make the appearance *shift*—not instantly at will, but repeatedly, with some effort. Taking the object back to the place where it had first looked to me red, I still *see* it as green, just as I now take it to be. But then I may remind myself, ‘A moment ago, it looked red in just these circumstances; so surely you should be able to see it as red again!’; and in that case, perhaps after a moment, the thing may after all shift back to looking red. Conversely, if I tell myself once again that the object really is, as I later discovered over by the window, green, then I can get it to shift back again to looking green. It is like a case of aspect-shift, where by *wanting* to see the rabbit in the duck-rabbit drawing—or wanting to see the Necker cube one way rather than another—, we find that the appearance changes. A difference, however, is that in the ordinary aspect-

shift cases, at least one standard view is that the thinker has ‘the same sensations’ whether she sees the duck or the rabbit, one cube-layout or another; whereas, in the colour case that I have just described, even the ‘sensation’ changes.⁷⁷ Indeed the apparent *lightness* may also change: when I recognize a word on a page as printed in red, it comes to seem *darker* and more clearly delimited than when I see it as green, as well as coming forward from the plane of the paper. One might say that this was a case of an *ambiguous stimulus*, rather than an ambiguous appearance.

I mention this because, though I am an anomalous trichromat, when the kind of experience I am describing here occurs, I am in some respects in the same position as a *dichromat*. Whatever information may be coming from the retina is apparently compatible with the object’s being red, and also with its being green: whatever 3D colour information may come from the eye at other times, at this moment the information available is in colour terms only 2-dimensional, and it is compatible equally with two very different 3D-colour layouts in the world—with there being a red thing there, and with there being a green thing there.⁷⁸ Perhaps the simplest comparison is with the Necker cube: it is literally a case of a 2-dimensional stimulus that can be treated or *taken* in more than one way 3-dimensionally.

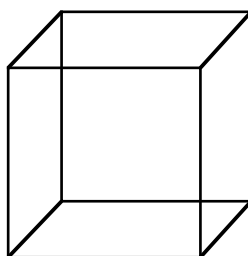


Fig. 18. Necker cube. A 2-dimensional stimulus that can be taken or ‘read’ in two different ways as a representation of a 3-dimensional scene: one might talk of the 2-dimensional image being ‘projected’ into 3-space in two different ways.⁷⁹

An ambiguity in colour-stimuli may seem a rare thing, but there are several comparable phenomena. The Gestalt psychologist Wilhelm Fuchs (1923, e.g. 278) describes cases where a circle which is actually grey can come to appear either yellow or blue, according as it is grouped or taken with one group—of yellow circles—or another—of blue.

And yet we should not think that these kinds of colour-shift are only found, so to speak, pathologically—either in cases where colour-blindness reduces the number of dimensions of colour-information, or else (as in Fuchs’s case), when we are actually misperceiving (e.g.) a grey thing as blue or yellow. We might compare some other kinds of sudden change of appearance. A classic example (Gelb 1929, 674) is the black spinning disk of 10 cm radius, illuminated with an arc lamp whose conical beam is adjusted to fit precisely the outline of the disk and no more. The result, to an unsuspecting person who enters the otherwise weakly-lit room, is that the black disk looks white or pale grey. ‘*This impression of*

white is—under the given conditions—*absolutely compelling*. But the impression changes or flips, as soon as someone puts even a small but genuinely white piece of paper into the path of the arc-lamp and holds it a few centimetres in front of the disk: ‘At that moment we see the disk *black*, the little piece of paper *white*, and both indeed as *strongly illuminated*.’ (Gelb 1929, 674) We might design a similar case with colour: a dark blue disk, set up to fit precisely the beam of light coming from a yellow spotlight, might initially look a pale grey to someone seeing it for the first time; but when a white card is introduced into the path of the spotlight, suddenly the disk should (one would expect) be recognized as dark blue, and the card as white, and both of them be seen as in a special yellow beam of light. This latest phenomenon is different from the cases usually described as aspect-shift, in that the viewer may have relatively little control over how to see the scene: once the white card has been brought into the path of the light, it may be very hard to ‘take’ or see the scene in the original way again. But with a special effort of will and ‘imagination’, once the card has been withdrawn, one might be able to ‘think away’ the existence of the beam of light. If one imagined oneself back into the state of mind one had been in on first entering the room (or perhaps imagined an authority saying, ‘In actual fact, it *is* a pale grey disk after all: while you looked away, we turned off the light and replaced the dark blue disk with a pale grey and turned off the spotlight’), then perhaps one could regain that perception of the card as pale grey. And again, the possibility of the *shift* comes from what we might call the fact that we are ‘taking’ the world, and representing it, to have features in more dimensions than we are reliably getting information about: with just 3 dimensions of colour input, the normal trichromat sees, in a sense, *six* dimensions of colours in the scene—3 dimensions of object colour and 3 dimensions of illuminant colour. (Cp. Katz 1935, 190, quoted in note to §6.1.) And of course, that is bound to result in ambiguities and occasional shocks to our expectations.

When things look red or green to me in this alternating way, this is, I am sure, not just a matter of *judgment*, but of *appearance* or sensation. What makes me so sure? What can I say to make it plausible to others that I really am at different times *seeing* red and green—rather than merely ‘judging’ that there are things of those colours in front of me? One revealing piece of evidence is the fact that in many cases (e.g. when dealing with coloured words printed on a page), when I see something as red, it seems to *come forward* and, when I see it as green, it seems to *recede*: this is surely not just a matter of judgment.⁸⁰ (The word will also look *paler*, i.e. lighter and less saturated, when I see it as green—and that too seems a matter of appearance rather than judgement.) But the best proof may be in the experience of *contrast* effects. I may be looking at an autumn tree. At first it looks to me unremarkable -- with some variation in colour, but nothing particularly striking and no notable colour contrasts. And then I come to wonder if the foliage might be an autumnal mix of red and green. Looking and trying to tell, suddenly I realize: the tips of the leaves are red, indeed a rich throbbing red; and the most central part of the leaves nearer the stalk is green, a good rich green; and as the appearances become definite, the two colours are suddenly in strong contrast with each other. One and the same scene, viewed from the same position, now looks utterly different. And this is clearly not just a matter of ‘judgment’: the red and the green are shouting at each other—there is a

visual contrast that was not apparent before. Judgment may play a role—I may notice a red precisely because someone has told me that it is there—but the role then is in *stimulating* what ends up as being a clear difference in the visual *appearance*.

Such cases suggest, I think, the need to recognize the considerable *time* it may take for determinate impressions to form in colour-blind people,⁸¹ and—perhaps more radically—the need to recognize (as present in a period before the full impression of colour has developed) *indeterminate* colour-experiences. ‘It’s some kind of yellow, but not a pure yellow—I’m not sure if it’s a reddish yellow or a greenish yellow. At this moment it doesn’t look reddish yellow, nor does it look greenish yellow. I can’t tell quite what it is; let me try to get a better view of it.’ *Looking reddish yellow* is different from *looking greenish yellow*: and both are also different from *looking a dark but desaturated unique yellow*—and the present indeterminate experience is identical with none of them.

8. Philosophical conceptions of colour, and how they fit with different conceptions of the relation of experience to understanding

There are of course among philosophers and cognitive scientists many different conceptions of colours. Some talk as if *red* or *redness*, for example, might be taken as literally a certain sensation or kind of sensation; others—more promisingly—take it to be a disposition in objects to produce a certain sensation or kind of sensation. One might initially think that whenever the illumination on an object changes, a perceiver's sensations will change, in parallel with the changes in the stimulus at the eye. It is a definite advance to pay some attention also to the phenomena of colour constancy: that is, to the fact that there is some sense in which, even where there are large changes of illumination, though of course only within limits, a red book will continue to look red. But we must be careful about the characterization of the phenomenon: it is not that the visual system wholly and literally ‘discounts the illuminant’ and yields an awareness of, merely, the constant colour. The wall in front of me looks in a sense a single shade of pale yellow or magnolia, but almost every square centimetre in another sense presents a different appearance, as the light from the several windows in the room falls differently upon it, modified also by shadows from the curtain rails, window frames and other objects, and by reflections from other things around. It would be a mistake to say that the visual system in such circumstances yields awareness merely of the constant colours of things; rather, it yields complex experiences in which we are aware, or able to become aware, *both* of the constant colours *and* of the varying illumination.⁸² The content of the perception might be said to be: of a wall of a more or less *uniform* shade of magnolia, with the *light falling differently* upon it in different places.

It seems to me a further step forward to see that that particular magnolia surface has not merely a disposition to produce a particular sensation S_1 under some particular lighting condition C_1 , but also a

disposition to produce a slightly different sensation S_2 under slightly different lighting condition C_2 , and so on. And then, one might say that the characteristic feature of that kind of magnolia colour was not straightforwardly *the disposition to produce sensation S_1* (or even *S_1 under condition C_1*), but rather, *the disposition to produce a range of sensations S_1 - S_n , under a range of circumstances C_1 - C_n* .

There are arguments for going one stage further again, however:⁸³ We may characterize the relevant character of the surface in terms of its disposition, not to produce a range of *kinds of sensation* under a range of kinds of illuminations, but to reflect a range of *kinds of light* under a range of kinds of illumination. That is: the colour of the surface would be characterized by the way it *changed* the light falling on it in process of reflecting it—for example (to take pure Cadmium Red pigment: see Fig. 9), reflecting only about 6% of incident light in the blue–green–yellow–orange parts of the spectrum (from ~400 to ~600 nm), but sharply increasing proportions through the orange and red (from ~20% at 620 nm to 80% at 700 nm). The relevant character of the surface would be given by its spectral reflectance function, and the colour of an object would be a little like the elasticity of a spring—when a weight of 2 is put on a spring with elasticity e , it stretches to $2e$ (assuming some suitable units), when a weight of 5 is put on it, it stretches to $5e$, and when a weight of 0.3 is put on, it stretches to $0.3e$. (Similarly, the character of the light reaching the eye from a Cadmium Red pigment will continually change as the incident light upon it changes; but there will be a constant function taking us from the character of the incident light to the character of the reflected light—and the types of constant function will be characteristic of the various types of colour.) The elasticity is a single feature of the thing, manifested in a pattern of activity fully exhibited only under a range of different conditions. So also, I believe, the colour of a surface is a single complex feature of it that is fully manifested only as the object is seen in a variety of lighting conditions: to get a full appreciation of the colour of a surface, one has, so to speak, to put it through its paces, and see what it can do to variety of kinds of light. Not that one cannot, of course, in some sense get a pretty good impression of the colour of something just at a glance—just as someone with some experience of springs may form a good impression of the elasticity of a spring seeing it just once, for example, with a standard 1 kilo weight on it. But what he is seeing at that time, if he sees the elasticity, is a power that will only fully manifest itself through a range of different actions (e.g. as one pulls the weight down a bit and sees how the spring bounces, or as one replaces the first weight with another and sees the results). And so also with colour: I may take it at a glance that what I have in front of me is a piece of white reflective card, but I will only be able to confirm that attribution to the thing if it goes on to react suitably as I pass my hand over it and see the pattern of shadows cast, or as I bring it close to the relatively orange light of a table lamp. If, on the other hand, it actually doesn't change its appearance, but goes on obstinately looking unvaryingly as it did at first, then I know that I have a strange glowing object before me—not a white reflective surface at all. (And actually, even if it is glowing, it should still reflect some light from things around and show some, even if relatively slight, variation in appearance from the different kinds of incident light—so if it doesn't

do so, it is, even more weirdly, a black body, emitting light (at the high temperature associated with white) and absorbing any light that falls upon it.)

Where does this leave our present debates about colour-blindness? Let me start with a first approximation. Suppose, as on the standard view, whether in Helmholtz's version or Hering's or some other, that dichromats lack a certain kind of colour sensation, because they lack a certain kind of colour receptor. In that case, the protanope—lacking the red receptor, or, perhaps even the 'red-green' colour system—will simply be unable to see red, if red is either a (certain kind of) sensation or a disposition to produce such a sensation. But if red is, for example, a *disposition to produce a range of sensations*, or if the redness of surfaces is—as on the suggestion of the last paragraph—a *disposition to change the incident light* in certain ways, then one might actually be able to allow the protanope in some sense still to see red. Even if we suppose (as we are currently doing) the absence of 'the sensation of red', it might be that the protanope could still recognize or represent red. Suppose one of the 'signatures' of a red thing is to darken as the light changes from (yellowish) sunlight to (bluish) skylight: that signature could be picked up upon by the protanope: lacking the 'sensation' of red, he might none the less be able to recognize red things. And the lack of 'the sensation' might in some sense not matter much. People wearing sunglasses can *see* the whiteness of a sheet of paper without having 'a sensation of white' from it: the pattern of appearance, even if, in some sense, entirely brownish, is still the pattern of appearance of a white thing—and it is white that the thing looks to be. If we view a surface colour as a power to produce a range of appearances under a range of conditions, then, one might say, the person with sunglasses may see only *one sub-set* of those appearances, and still be said to see white. (But one would have to admit that he doesn't see the same manifestations of the whiteness as a person looking at the object without sunglasses.) If on the other hand we view a surface colour as a power to change the light in various ways, then we might even say that the person with sunglasses saw *the very same* manifestations of whiteness as the person without: the two of them might even be watching the same pattern of changes in the world, whether mediated or not through the sunglasses, as one of them, for example, moves her hands over the objects at a distance of a couple of centimetres, watching the shadows cast, and moves the objects around to catch the light from different sources in different ways. It could be no less true that they saw the same colours and same patterns of variations in light reflectance, as that they saw the same shapes and sizes of the objects, even though one viewer was obviously enough closer, and the other a little further away. I have mostly worked with this kind of conception in this discussion: allowing that the dichromat may lack a certain kind of colour sensation, I have been interested in how he might none the less *see* colours—making use of colour information available through variation in illumination over time and space, or with and without the macula.

But a further step could be taken. One might conjecture that, if the information for colour-identification is available, perhaps in some sense the 'sensation' of red itself might also be available. Must we assume that the processes in the brain that underlie the sensation of red only ever occur in response to

the standard textbook pattern of activity involving both L and M cones (e.g. positive firing in some L-M opponent process system)? Might the ‘sensation’ of red not occur, not merely in response to some such particular momentary stimulus, but also in response to a suitable *pattern* of stimuli—e.g. the pattern of *darkening-under-a-change-from-sunlight-to-skylight*? And might not the dichromat, in that case, be lucky enough to be able to enjoy some sensation of red despite the absence of what in normal trichromats is its most obvious standard cause?

A third option—besides saying that the dichromat might *see* red and green while having rather different ‘sensations’, or alternatively saying that he might after all be able to have the normal ‘sensations’—might be to say that the notion of a sensation or *quale* of red might need to be rethought altogether. Not that there is no such thing as experience of white, or of red; but the idea that there is an identifiable quale of (say) white, or of red₂₇, may make little sense, any more than the notion of an quale of say, shininess. Of course one can experience the shininess of something, but that experience comes from having a range or *pattern* of experiences, not from having just one. And maybe the idea of a quale of white or even red₂₇ make no more sense than that of a quale of shininess. But then, what of the experiences that make up this range or pattern? Would they not be still describable as qualia? Maybe: but there might still be questions about the coherence of identity conditions for such qualia in ways that do not arise with other things or other kinds of experience. This is not the occasion for debate on that; but at the very least, our investigation of philosophical conceptions of colour has already given us two promising lines of thought that seem to leave room for the idea of dichromats having genuine sensory experience of colours, even if they do not see them in quite the ordinary way that the normal-sighted do.

9. Some Experiments for the future

I would like to propose a principle of some importance, though it may sound naive at first:

We need to spend more time investigating what the colour blind *can* do, rather than what they can’t.

From the late 19th century, colour vision testing was mostly driven by the need for tests to prevent people from having jobs on the railways and at sea where they might be a danger to others, or in the textile or paint industries where they might be slow or unreliable—while on the other hand, there were young men hoping that a bit of coaching (or vitamin A) might save them from rejection. While much effort was put into contriving conditions in which failures of the colour blind become clear (with small and desaturated samples, and lights like the Macbeth lamp), the relative success of those people under other conditions was often treated as little more than an annoyance—either insignificant, random, or fraudulent. We know, however, that the successes are not random and they certainly need not be fraudulent—they are persistent

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and repeatable enough, even under controlled conditions, to be a headache to people designing diagnostic tests—and we owe ourselves a better understanding both of what they amount to, and of how they occur. As subsidiary principles, therefore, I would like to propose more investigations of the colour-blind in those conditions where they tend to do better rather than worse. We should:

- (1) Have more experiments using *surfaces*, rather than *lights*.^{84, 85}
- (2) Investigate with *large* samples, not just *small*;⁸⁶
- (3) Use *saturated*, as well as *desaturated* samples;⁸⁷
- (4) Test how subjects improve (or deteriorate) when given *more time* to look at samples;⁸⁸
- (5) Investigate performance under *varying illumination*: not just brighter and darker,⁸⁹ but also deliberately of different spectral composition (e.g. yellowish and bluish)—especially where one object can be seen under a *succession* of different kinds of light and the ‘dynamics’ of a surface colour can be exhibited and tested; also cases where objects are seen *simultaneously* under a *variety* of kinds of illumination coming from different directions;

Two proposals relate to issues that came up when considering contrast effects and indeterminate colour impressions (in §7). We should, with the colour-blind:

- (6) Investigate perceived *structure* and colour *relations* (e.g. being *darker than*, *redder than*, *whiter than*, and *more saturated than*), rather than just *colour-matching* and *colour-recognition*.⁹⁰ We should investigate, in particular colour arrangements and juxtapositions, what things are seen as *contrasting with* other things and (though this is a further issue) as *coming forward from* or *receding from* other things.
- (7) When subjects are being asked to describe the colours of things, check not only the *degree of confidence* in the judgments they make, but also the degree of *indeterminacy* in their colour-impressions. If subjects are forced (for example) to use a basic colour term in response to a presentation, let them not only be allowed to say if they feel unconfident in their judgement; but also to report if a change in circumstances (e.g. more time, or looking from closer up) results in a more determinate colour impression.

One last principle is, I suspect, extremely important, though I cannot do more than mention it here. We should :

- (8) Pay attention to the *individual differences* within the present classificatory groups, which may tell us about (i) sub-groups with different genetic endowment and different receptor systems in the

eye, and (ii) the different extents to which people can learn to compensate for deficiencies in their sensory systems.⁹¹

Using these principles and others, there are many questions that we might be able to begin to give a good answer to. Some of them arise particularly out of the issues of §§6-7: What is the *structure* of the colour space of dichromats—and is it 2-dimensional or 3-dimensional? (Of course, the answer may not be the same for all dichromats, in view of (8).) One helpful way to investigate it, I think, would be to investigate a subsidiary issue: How widely is what I called ‘aspect shift’ in colour found among dichromats? And where it does occur—for example, with alternations between what are reported to be appearances of red and green—, is the idea of *projection* into a third colour-dimension (perhaps as in §6 above) helpful in modelling how it occurs?

There are many experiments that I have begun to sketch, that should contribute to just such questions, but I shall here just mention three. The first (1) is best illustrated with a diagram.

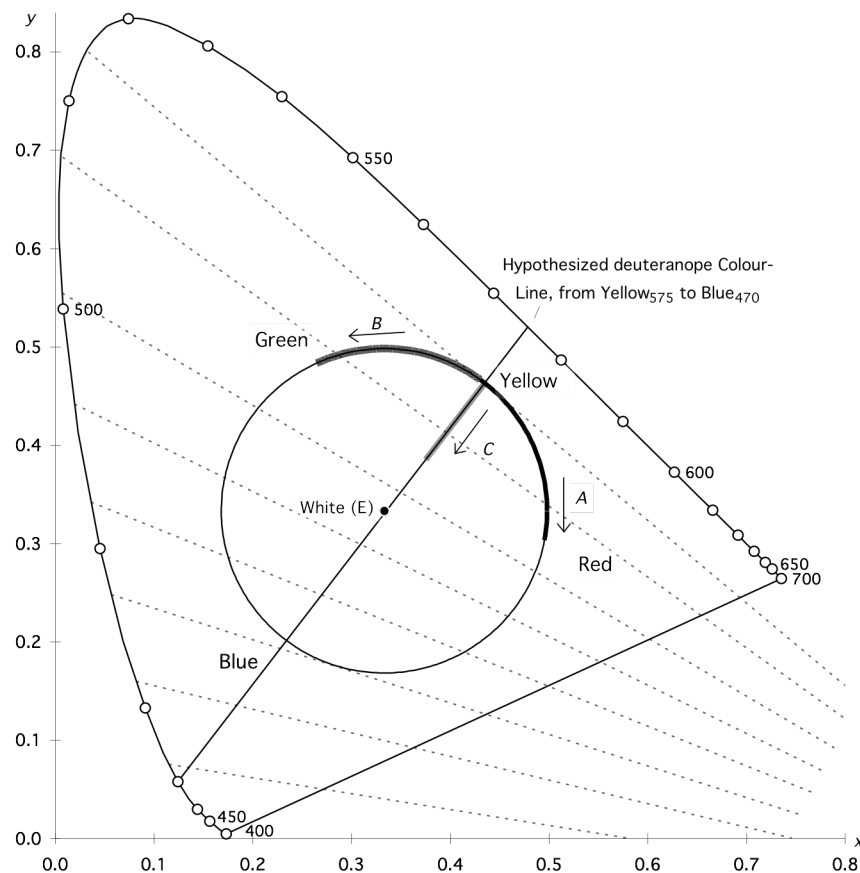


Fig. 19. *Appearance of colours from a Colour Atlas to a deuteranope, on the Standard Model: some experimental tests.* The circle is a rough illustration of the chromaticities of a range of pigment samples in a Colour Atlas at fairly high saturation.⁹² The standard confusion lines (dotted) indicate how these samples would (for a theoretical deuteranope)

project onto the standard Yellow-Blue hue-line. I have marked three sequences of colour-samples: Sequence A runs on the periphery of the circle, approximately from Unique Yellow to Unique Red; B runs, again on the periphery, approximately from Unique Yellow to Unique Green; Sequence C is a series of increasingly desaturated yellows, running along the diameter of the circle, in the direction of a neutral grey. On the standard theory, sequences A and B should look more or less equivalent to C, approaching a neutral grey. We should test whether certified dichromats do in fact find these the relevant sequences equivalent; and if not, then how they describe the differences between them. If some of the colours in Sequence B seem to be tending increasingly towards green, then we should see if there is any tendency to treat that colour Green as a 'Unique' colour: that is, as a colour, some instances of which can be phenomenally pure or unmixed. (And if so, we should ask the subject to point out an instance of this pure green.) With tests like these, we can learn much about the structure of the experienced colour space of the dichromat.

(1) Let us take a sequence of colours at fairly high saturation and medium and equal lightness (for the relevant type of dichromat being tested), ranging from Yellow to Red (Sequence A in the accompanying Figure). On the standard model, dichromats should see this sequence of increasingly red samples as actually a sequence of increasingly desaturated forms of yellow tending towards grey (as in sequence C). So, (i) let us ask, when participants are presented with Sequence A: Do you see this sequence of samples as *getting redder*? (Alternatively, or in addition: Do the samples look to be *getting less saturated*, i.e. *losing intensity of colour, coming to look more like a neutral grey*?) Try again with a sequence of samples going from *yellow* to *green* (Sequence B). Try again with a sequence from *yellow* to a desaturated *yellow-grey* (Sequence C). The three sequences should, on the standard model for deuteranopes, all yield the same answers. Much of the evidence (reviewed in §5) already suggests we should expect a rather different outcome—that, as long as the fields are reasonably large, these various sequences will not in fact be seen the same way—; but the details of an empirical investigation would I think be of interest.

(ii) If the traditional view were correct, then a strong red should look a desaturated yellow or (for protanopes, even a grey). So let us ask (showing a deep red and a supposedly equivalent desaturated yellow): Which of these samples is most highly saturated (or 'has a deeper colour')? And (showing a series of reds): Which of these is the deepest or purest red (i.e. the least yellowish)? Is it (namely, the one chosen as most pure red) a *strong* (or *saturated*) colour or a relatively weak (or desaturated) colour? (We surely should be able to rule out that something might at one and the same time look a saturated red and a desaturated yellow?!) (Presenting the participant with what they have chosen as the purest or deepest of reds): Is there any sense in which this sample looks yellow? Is it in fact just a yellowish red? We could present participants with an extension of Sequence A beyond unique red into the purple region. Ask: You've described the earlier sequence as a series of samples of increasing redness [supposing this is true]; do you find the redness continues (in the further region A', the extension of the sequence) to increase? Or is there a pure 'unique' red among all these samples—and if so, please indicate which it is? Depending on the outcome, we might have very good confirmation that some dichromats were quite capable of distinguishing

hue-variation from mere saturation-variation—that is, that they had (along with brightness) three dimensions of colour-vision after all. It is worth noting that, in the phenomenal colour-circle, unique red, *stands out* or is, for normal perceivers salient; but there is nothing particularly special, in the sequence of progressively more desaturated yellows, about any (supposedly dichromat-equivalent) desaturated dark yellow or grey—so it really is particularly implausible to think that correct labelling (if it occurs in our dichromats) of samples as ‘unique red’ can be dismissed as just a matter of ‘judgement’ or ‘inference’ (labelling a particular shade of greyish-yellow ‘red’).

(iii) Show Sequence A (that we might loosely call ‘the reds’) and Sequence C (‘the yellows’), which (according to the standard theory) should be indistinguishable to the relevant group. Ask subjects: ‘Do these two sequences of colours look the same?’ Have one additional sample, which actually belongs in the red sequence between two other samples. Ask the subject to place the additional red sample next to the samples that most nearly resemble it (either among the reds or among the yellows). —Of course we really know which outcome to expect if the reports surveyed in §5 are correct: if the fields are reasonably large, the subject should place it among the reds; but, again, the empirical details as they emerge could be very interesting.

All of this could be repeated with larger and smaller samples; and (in slightly different form) with suitable randomizing of the colour presentations—trying to find out the relevant dimensions of colour space for the relevant groups of dichromat: it might well be that dichromats do better seeing certain colours correctly when they appear in relevant sequences rather than when out of context.

(2) Jameson & Hurvich say that, when the subjects in their 1978 experiment (see §1.1 above) were asked to give the colour names of the Farnsworth caps and did so so remarkably correctly, there was *no change in their experience* as a result of the questioning procedure. The authors give no particular basis for their claim, and it would be worth checking, particularly in view of the fact—unmentioned by Jameson & Hurvich—that the colour-blind often report that it takes time for determinate colour-experiences to ‘develop’. My own experience while doing the Farnsworth D-15 test under a Macbeth lamp,⁹³ was that the caps were extremely hard to identify or put in any colour-order, and I mixed up reds and greens in a typically protan way. At slightly higher levels of brightness (with ‘daylight’ fluorescent lighting) the colours continued at first to be hard to identify; but, when the conductor of the test asked ‘what colour is that?’, I suddenly found (quite to my surprise) that I could answer almost immediately and (as the experimenter later confirmed) almost entirely correctly, and I then went on to put the caps in virtually the correct order; and I found (as I would put it) that the caps were coming to *look* the colours I was describing them as being, which they had not done before. The greens looked green, the reds and pinks looked red and pink, whereas earlier they (and indeed the blues too) had looked an indeterminate medium-dark colour: I had had the feeling only of seeing something that I could not ‘place’ or identify in colour. (And, note, the change did not occur simply with the brighter illumination; the really decisive change occurred only when the experimenter began to ask the questions.) And, whereas, at the earlier time, I had placed cap 2 between

caps 14 and 13, I later came to place it between caps 1 and 3: the *patterns of apparent similarity* among the caps had changed.⁹⁴ It may well be, that hearing and trying to answer the question ‘What colour is it?’ itself provokes either a different way of looking or a different mode of brain processing, and that this occurs not just in myself but in dichromats. Dichromats often report that it may take time for their more determinate colour-experiences to develop. We should see whether we can replicate this stage of the Jameson & Hurvich experiment, to see whether other subjects report a richer colour experience developing as a result of the extra time and perhaps better illumination and the request to give the colour-names of the caps. By examining patterns of similarity that dichromats find among the caps, we might perhaps find that, even if when first presented with the test caps under the Macbeth lamp, a subject had what could indeed be described as a 2-dimensional colour space, still perhaps under better conditions (when asked the colour-names, with better lighting, larger samples, etc.) he might prove later to have a 3-dimensional colour space.

(3) On the subject of ‘aspect-shift’: our first task would be to find out how widely the phenomenon can be found among dichromats as well as anomalous trichromats—how much there are ‘ambiguous’ stimuli that are reported to ‘shift’ in colour-appearance for them. A second task would be to try to identify exactly which are the reds and greens particularly liable to be ‘ambiguous’ (for perhaps a particular subgroup), and what kinds of shift in appearance they undergo. I shall not pause here over those tasks; but if we had some success with them, there is one more unconventional line of enquiry we might explore. Suppose we have identified a particular dark green that is (for the relevant group) liable to be seen *either* as a dark green *or* as red. Let us give a dichromat a ball A of the relevant dark green, in lighting condition C; suppose it initially appears red to him; he then moves it around, takes it to the window, comes after all to see it as green; he brings it back to the earlier condition C and still sees it as green. We might take it that *tracking* the object is important to its having now a relatively settled appearance: the appearance in C is influenced by the history of the object’s having being seen in other circumstances, which have (by cues perhaps like those we modelled in §6 above) revealed its true colour and given it a relatively settled appearance. But then, with the subject looking at ball A still in circumstances C, suppose we produce another (in fact practically indistinguishable) ball B of the same dark green. Now, does it ever happen that ball A is seen as green by the subject, while ball B is seen as red? Or do they always get seen as of exactly the same colour?

Suppose they actually do sometimes get seen differently. What if we now take the two balls from the subject? (Presumably ball A continues to look green, and B to look red.) We put them behind our back, shuffle them, and then present them to the subject once again. Suppose he does not know which of the two is A and which is B; does he then see them both as red, both as green, or (perplexedly and perplexingly) see one as red and the other as green? (After all, he’s currently under the impression that that’s what his environment really contains: one red ball and one green.) And in that case, if an aspect-shift can be brought to occur, does it ever happen that the subject shifts from seeing one as red and the other as green to seeing the other as red and the one as green?!

The options can seem almost too absurd to contemplate. But the question is only a little more far-fetched than some of the cases in Fuchs and Wertheimer, and we really know far too little about aspect-shift to know what the answer will be.

10. Conclusion: The Metatheory of the Debate

At every stage of the evolution of this subject, at least since Dalton gave it a first really thorough description, there has been evidence of great authority — whether it was ultimately to be accepted or not —, against the ‘yellow and blue’ theory. While John Herschel (in 1827 and 1833) was arguing that Dalton saw only yellow and blue, Dalton’s own 1798 report had already given a strong indication that he sometimes saw green things *as red* and red things *as green* (where *looking red* and *looking green* certainly did not seem the same as each other, nor the same as some variety of looking yellow or grey either) and Dalton himself thought that he often saw the colours correctly in candlelight. As Maxwell was coming to publish his first, brilliant, paper on the subject (1855), he also came across George Wilson’s articles in the *Edinburgh Monthly Journal of Medical Science*, based on Wilson’s studies of some 700 men in the Edinburgh garrison and twenty or so people who had written in response to a request for personal reports of colour-blindness that he had made in the pages of the *Athenaeum*. And Wilson believed his evidence pointed to a conclusion quite opposite to the two-hue theory that Maxwell supported.⁹⁵ While Helmholtz advanced a version of the two-hue view, the editors of the third edition of his book had different views on the matter: von Kries expressed reservations,⁹⁶ and Nagel, as we have seen, produced dramatically opposing evidence—both from his own case, and from many dozens of other dichromats. One might have thought that, from the early 20th century, the view that dichromats see more than just two hues would be at least a respectable minority view, to be mentioned if also rebutted by those advancing the more standard view: but in the pages of Pitt, Wright and Judd there is scarcely a mention of it. Pitt (1935, 34) mentions Mary Collins, who in turn quotes from R. A. Houston’s (1922) paper; but Collins and Houston are, I am afraid, not the equal of Nagel or even George Wilson,⁹⁷ and in any case, Pitt gives them no serious discussion. Judd (1948) talks of Hayes (1911) and Nagel (1905), but as if they actually *supported* his view—which is the very opposite of the truth.⁹⁸ Curiously, even though Boynton was a collaborator on many important studies of residual red-green discrimination in dichromats, still, in his own 1979 book, when he feels called upon to deliver a general statement on the experience of dichromats, he reaches again for the standard yellow-and-blue theory.⁹⁹ Jameson & Hurvich 1978 have evidence of red-green discrimination in their dichromats, but still state—offering no particular evidence, as if it were it were simply *established fact*—that their dichromat subjects can only have perceptual experiences along the yellow-blue axis¹⁰⁰ and must therefore be using a rule like ‘if it’s dark then call it red’—a rule that they also admit could not account for the behaviour of their subjects.

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It is almost as though there were a kind of blindness in the supporters of the standard view even to the *possibility* of an alternative. My suspicion is that these authors know that opposing evidence exists; but they do not take it seriously—for example reporting it honestly and giving reasons to reject it—because, in their view, it has literally no chance of being true: to suppose that, with just two dimensions of input, dichromats might have three dimensions of colour experience would, for these authors, be to believe in a kind of *magic*.¹⁰¹ In a way, this is the view even of many of the dissenters of recent years like Nagy, Montag and Boynton: when they find residual red-green discrimination in dichromats, they also almost invariably go on to speculate on the *extra receptor type* that, they suppose, must be at work—whether rods or residual cones of the ‘missing’ or ‘forbidden’ (the terminology is remarkable) type. My own view is that it is not particularly plausible that either rods or a few remaining cones of the supposedly ‘missing’ type are responsible for the red-green discrimination that has been found. There are further hypotheses that have been suggested—like variations in pigment density (whereby one pigment could produce different sensitivity profiles in variants of a single cone-type)—and these hypotheses are both interesting in themselves and promising as possible factors in the phenomena we are studying. But a controlling assumption in almost all of the work has been that for a three-dimensional space of colour, we have either to find or postulate three kinds of receptor—that 2 receptor types can provide at most a 2-dimensional space of colour experience. A central project of this paper has been to question that assumption. Merely questioning it is of little value, however, without some conception of *how* it could actually be false: a model of how three dimensions can come out from two. This is what I have attempted in §6, suggesting that what matters is not so much which receptors we have as what we do with them—something that I have attempted to link with a more dynamic conception both of colour vision and of colours themselves (§8). Suggestions of actual underlying mechanisms will have to come later.

So, what do the colour-blind see? I do not know, and suspect it will take some time for us even to follow through the kind of experimental tests I sketched in §9 that I think would prepare us for an answer. But to hazard a guess, on the basis of the evidence we already have: quite possibly pretty much what normal perceivers see. Only not of course always at the same time or under the same circumstances as normal trichromats—and particularly not with small samples, in poor lighting. And probably not with anything like such fine discrimination as normal perceivers. But as for the broad structures of the colour space of the normal trichromat—hues (forming a circle), saturation, and brightness—I would be very surprised if the majority of dichromats did not have all of that.

But—and this is another theme of the whole essay—I have to say that I do not think any of us has much knowledge at the moment on the matter. There is virtual certainty that there is red-green discrimination among many dichromats—though it is time that this were regarded not as a peripheral detail but a significant truth (and it is only one of several reasons for having reservations about confusion diagrams like Pitt’s (§3)). But whether many or most of the discriminating dichromats also have *sensations* of red and green may be genuinely uncertain. The opponents (like Judd) mostly oppose the claims for

extremely bad reasons—as we have seen (§4). But at least some of the supporters could surely be wrong too: Pole (1859) declared that he had been quite convinced for 30 years that he could see red and green, but later decided he had been deluding himself. I do not think Pole's reasons for saying this are good ones; but he might conceivably have been right and there are likely to be important individual differences. The balance of probability seems to me mainly with the reports of Nagel (and Dalton indeed): but we certainly need more empirical work to find out how widely their condition is shared—whether many dichromats have red and green experience or only a few, and if so, how exactly they manage it.

People sometimes talk as if colour vision were something on which the main work was done in the mid-19th century and redone, with greater accuracy, at the start of the 20th; and psychologists know just about all they need to know—with the exception of the genetics and neurophysiology that will show how the processes long known about are realized and come into existence. Meanwhile the psychophysics of colour can be thought of as working with early-20th-century equipment, while the fruits of newer technology, whether in computing or genetics or functional MRI, have transformed experimentation to a much greater extent in other areas of psychology and cognitive science. These contrasts seem to me rather overdrawn. But in fact my suspicion is that large issues remain quite unexplained—the Land phenomena, the character of dichromat discrimination and experience, the modelling of the experience of anomalous trichromats, much of colour constancy in normal trichromats—and to work out explanations of those phenomena, we are going to have to rethink much of the work that has been taken for granted for so long.¹⁰² I am no fan of the idea that scientific revolutions overturn what came before—on the contrary, I would say that they often preserve the core of what had earlier seemed established, but transpose it into a new key, or put it in a new frame. What Newton, holding a particle theory of light, had said, about 'Fits of easy Reflexion and easy Transmission' (1704/1730, 348) was rephrased a century later in terms of wave activity and has been rephrased once again by people doing quantum electrodynamics; but much of Newton's quantitative and explanatory work can be kept, though rephrased in new terms. I have a suspicion that we will not have an explanation of the phenomena in Land's red-and-white presentations and of what (if Nagel is right) seems to be 3D colour experience on the part of dichromats, until we put at the core of our models of vision the dynamic and the varying *series* of experiences, rather than the static pairs of stimuli that were the main object of so much experimentation with spectral apparatus—and that are actually the main experimental basis we have for our standard conceptions of colour blindness. If we have success in that direction—some of which, I suspect, will rather significantly depend on adventurous use of the technologies of computing, genetics, and MRI—then I rather expect that everything will look different, and yet the old things in Helmholtz and Maxwell will also in a sense look much the same, rather as the theory of complex numbers left the theory of real numbers looking both the same and different. An emphasis on the dynamic, on contexts and change, is, in a sense, a commonplace of the theory of vision in general—in spatial vision and object recognition, for example. It has not been a commonplace in the central theory of colour vision—though it has had its place in what seemed sub-fields of constancy and contrast

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phenomena—because colour has so often seemed the *simple* out of which the other complexes are formed. But maybe that too is an appearance that could change.

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² A basic reading list of classics would include Helmholtz 1867/1924, §§19, 20; Maxwell 1855 and 1860; and (for those with mathematical interests) Grassmann 1853 and the very remarkable Mayer 1775. For more recent theory, excellent introductions and masterly surveys, of very different types, are Evans 1948, Le Grand 1948 (vol. 2 of [1945-1956], tr. 1957/1968), Boynton 1979, Wandell 1995, and Mollon 2003.

³ For the work of W. D. Wright and others that fed into the standard colorimetric systems of the C.I.E. (*Commission internationale de l’éclairage*), which we will be using in later sections, see Wright 1944/1969; a helpful introduction is Hunt 1987 (with later editions 1991, 1998). On technical issues, the standard authority is Wyszecki and Stiles 1982.

⁴ ‘As there is little hope of detecting it [the exact nature of the ‘anatomical contrivance’ at work in the eye] by dissection, we may be content at present with any subsidiary evidence which we may possess ... furnished by those individuals who have ... a variety of ... Colour-Blindness’ (Maxwell, 1855, 137). On the basis of the confusion points of the various kinds of dichromat (see §3 below), together with the CIE colour-matching functions for the normal observer, Wyszecki and Stiles 1967 calculated the sensitivity profile of each of three normal cone types: see Wyszecki and Stiles 1982, 604-8; cp. König & Dieterici 1892, Judd 1952, 104-107, Vos & Walraven 1971.

⁵ Blue rather than violet was sometimes taken as primary, but I shall mostly not mention that option separately here.

⁶ I shall explain the classifications further in §2. I shall occasionally talk of ‘dichromats’, where what I mean is strictly ‘the 99.9% of dichromats who are protanopes or deutanopes’, setting aside the very rare cases of tritanopia.

⁷ Cf. John Herschel, 1827, §507, p. 434; see also his Letter to John Dalton, 22 May 1833 (in Wilson 1855, 60). ‘All hues for the colour-blind eye can be obtained by mixing yellow and blue’ (Helmholtz 1867, 295; 1896, 362; 1924, II.148).

⁸ For similar views, see Le Grand 1957, 329 (Fr. edn. ii.333); 1968, 344; Boynton 1979, 380; Hardin 1988, 145-6; Neitz & Neitz 2000, 695; and, hypothetically and with some philosophical differences, Byrne & Hilbert, this volume.

⁹ ‘*Saturation* may best be defined as the percentage of hue in a color. In common speech the saturation of a given color is described by such words as pale or deep, weak or strong, in connection with the name of some hue. The concept is roughly parallel to that of the purity of a chemical compound or the concentration of a solution.’ (Evans 1948, 118)

¹⁰ The size of a visual stimulus is often given in terms of the ‘visual angle’ it subtends at the eye: a circular cap of diameter 1 cm at a distance of 60 cm, for example, subtends an angle of about 1°. For present purposes, I shall describe 1° stimuli as ‘small’; by large fields, I mean those of 8°-10° or more. For ‘spectral light’, see Note on terminology in §2.1.

¹¹ There are many directions in which people have taken these issues further; for one line of development, see O’Regan & Noë 2001 and Noë 2004, esp. ch. 3, which take up some proposals from Broackes 1992.

¹² Kuhn 1962/1970, esp. chs. 6-8, argues that a scientific theory is typically not rejected simply because (as we might say) the theory ‘conflicts with the evidence’. (On the contrary, we live all the time with ‘anomalies’ and recalcitrant evidence which ‘normal science’ may ignore, or treat as unimportant, or try to explain (whether with epicycles, or ideally, with success).) Rejection typically occurs only when some more attractive theory comes along, which gives theoretical significance to recalcitrant phenomena that had previously seemed unimportant or incidental.

¹³ Pitt, on Abney’s wool test: ‘while most of the dichromats were readily detected while using this test, others managed to get through it without making a fault’ (Pitt 1935, 31-32).

¹⁴ The Report of the Committee on Colour Vision 1892 (see Royal Society 1890) gives in the Appendices (368ff.) a survey of the means of practical diagnosis available at the time; for more recent methods, see Pokorny et al. 1981, or Fletcher & Voke 1985 chs. 5-8.

¹⁵ *Note on terminology*: ‘spectral light’ is light from a portion of the spectrum (e.g. a narrow band specified, approximately, as 652 nm \pm 1 nm), produced, for example, by passing sunlight through a prism (or by reflection or transmission from a diffraction grating) and selecting a suitable portion by means of a narrow slit in a metal assembly. I will talk at times of ‘red light’, ‘yellow light’ etc., meaning light from the red part of the spectrum, the yellow part of the spectrum, etc. (A mixture of red and green light (with no light from the yellow part of the spectrum) may indeed also look yellow: but when that is what I have in mind, I will give further specifications.) I will at times specify the wavelength (in nanometres) of a light by using a subscript number, e.g. Red₆₇₁ (for red light of 671 nm) or Green₅₂₀ (for green of 520 nm); and sometimes I shall abbreviate the main colour terms by using just the initial letter, e.g. Y₅₇₅ (for yellow light of 575 nm), or B₄₄₀ (for blue of 440 nm). All colour terms used in such labels are intended as rough indications, not exact specifications.

¹⁶ For a classic presentation of chromaticity diagrams, see Wright 1944/1969 chs. 3 & 4; Hunt 1987, chs. 2 & 3, is a good textbook account; a popular introduction is Rossotti 1983 ch. 15.

¹⁷ This slightly undercuts the simple account I gave earlier of Rayleigh matches and the principles of the Nagel anomaloscope: if in these colour-matching experiments a mixture of R₆₅₀ and G₅₃₀ is not a perfect match for Yellow₅₇₃ unless the latter is desaturated with some blue, then we should not expect any mixture of R₆₇₁ and G₅₃₅ to be a perfect match for Y₅₈₉ in the Anomaloscope, without some blue to desaturate the Y₅₈₉. And if the L : S (or M : S) cone

response ratios are actually different for R_{671} and G_{535} —as indeed is the case according to e.g. the Stockman & Sharpe (1999) functions, and the König fundamentals derived from the CIE XYZ functions—then we might think we there had a basis for dichromats (lacking M or L cones) still to distinguish R_{671} from G_{535} even in the anomaloscope. This is all true, but the effects are small and on this basis, the difference for a deuteranope between R_{671} and G_{535} would still only be the same as the difference between R_{671} and R_{671} very slightly desaturated with some white. The issue is worth further study, however; and it is worth remembering that the newer anomaloscopes that use LEDs to simulate the Nagel primaries are not doing quite the same thing as the original model.

¹⁸ This should be read: 10.0 units of Y_{580} *mixed with* 0.11 units of B_{460} *matches* 2.88 units of R_{650} *mixed with* 3.26 units of G_{530} . The + sign signifies additive light mixture; the = sign signifies matching. The units used here for the R_{650} , G_{530} and B_{460} primaries are set on similar principles to W. D. Wright's system (so that a match for $Y_{582.5}$ requires equal numbers of units of R_{650} and G_{530} ; and a match for G_{494} requires equal numbers of units of G_{530} and B_{460} ; cp. Wright 1946, 125-135), but values are my own recalculation from CIE XYZ functions. The units for Y_{580} are arbitrary.

¹⁹ Grassmann 1853, 78; 1854, 260; wrongly listed as Grassmann's second law in Hunt 1987, 206; and completely travestied in the English translation of Helmholtz. Helmholtz himself states the principle right: 'Colours that appear the same produce [i.e., with some third colour] mixtures that appear the same' ('gleich aussehende Farben gemischt gleich aussehende Mischfarben geben', Helmholtz 1867, 283, my transl.). Unfortunately, the published translation has: '*Colours that look alike produce a mixture that looks like them*' (Helmholtz 1924, II.133)—which is not what Grassmann or Helmholtz either means or says, and which is a much weaker principle that would not serve the purposes either author has for it. The translation (for which, shockingly, the Harvard psychologist L. T. Troland seems (with E. J. Wall) to have had responsibility (Helmholtz 1924 I p. vi)) makes the same mistake a second time (1924, II.148), thereby making nonsense of an important argument in Helmholtz (discussed below: see note at the start of §3).

²⁰ Given R, G and B values within a system like W. D. Wright's (or our earlier Maxwell triangle), we may define X, Y and Z values by a linear transformation like this:

$$\begin{pmatrix} X \\ Y \\ Z \end{pmatrix} = k \begin{pmatrix} 2.6495 & 0.3120 & 0.3754 \\ 1.0000 & 1.6248 & 0.0796 \\ 0.0000 & 0.0796 & 2.1546 \end{pmatrix} \begin{pmatrix} R_{650} \\ G_{530} \\ B_{460} \end{pmatrix}$$

The system is set up so the Y-value of a stimulus is equal to a measure of its *luminance* (the psychophysical correlate for brightness), a function for which (for spectral stimuli) had been previously defined by the CIE in 1924.

²¹ x , y , z are defined as the *proportions* of X, Y and Z in a mixture: $x = X / (X + Y + Z)$; $y = Y / (X + Y + Z)$; and $z = Z / (X + Y + Z)$. Since $x + y + z = 1$, we only need directly to represent x and y in a diagram: that fixes z as well.

²² In addition: The spectral locus above about 630 nm (which is a straight line, corresponding to the fact that the S cones seem inactive in that range) is set in the x , y diagram to coincide with the line at -45° from (0, 1) to (1, 0).

²³ *Correlated colour temperature*: At successively higher temperatures, a black body glows reddish-orange (e.g. at about 1300-3500 K), yellow (~4000-5500 K), white (~6000 K) and finally blue (7000 K and above). (A black body is

defined as one that absorbs all the radiation incident upon it (unlike, e.g., a grey body, which reflects some of that radiation.) Natural light sources (even if not strictly ‘black’) often emit a similar energy distribution to that of a black body and their character may be specified by giving their *correlated colour temperature*, i.e. the temperature of a black body that is the closest fit to their own energy distribution, as, e.g.: candles (~1800 K), 60W incandescent bulb (2800 K), Sun disk at 60° (5100 K), Sky without sun (7500 K), Clear blue sky (15000-30000 K). (Evans 1948, 26-27; Wyszecki & Stiles 1982, 28 & 8-9.)

²⁴ Similar demonstrations had been presented by Ralph Evans at the Optical Society of America in March 1943 (Walls 1960, 32), and similar processes seem to be involved in the limited, but at their best remarkable, successes of two-colour movie film systems of the Technicolor Corporation (1917, 1922 and 1927) and, later, Cinecolor, which continued into the 1950s.

²⁵ It seems to me very likely that the success of Land’s images comes precisely from their involving ordinary objects—with expanses of more or less uniform surface colour presented as illuminated differently in different parts of the image, and with highlights and shadows. Indeed it may turn out to be somewhat ironic, that after presenting his remarkable red-and-white experiments, Land looked, in later work, to Mondrians to illustrate the fundamental principles of colour constancy at work in his earlier phenomena. It may be precisely the opposite: that the colours of Mondrians, if photographed and displayed in a red-and-white presentation, would actually produce a far narrower range of colour-impressions than Land’s original red-and-white demonstrations did. It may be that it is precisely because the original demonstrations were of more ‘natural’ three-dimensional scenes that the colours in them were (to the extent that they were) recognizable. But that is a hypothesis that needs to be tested.

²⁶ Rods have been given a significant role in perception particularly of the red part of the spectrum, at over 580 nm, and especially around 645 nm—which has occasioned revisions of the colour-matching functions by Stiles & Burch 1959. See Wyszecki & Stiles 1982, 333-43; 354-71.

²⁷ I should perhaps point out immediately the invalidity of the move from (i) to (ii) here: even if lights *a* and *b* on a single confusion line are indistinguishable when (for example) *a* is increased in intensity so as to appear as bright as *b* does, that does not imply that, in advance of any such adjustment of intensities, *a* will look of the *same hue* (or saturation) as *b*: *a* might, for example, (for all that is implied by the chromaticity diagrams) look *red* and *b* look *yellow*—though if *a* was increased in intensity, it would come to look yellow like *b*. For a report of something like this actually happening (in a case of acquired unilateral tritanopia), see Alpern, Kitahara & Krantz 1983b, 688-9. We are told that Violet₄₂₂ and Green₅₂₃, for example, though they lie on the same tritan confusion line, looked to their subject’s tritan eye of *different* hues: Violet₄₂₂ looked a fairly saturated Blue₄₈₃, whereas Green₅₂₃ looked a more desaturated variant of Blue₄₈₉, which is greener in hue. (I read these figures, rather approximately, from Fig. 2 at 688.) The authors attribute the hue-difference merely to the fact that (in their experimental conditions) the Violet₄₂₂ was less bright than the Green₅₂₃. Wachtler et al. (2004) effectively maintain (1)(a) without (b): they propose a model, on which the experienced colour-space of dichromats collapses to a plane, but it is not a vertical slice, nor a flat surface slicing through at an angle, but a curved surface that bends through the normal trichromat’s colour-space—thus yielding

different hue-sensations from physically similar stimuli at different intensities. MacLeod and Lennie (1976) propose a version of (1)(a) and (b) without (2): they believe the sensations from their subject's dichromatic eye are restricted to a line in chromaticity space, but it is an arc that curves through several hue-regions (see end of §4 below). MacLeod, this volume, explores this option further, in his section 5.

²⁸ Helmholtz and Maxwell of course hold (2) in a different form: the single axis of sensation runs in the case of the 'red-blind' between green and violet or blue (and, in the case of the 'green-blind', between red and violet or blue), so it will pass through several hue regions, all at virtually maximum high saturation. (By contrast, Hering's line between yellow and blue via white might be said to involve no change of hue at all, but only positive and negative saturations of a single hue.) It might be more precise to talk of a single 'hue-and-saturation' axis, but I shall talk of a single 'colour-axis' (having in mind the aspects of colour other than brightness), hoping that the flexibility in ordinary talk of colour makes this accurate enough as well as less cumbersome.

²⁹ (For reasons given two footnotes back) Alpern, Kitahara & Krantz 1983b effectively reject (1b) and (2) for their tritanopic subject, but still insist on (1a); Wachtler et al. (2004) model a similar position; and Macleod holds (1)(a) and (b) without (2).

³⁰ Helmholtz 1867, 295; 1896, 362; 1924, II.148. (The 2nd German edition is the one to read: it contains some improvements over the 1st in details of the argument, but they are omitted in the 3rd edn; and the English translation (which is done from the 3rd edition) also contains a complete misunderstanding of Grassmann's 3rd Law (see n. 19 above). For those who read French but not German, the French translation (from the first German edition) is much better than the English.) The core of the argument is that, if red and green lights R and G are indistinguishable for colour-blind person *p*, then (by Grassmann's 3rd Law) R and G, and any intermediate shade produced by a mixture of them, will all have the same mixing potential for *p*: they can all be substituted one for another in any mixture with further kinds of light, and the change will be indiscernible to *p*. But one of the intermediate shades produced by mixture of R and G will in fact be equivalent to some shade that could be produced solely from yellow and blue (it will effectively be neutral in red-greenness): let us call it S. In that case, all colours that are normally produced by R, G and blue, can instead (for *p*) be produced *from yellow and blue alone*. (We simply substitute S—which is producible from yellow and blue alone—for whatever R and G constituents would normally be involved.) And the same is true (by parallel reasoning) of colours normally produced by red, green and yellow. In short, all colours whatever can for *p* be produced (or matched) by mixtures of *merely yellow and blue*. (Strictly, note, this doesn't immediately tell us that they all *look* merely yellow and blue to *p*; that is a conclusion requiring further evidence, drawn, typically, from the unilateral colour blindness cases.) To be very brief on what goes wrong here: we know from anomalous trichromacy that there are cases where a red and a green R and G are confused in some particular circumstances but not in all, and that even the regular confusing of them is not sufficient for us to conclude that R and G will have identical mixing potential (especially if negative quantities, or proportions > 1, of either might be involved). In general, we know from anomalous trichromats that confusions can occur without the whole chromaticity space collapsing to a line. And something like this may be true for dichromats as well as anomalous trichromats. For example, R and G may be

confused in small fields but not large, or at low brightness but not high brightness, or (to speak approximately) when relatively desaturated but not otherwise—and if we set the standard of indistinguishability as, not indistinguishability in *some* circumstances, but indistinguishability in *all* (even with large fields, high saturation, etc.), then the empirical evidence today suggests that this standard is one that, with the dichromats of the real world, real reds and greens mostly do not actually meet.

³¹ Wright himself said that brightness of his colorimeter fields was ‘more limited than could have been wished’ (Wright 1946, 50).

³² Wyszecki & Stiles 1982, 464; though I should mention that the neutral points of the protanopes in Scheibner & Boynton (1968) imply a protan confusion point at $x = 1.03$, $y = -0.03$, or even (if we include one ‘discrepant’ subject) $x = 1.28$, $y = -0.28$. This is theoretically absurd, in that it would place the ‘red’ receptor in a part of the CIE diagram that corresponds not to red, but to a red of negative luminance, i.e. a *green-blue*! (One of the details of the CIE system is that line $y = Y = 0$ represents zero luminance, and things with Y -values less than zero would have negative luminance.)

³³ Where authors have given values in other systems (e.g. the W. D. Wright system or the CIE 1976 u' , v' system), I have converted the figures to the CIE 1931 x , y system and given the original figures in parentheses. In some cases I have not seen the original sources but have followed an authoritative intermediate source: obviously this puts me in no position to judge the accuracy of the figures or the methods employed, but it is still, I hope, of some help in showing the variety of conflicting voices around. For two other surveys of many very different reports of confusion points, see von Schelling 1960 and Nimeroff 1970.

³⁴ The first three lines in this Table derive from the fact that in his original 1935 study, Pitt gives no specification for the confusion points at all; in his 1944 paper, he gives coordinates ($r = 9.5$, $g = -8.5$), but they agree neither with his diagram nor with the data it was based on; what is more, having admitted that the lines actually converge (at a point he incorrectly identifies) Pitt goes on to propose that we should *treat* the confusion lines nonetheless as being parallel in the coordinate system of his diagram. Pitt is motivated here by his conviction that in deuteranopia ‘two of the fundamental sensations are identical’ or have *fused* (1944, 106)—a view that had supporters at the time but has practically none today. Pitt seems unaware that there is nothing particularly special about the confusion lines being parallel in the particular coordinate system he is using: lines parallel in one coordinate system will be non-parallel in another system, and it makes no sense to suppose that a convergence of lines (in one fairly accidentally-chosen system) indicates the ‘loss’ of a primary sensation whereas parallelism (in that system) indicates ‘fusion’ of two sensations instead. Quite aside from the empirical disagreements between Pitt and other researchers, Pitt’s readiness to abandon his own evidence, and the theoretical confusion with which he does so, are hardly confidence-inspiring.

³⁵ These figures are utterly unbelievable: the deuteranope confusion point would be a purple (rather than some kind of green, which would surely, if anything, be needed, if it is to correspond to the receptivity of the missing M-cone type).

³⁶ Farnsworth puts the variation down to variations in macular pigmentation. There is good logic in this, but it opens up a multitude of further problems. If we suppose (following Pitt 1935) that the neutral point (matched with illuminant B) is ~500 nm for deuteranopes with average macular pigmentation, then at a rough estimate, we might expect a

deuteranope with only *half* the normal density of macular pigmentation to have a neutral point (matching that same illuminant B) at ~496 nm, and one with *one-and-a-half times* the normal density to have a neutral point at ~504.5 nm. (My own calculation from (the square root of) the Wyszecki & Stiles function for macular transmittance (1982, 721), and the profile of illuminant B (1982, 759).) And on one standard method of deriving a confusion point—by joining, in a 1931 CIE chromaticity diagram, the neutral point and Illuminant B, and seeing where that line crosses the line at -45° that joins the spectral greens, yellows and reds which dichromats confuse in an anomaloscope—, that would give deuteranope confusion points ranging from about $x = 0.743$ $y = 0.257$ to $x = 2.283$, $y = -1.283$! It should be obvious that variation in the macula can wreak havoc with any attempt to get a single standard deutan confusion point. However, this is only the beginning of our problems: for, on similar principles, we would expect a similar variation in macular pigment density (ranging from $1/2$ to $1\ 1/2$ times the average) to shift the protanope neutral point (matching Illuminant B) between about 491.4 nm and 500 nm—which would imply a confusion point shifting between $x = 0.626$ $y = 0.374$ and $x = 1.014$, $y = -0.014$. Both those extremes are absurd. (The first is close to the position of Orange₆₀₀, which is certainly not invisible to protanopes with low macular pigmentation, and the second is below the line $y = Y = 0$, so would represent a red of negative luminance, i.e. a *blue-green*, which, again, cannot be the ‘missing primary’ of a protanope with high macular pigmentation.) What this shows is that the shape and proportions of the spectral locus in a 1931 CIE diagram cannot be taken to fit the colour matching of people with even moderate deviations from average macular pigmentation, and that the line at -45° ($x + y = 1$) cannot be taken as a dependable confusion line for them either. We have a lot more work to do.

³⁷ Nuberg & Yustova’s figures—derived using just one confusion line and a method of Maxwell’s—are described by Scheibner & Boynton (1968, 1157) as ‘probably one of the most accurate determinations’.

³⁸ These are not experimental studies: Vos 1978, Walraven 1974 and Smith & Pokorny 1972 all build upon Vos & Walraven 1971, which is a derivation of König receptor primaries, based among other things on the authors’ choice of $x = 1.40$, $y = -0.40$ as the deutan confusion point, itself based largely on Nimeroff 1970. Nimeroff’s article is actually a survey: it arrives at a weighted mean of the various deuteranope confusion points reported by investigators (principally Pitt for 8 subjects and Nuberg & Yustova for 12). (Nimeroff’s weighted mean was, incidentally, $x = 1.53$, $y = -0.53$, not $x = 1.40$, $y = -0.40$: Vos & Walraven evidently take it that a certain degree of indeterminacy attaches to Nimeroff’s figures, so they are at liberty to *choose* a figure within the range $x = 1.53 \pm 0.17$, $y = -(0.53 \pm 0.17)$, and they give some complex reasons for their particular choice (802, 811).) Nimeroff 1970 himself, incidentally, presents $x = 1.40$, $y = -0.40$ as his own (1969) reevaluation of the confusion point implied by Pitt’s data—on the basis of treating the confusion lines in Pitt’s deuteranope diagram as being *parallel*—which is simply incorrect, as anyone knows who gets a photocopier, reduces Pitt’s diagram to 25% size and extends the lines to see where they meet. It should be obvious that none of this constitutes a basis for anything but the most approximate view.

³⁹ More perplexingly, Judd had previously suggested $x = 1.000$, $y = 0.000$ (1943, 305)—and repeated the suggestion at (1952, 105)—which makes little sense, in that it would imply (in placing the confusion point on the line $y = Y = 0$) that the M-cones made no contribution to luminance.

⁴⁰ Dean Farnsworth takes up the issue of confusion points in the opening article of the opening issue of *Vision Research* (1961), the journal which he founded but never lived to see published. Farnsworth points out the huge spread among Pitt's six deuteranopes in the spectral light they match with white (Illuminant B) (which in turn implies huge differences in the confusion points each one of them had, if he had one at all; only in fact their confusions don't imply a single point). He points out how 'the six real existing individuals with apparently impossible responses were averaged into one theoretical individual with reasonable responses' (1961, 3); thus was born the canonical deuteranope of Pitt's famous confusion diagrams. The spread is not mere experimental error; it may best be explained, Farnsworth believes, by individual variations in ocular pigmentation, e.g. in the macula; such individual differences mean that, even if the receptors of (for example) all human deuteranopes were the same, we should not expect their neutral points to be the same. (Any white will have its hue modified according to the pigmentation in the eye even if a spectral hue will not.) Even more troublingly, Farnsworth adds that, given ocular pigmentation, we should not even expect confusion lines to be *straight* (given the varying results to be expected when a filter interacts with light of one hue but at a series of graded levels of saturation). Joining points corresponding to pairs of lights confused by a non-averaged dichromat, *we will not get a set of lines that intersect at a point*. 'We can make sense of this individual's data only if we assume that his confusion line with white is curved through the two points.' (1961, 5) Farnsworth's conclusion is that, if we want to use such diagrams to discover the 'missing sensation' of the dichromat, then what we really need is 'a color-mixture diagram at the *retinal* level. The immediately apparent way to approach this is to devise means of measuring the quality and variety and *range* of pigmentation in normals and in color defectives.' (5, my emphasis) In the absence of that, we can only expect Pitt's 'enticingly simple diagrams' to continue to be the object of 'uncritical acceptance and a consequent remarkable number of misleading beliefs' (1). I enthusiastically agree.

⁴¹ 'A review of the rather considerable literature ... shows that the color perceptions of both protanopic and deuteranopic observers are confined to two hues, yellow and blue, closely like those perceived under usual conditions in the spectrum at 575 and 470 m μ .' (Judd 1948, 247, quoted in §1)

⁴² Helmholtz (1867, 298; 1924, ii.151-2) interestingly, thinks a 'red-blind' person will *talk* of seeing only 'yellow' and 'blue' in the spectrum, but his sensations will actually be of *green* and violet or blue. Maxwell takes the same view.

⁴³ As far as I can see, Judd believes he can stop Hayes's report from being evidence against his own view, by downgrading the subject's deficiency: Hayes calls it Protanopia (1911, 397), but Judd reclassifies it as Protanomaly (1948, 253)—because of the subject's seeing certain mixtures on a spinning top as 'much too green'. But we should be wary of assuming that anyone who 'sees green' (or 'sees red') cannot be a dichromat, when the very matter at issue is whether dichromats are or are not capable of seeing red or green: it would be a case of reasoning in a circle. Of course, a protanope will surely not reject a brightness-adjusted Rayleigh match in a Nagel anomaloscope as 'too green' (compared with the Y₅₈₉)—indeed this is the standard criterion for distinguishing a protanope from a protanomalous person. But the case with spinning tops—with surface colours and larger fields—may well be different. And when the lightness of the two fields in an anomaloscope is not the same, then even certified dichromats sometimes describe the darker field as 'greener' than the other—the criterion for dichromacy is not that no greenness is ever reported, but that

it may be cancelled by adjustments of lightness. In the end, I too have doubts about whether Hayes's subject G. S. was or was not a protanope; but if she was, then (because we are told she had sensations of green) she is at least a putative counter-example to Judd's only-yellow-and-blue claim, and certainly no support; and if she was not, then she falls into my category (c) (of anomalous trichromats) and again, though for different reasons, is no help to Judd.

⁴⁴ I have replaced Judd's 'do' (i.e. 'ditto') with whatever it refers back to; and I have listed author and date together in col. 1; I have replaced 'm μ ' (and the inaccurate ' μ ') with 'nm'. The four categories (**a**, **b**, **c** and **d**) and the ordering of the various rows under them are my own: Judd simply lists the ten reports in chronological order. Judd's reports under the column for 'Author's own' classification of the defects are in four cases inaccurate, in that Hering and his followers Hippel and Hess wholly rejected the concepts of protanopia and deuteranopia (or their ancestors 'red-' and 'green-blindness') in favour instead of the general notion of 'red-green blindness' (with shortening of the spectrum at the red end, if admitted at all, put down to the media of the eye). Hering's own description of his subject's diseased eye is as 'almost red-green-blind' (1890, 15); Hess describes his own subject as having a 'red-green sense' that has 'almost completely disappeared' (1890, 35). What is more, Hering diagnoses his patient as suffering from an atrophy of the optic nerve, and the Hess case is similar—so neither case is likely to have been fundamentally very similar to deuteranopia.

⁴⁵ Once we recognize the possibility of systems developing to compensate in part for colour blindness (whether or not we will decide that the possibility is actually realized), then, *a priori*, we might expect acquired colour-blindness (especially in just one eye) to be less debilitating than congenital colour-blindness (in that an acquirer at least has had some period of normal trichromatic colour-input, which might help in guiding or calibrating any compensation system); alternatively, we might expect an acquired colour-blindness to be more debilitating (if some forms of compensation depend on neural changes that can occur early in neural development, but not later). Without direct empirical knowledge of *both* kinds of case, there is no obvious way to say how far the one may or may not be a guide to the other.

⁴⁶ This fact makes me wonder if neurological disease outside the eye may not have been the cause of both conditions—in which case the colour-blindness really belongs in category (d) and is of even less evidential value to us. Certainly nothing in Hippel and Holmgren gives any information on the exact cause. It should be said that even with the most recent case of supposedly congenital unilateral colour-blindness (MacLeod & Lennie 1976), we do not have independent information on the genetics or (e.g. from reflectance densitometry) the actual cone equipment of the retina.

⁴⁷ Colours 1-30 in Radde's Scale form a hue circle with the following main reference points: 1. Cinnabar (i.e. slightly orange red) 4. Orange 7. Yellow 10. Yellow-Green 13. Grass Green 16. Blue-Green 19. Blue 22. Violet 25. Purple 28. Carmine (i.e. deep red, inclining perhaps to purple). (I am grateful to Rainer Hertel for letting me see a copy from the Staats- und Universitäts-Bibliothek Hamburg.) They were described by Hippel's subject (when presented individually and out of sequence) as: 1-15: yellow, 15-16: greenish or yellowish, 17-22: blue, 23-24: light blue, 25-30: red. (Hippel 1880, 181; sample 15 is indeed double-counted by Hippel).

⁴⁸ Judd talks only of ‘Sloan 1947’, apparently forgetting the co-author Lorraine Wollach. The 1947 publication is an abstract: the main report (1948) appeared only later, and Judd does not mention it; but he no doubt knew plenty about the experimentation, since he is thanked for having advised the authors on it (Sloan & Wollach 1948, 509).

⁴⁹ They were of Value and Chroma 5/5. The maximum for Value in the Munsell system is 10/ (for White). There is no general maximum for Chroma, and different ranges of Chroma will be practically realizable depending on the Hue (and Value) in question; but in many cases, Munsell papers are available at Chromas up to /10, and in some cases /12 or /14.

⁵⁰ One might compare the well-known weaknesses of the CIE Standard Source C, which combines a tungsten filament lamp (2856 K) with a deep blue filter to yield light of correlated colour temperature 6774 K, which supposedly matches daylight: ‘The illuminance levels that normally can be achieved with such sources [as Standard Sources B and C] are relatively low and confined to small areas of illumination, making them *unsuitable as sources for visual inspection*.

Their relative spectral radiant power distributions, closely correspondent to [the mathematically specified functions that define] CIE standard illuminants B and C, *do not represent adequately* spectral distributions of *daylight ...*’ (Wyszecki & Stiles 1982, 147, my emphasis). If these kinds of illuminant are held unsatisfactory for industrial colour evaluation and matching by normal trichromats, then it is puzzling that they should be used in an investigation that pretends to tell us the *full range* of colour sensations available to a dichromat eye.

⁵¹ Except as part of the compound word ‘blue-green’ (Blaugrün) in two places (97), one of which says something which contradicts both what Judd reports from the article and what Judd himself wants to conclude. Nagel says that he, a dichromat, finds ‘a match between blue-green and purple *always unsatisfactory*, as soon as the size of the field exceeds 10°’ (1905, 97, my emphasis). That is: with large fields of blue-green and purple, Nagel does *not* (as Judd would like) see merely the blue component—and he has found ‘numerous other deuteranopes’ with very similar behaviour (97).

⁵² When presented with a circular 3°-4° field one half of which showed R₆₈₀ light and the other half YG₅₅₀, Sch. saw them both as yellow (as indeed Judd would like) as long as the two half-fields were of equal lightness. However: ‘If the one half was darker than the other, then Sch. mostly *said the halves were differently coloured*, sometimes the darker one green, the lighter one yellow or red, sometimes the darker one red, the lighter one yellow’ (1905, 94, my emphasis).

⁵³ ‘The probability of such contradiction arising may be estimated from the fact that the eight [unilateral or similar] cases recorded so far ... fail to contradict it’ (Judd 1948, 255). Judd talks also of how ‘a reasonable search of the literature has failed to uncover any reliable evidence against this indication’ (255) (the ‘indication’ here being, strictly, a slightly weaker claim, that red-green confusers see yellow and blue, without it being specified exactly which yellow and blue they are).

⁵⁴ He was President of the Optical Society of America in 1953, and recipient of many awards from technical and engineering societies. For some biography, see Judd 1979, iii-vii.

⁵⁵ It is puzzling that Judd cites Goldschmidt 1919 at all: the case is not of unilateral colour-blindness but of a unilateral scotoma with peripheral loss of acuity, following a gunshot wound to the left forehead and temple: the colour blindness

(an anomalous trichromacy ‘with red-weakness’) showed up with ‘similar results’ in both eyes (199). (Judd may be hoping that the exacerbation of the protanomaly when combined with the effects of the gunshot wound will tell us what a more severe colour-blindness—protanopia—would involve. But the causal factors in the two cases are so fundamentally different that it is anyone’s guess what inferences one might be able to make from the one kind of case to the other.) Yet more oddly, the one relevant passage that Judd quotes from that article seems, if anything, to imply that the subject saw *green*—whereas Judd reads it as saying almost the opposite of that. Goldschmidt reports: ‘... H. had the habit of fairly regularly naming a number of colour hues incorrectly, especially yellow and violet ones, in particular, *as if the green in the spectrum seemed spread out into the yellow*, and the blue into the violet ...’ (1919, 205, my emphasis). What that surely says is: the man talked as if certain yellow parts of the spectrum *looked green to him*, and certain violet parts looked blue. Judd quotes part of the passage, but reads it as implying (by a line of inference that I can only guess at) that the dominant impression from the long-wave part of the spectrum was actually ‘orange-yellow’ (Judd 1948, 252), the latter being a disappointment to Judd, since he’d rather that the dominant impression there had been of unique yellow.

⁵⁶ ‘Hayes’ conclusions are sound if translated into accepted terminology.’ (Judd 1948, 253) This is an extraordinary statement. Hayes’s ‘main contention’ is ‘that people properly classed as color-blinds have some sensations of red and green’ (1911, 399)—and there is no sense in which that claim can be ‘translated’ into agreement with Judd’s own view other than by ignoring what it actually says. What Judd no doubt has in mind is that he can undercut Hayes’s opposition by downgrading the diagnosis of the bad eye in Hayes’s subject from protanopia to protanomaly (in which case it would not be surprising that it provided sensations of green): but while that might at a pinch be called a change in terminology, it too is actually, and would be understood by all parties as, a substantive change of diagnosis, not a purely verbal change.

⁵⁷ In his first article, Hippel reports his patient as naming the colours of Radde’s Index, when presented separately, with a variety of colour terms including ‘greenish’, ‘red’, and ‘light green’, as well as ‘yellow’ and ‘blue’ (Hippel 1880, 181). A year later, Hippel takes it all back: ‘I must add a modification to the reports made by the patient in my first communication, in so far as they relate to the naming of the individual colours in the Index to Radde’s Plates. Since this person had previously never attempted to compare the colour sensation of his right eye with that of the normal left eye, he was therefore, at the time of the first experiments with the right eye alone, precisely *in the same situation as a bilateral colour-blind person*. As the latter employs the expressions Red, Yellow, and Green, which he is familiar with, *in complete confusion*, so also did my patient at first. Hence [sic] the names that he applied to the colours in the first trials give us not the slightest information on the sensations he had of them. This changed very soon with him, *once he became aware of his defective sense of colour* in the right eye and began then to compare the sensations of the two eyes with each other. *The terms that he now chooses* for the individual colours of Radde’s Scale *correspond completely to the sensation he has*; they are as follows: 1-14 yellow, 15 grey, 16 & 17 dark grey, 18 slightly blue, 19-21 blue, 22 & 23 dark grey, 24 blackish, 25-27 dark grey, 28-30 yellow-grey, 31 grey, 32 darker grey, 33-36 yellow, 37 grey-yellow, 38 dark grey, 39 light blue, 40-42 blue-grey.’ (Hippel 1881, 51-52, my emphasis) What Hippel curiously seems to

forget is that his patient had *not* used the expressions Red, Yellow and Green ‘in complete confusion’: on the contrary, he had used them with considerable (though imperfect) sensitivity to the red-green variations among things. And it is bizarre that Hippel thinks his patient knew how to apply the colour terms perfectly correctly in relation to experiences caused by his left eye, but somehow not in relation to experiences caused by his right eye (until, that is, he had been instructed in the special process of the ‘comparison’ of sensations). One can indeed misreport impressions. But the reasons Hippel gives for thinking his patient was doing so are bad ones, and I shudder at the thought of this 17-year old being retrained by his distinguished doctor (we are not told how) until he properly confined himself to the approved vocabulary of yellow, grey and blue.

⁵⁸ Graham & Hsia mention some significant differences (1958, 47)—and MacLeod & Lennie (1976, 691-2) point out more. To match 500 nm light, so they report, Pitt’s deuteranopes needed a 1:15 mixture of B₄₅₀ and R₆₅₀, but the Graham & Hsia subject seems to have needed a 5:1 mixture—i.e., some seventy-five times more blue than an ordinary deuteranope! And the reported neutral point (502 nm) is completely incompatible with what Graham et al. publish as the subject’s colour-mixing functions.

⁵⁹ Scanning the spectrum from 700 to 400 nm the subject’s experience pretty much divides into three: a region varying in hue, with red, orange and some yellow all represented (and all at fairly high saturation); a zone of yellowish hue and fast diminishing saturation, reaching a neutral point (around 567 nm), and no sensation of green at all; and finally a blue region (with no violet) where there is only a small amount of variation in hue sensation (between sensations corresponding to 470 and 490 nm), but the saturation first increases sharply to a maximum (with stimuli in the ~488-482 nm region) and then decreases to a second near-neutral point as the stimuli approach 400 nm.

⁶⁰ See footnote in §2.3 above. It cries out to be studied further, how *lightness* variation in a stimulus may also change the *hue* experienced by dichromats (such changes being already quite interesting enough, though no doubt largely different in nature, in the case of normal trichromats: see the curved lines of constant hue in e.g. Wyszecki & Stiles 1982, 420-424, 670-672).

⁶¹ Compare the comments on the many different factors at work in depth perception, at the start of §6 below.

⁶² I am grateful to John Mollon for the observation. Unsurprisingly, such sufferers tend to be more distinguished as bowlers than batsmen; and sometimes particularly as captains. Others cricketers who had lost the sight of one eye are William Clarke (1798-1856), first-class underarm spin-bowler, who played for Nottingham, the MCC and his own All-England Eleven, and Eiulf Peter Nupen (1902-1977) who has been described as a deadly bowler on the matting pitches on which South African cricket was played in his time. Baseball seems to have been slightly less forgiving of this injury, but Thomas Jacob Sunkel was a major league pitcher—for the Cardinals in 1939 and later the New York Giants and Brooklyn Dodgers—despite a cataract that had left him effectively without sight in one eye.

⁶³ R. Descartes, *La Dioptrique* (1637), Discours VI. (AT VI.130-147, translated in CSM i.167-175); G. Berkeley, *An Essay towards a New Theory of Vision* (1709), §§1-51; Wheatstone 1838; for a historical treatment of these and many other contributions on the issue, see Wade 1998, Ch. 6. For a person using only one eye to recognize the shapes of solid things, ‘The motion of the head is the principal means he employs’ (Wheatstone 1838, 380)—though Wheatstone (I

think unfortunately) takes it that while that supplies ‘accurate information’ on depth, it does not supply ‘that vivid effect arising from the binocular vision of near objects’

⁶⁴ As Katz said, ‘The objects, with their colour qualities, are apprehended as belonging in a *chromatically illuminated* field.’ (1935 [1930], 190)—a point beautifully illustrated by the demonstrations of Lotto & Purves, e.g. 2002. See also the discussion of the blue card disk seen in a yellow spotlight, in §7 below.

⁶⁵ On the other hand, one should mention that normal trichromats do more or less equally well with illuminant colour variation in the direction of Green as they do with variation along the YB axis—though they do slightly less well with variation in the direction of Red (see Delahunt & Brainard 2004, and refs.); whether this is true also of dichromats is something that I do not know has been investigated.

⁶⁶ I give the pigments (both green and red) in order of increasing yellowness (i.e. from bottom left to top right in the chromaticity diagram), so that the respective members of each group of three correspond: corresponding members fall more or less on a traditional confusion line.

⁶⁷ The perspective is as if looking down from a height, directly above the point $x = 0.315$, $y = 0.13$, and the lines of the ‘skyscrapers’ all converge on a point (in line with this point, and behind the paper, so to speak) which can be taken to represent $X = Y = Z = 0$: the diagram can therefore be thought of as a 2D drawing of a 3D representation of the CIE XYZ tristimulus space. For any line coming out of the floor, the distance (in the 2D drawing) from the origin (‘behind’ $x = 0.315$, $y = 0.13$) to the point where the line emerges from the ‘floor’ represents a ‘floor’ luminance of 15 (where, for comparison, Lead White has a luminance of ~369 and Ultramarine ~16): and ‘heights’ above the floor along that line are represented proportionately. The metric for heights is therefore strictly not the same as in a perspective representation.

⁶⁸ For our model, I use the hypothesized König fundamentals, dichromat colour matching functions, and luminous efficiency functions, V_p and V_d , devised by Wyszecki & Stiles 1982, 463-71, 604-8. I have doubts about the confusion points derived from Pitt 1935, and these functions will be subject to similar reservations; but they are quite good enough, I think, to illustrate the general principles in the circumstances.

⁶⁹ The same point is valuably employed in Golz and MacLeod 2002 on colour constancy.

⁷⁰ If a series of points is scattered within a 1-unit radius circle centered on (0, 0), then if a point has an x -value of 0.2 or 0.4, then it may have any of a very wide range of y -values; but if it has an x -value of 0.95 or 0.97 then it must have a y -value much closer to zero.

⁷¹ If we keep S'' equal to S' , then we get an improved recovery if we set them equal to -23.085%, but that lowers the mean discrepancy between recovered and original coordinates (for our 15 coloured pigments) only from 0.01860 to 0.01854. There is, I think, no special reason why the *Luminance increase per unit of RG-ness* should be the same as *Luminance increase per unit of YB-ness* in CIE x , y chromaticity space (that is, why S'' should be the same as S'). (Lengths that are equal in one colour system may not be equal when transformed into another system, and most theorists believe that, for example, the CIE 1976 u' , v' chromaticity space has a metric that fits better with the perceived distances and differences among colours than does the 1931 x , y space.) But (given the x , y space and the

general structure of the algorithm) it turns out to be not far from the best way to minimize the discrepancies between the recovered coordinates and original coordinates. If we allow S'' to differ from S' , then if we set S' to -21.7% and S'' to -25.5%, we can reduce the mean discrepancy to 0.01818. But that too is only a small improvement.

⁷² We know perfectly well, for example, that two metameric greens that under illuminant D_{55} belong in the same position in x, y chromaticity space will behave slightly differently as the light changes—and may look different from each other in D_{75} . But in that case any algorithm of the kind I am envisaging here would be almost bound to end up ‘recovering’ slightly different chromaticities for the two things in D_{55} —which would, *ex hypothesi*, be false. But still, the differences in behaviour between those metameric greens would pale in comparison with the differences in behaviour between either of them and red things—and that is the kind of difference I am working on here, and somewhat more subtle varieties of it.

⁷³ There are rare circumstances where the macula has a noticeable effect. Maxwell describes one, suggested to him by the physicist George Stokes: ‘It consists in looking at a white surface, such as that of a white cloud, through a solution of chloride of chromium made so weak that it appears of a bluish-green colour. If the observer directs his attention to what he sees before him *before his eyes have got accustomed* to the new tone of colour, he sees a pinkish spot like a wafer [i.e. an orange-red crisp cake, baked between wafer-irons, typically pink or orange-red] on a bluish-green ground; and this spot is always at the place he is looking at. The solution transmits the red end of the spectrum, and also a portion of bluish-green light near the line F [i.e. about 486 nm]. The latter portion is partially absorbed by the spot, so that the red light has the preponderance.’ (Maxwell 1870, 230, my emphasis. von Kries makes the same point in Helmholtz 1924, 405.) For the effects of this $\sim 4^\circ$ ‘Maxwell spot’ in viewing of large and even fields, see Wyszecki & Stiles 1982, 133.

⁷⁴ When first writing this, I had never heard anyone else talk of it—except for one philosopher, who had heard me describe it and had replied enthusiastically that the description fitted his own experience too. Since then, one other person has reported the same to me (when I talked at a symposium at the Laboratory of Integrated Neuroscience at UIC in 2007). The New Zealand artist Will Furneaux can be found (in a post at www.conceptart.org dated November 20th, 2008), saying: ‘Sometimes ... (I think because it's your brain that interprets signals from your eyes) I actually "see" red when in actual fact it's green, and then *when I realise it turns green*.’ (my emphasis) It seems that the phenomenon may be quite common, while being unrecognized in the academic literature.

⁷⁵ Dalton and Nagel were deuteranopes (see Hunt et al., 1995 on Dalton), Koffka was protanomalous, Pole was a protanope. Friedrich Schumann counts as deuteranomalous by the anomaloscope, but describes himself as lacking any ‘green process’ in the cortical centre for light sensations (1904, 12-13). Pole (1859) is an extraordinary case, a convert, one might say, and proselitizer for the ‘yellow-and-blue’ view that was just at the time becoming established. A civil engineer by training, talking to the distinguished Fellows of the Royal Society (among whom is Sir John Herschel, a great promoter of the yellow-and-blue view, and to the company of whom Pole will two years later himself be elected Fellow), he declares that for thirty years he had *believed* he saw red and green along with other colours, but that he has now come to recognize that it was only various tones of yellow and blue that he had ever been perceiving (together

with a sensation of green, which he supposes to be the mixture of yellow and blue sensations, and which would be experienced on looking at white and other neutral things). With Pole's personal confession being apparently almost ideal support for Herschel, Herschel in a further paper of his own congratulates Pole—while also correcting some of his confusions—as having given 'the only clear and consecutive [i.e. consistent] account of that affection which has yet been given' by a colour-blind person who knows the theoretical accounts of it and is '*in a position to discuss his own case scientifically*' (Herschel 1859-60, 73, my emphasis). I must consider Pole's arguments on another occasion. (His evidence to 'prove' that his sensations are confined to yellow and blue and their mixture (1859, 328) comes only from the confusions he makes—which are certainly not conclusive on the matter.) But the extraordinary interplay of intellectual styles and social and academic precedence (and interdependence) here—as also, over the subsequent century, the influences of technological changes and public safety imperatives, especially in shipping and the railways—make clear how a serious assessment of the textual and intellectual evidence on this subject cannot be separated from the larger social and cultural history.

⁷⁶ I display strong protan deficiencies on the Farnsworth D-15 test; on the Oculus anomaloscope, I behave as protanomalous; genetic tests indicate the absence of a gene for L-receptors. I am grateful to John Mollon for the Farnsworth and anomaloscope tests and to Dr. Ulrike Dohrman (Institut für Humangenetik, Albert-Ludwigs-Universität, Freiburg) for the molecular-genetic analysis.

⁷⁷ It may be of interest to readers of Pylyshyn (1999) that this case—if I am not misdescribing it—seems a clear counter-example to his thesis of the 'cognitive impenetrability of visual perception': it seems to be *thought* or *cognition* that influences the second colour-shift (when I have returned to the earlier lighting environment, and then, with some effort of will, get the thing to 'flip' back from green to red). Pylyshyn argues that in the Necker cube case, the shift in appearance is caused not by cognition directly, but only by cognition influencing which parts (and especially corners) of the cube image the viewer focusses attention on (Pylyshyn 1999 §6.4, following Kawabata 1986, Peterson & Gibson 1991: it may be worth mentioning that Pylyshyn's claim here was originally made by Necker himself (1832), and opposition to it voiced by Wheatstone (1838, 382)). It would be interesting if Pylyshyn or others were tempted to run a similar line for the colour case: arguing, perhaps, that to get the surface to look red, I would need to focus on one particular pattern of shading and highlights, and to get it to look green I would need to focus on some other pattern. I would be fascinated if there were support to be had in that manner for the kind of suggestions I have made in §6 above; but for the time being, the case seems a *prima facie* challenge to Pylyshyn's thesis.

⁷⁸ It may be significant that, in at least some such cases, there is a flipping between the thing's looking deep red and its looking deep green, with apparently no option of seeing the thing as (for example) merely a moderately saturated red, or, simply, greyish dark yellow. The situation seems to be of a 'bistable', rather than 'multistable' stimulus (where 'many' means >2)—perhaps like the surface depicted in a picture that can be seen as concave or as convex, but not—or not easily—simply as *flat*. I wonder what might be the cues here that, so to speak, force the projection to alternately quite distant points in colour space and not those in the middle.

⁷⁹ Louis Albert Necker discussed ambiguous 2-D representations of solids in the second part of his ‘Observations on some remarkable Optical Phænomena seen in Switzerland; and an Optical Phænomenon which occurs on viewing a Figure of a Crystal or geometrical Solid’ (*Philosophical Magazine*, 1832). Necker’s special interest arose directly from his work as mineralogist—the ‘sudden and involuntary change in the apparent position of a crystal or solid represented in an engraved figure’—and his example was actually of a rhomboid figure (336), which is in some ways more dramatic than the now standard example of a cube.

⁸⁰ Nor is just an accidental or personal matter. Light travelling from air or a vacuum into a dense medium is refracted differently according to its wavelength, as we see with a prism. The refractive index of the lens of the eye is greater for blue light than for red, and the focal length smaller. So when a green object is effectively in focus on the retina, a similarly clear image of a red thing at the same distance will be behind the retina—and to bring it into focus, one will have to focus as if the red thing were closer than it really is.

⁸¹ As Mr. T, one of George Wilson’s informants, tells him: ‘the individual berries ... appear to me for the first few seconds rather black than red, and *only gradually assume their red hue ...*’ (Wilson 1855, 30, my emphasis).

⁸² Cp. Katz (1935 [1930], 190), quoted above in note to §6.1.

⁸³ I have developed the arguments in my 1992, esp. sects. 3.1 & 3.3.

⁸⁴ Dalton thought that in the spectrum he saw only yellow and blue (and perhaps in some degree purple), but could see red and green surfaces under candlelight. Yet plenty of writers (Graham et al. 1961 are an example) move without any argument whatever from ‘*x* sees only two hues in the spectrum’ to ‘*x* sees only two hues’. See §5 above on Montag & Boynton 1987 and Montag 1994.

⁸⁵ Where we do continue with light stimuli, rather than surfaces, I would particularly recommend a study of the effects (e.g. on dichromats’ hue sensation) of variation in lightness of stimuli. Is it the case that red lights if brighter look ‘more yellow’ to the dichromat? (Always?) What are the conditions (of brightness, contrasting surround, or whatever else) under which dichromats take yellow light to be green? With a unilateral case, the way hue sensation may vary with stimulus brightness should be particularly carefully examined. Alpern et al. (1983b) show the importance of the issue (see end of §4 above), but do not study it *per se*.

⁸⁶ There have been recent ‘large-field’ experiments (see §5 above), but the spectral light fields are of up to 8° (Smith and Pokorny 1977) and 12° (Nagy & Boynton 1979, Nagy 1980)—which pales beside Nagel’s use of 20° and even 60°-85° (Nagel 1910, 6-7). The ‘large field’ surface samples of Montag & Boynton 1987 and Montag 1994 are in fact 3.8 cm squares—subtending an angle of only about 4° at 60 cm. By contrast, Nagel used (among other things) papers of 50 cm x 70 cm (1910, 8) -- some 240 times larger.

⁸⁷ By contrast, for his dichotomous B-20 test, Farnsworth proposes: ‘The saturation should not be high enough to permit the factor of *hue recognition* in color experienced applicants to interfere with apparent serial arrangement according to hue.’ (1943, 575, my emphasis). Farnsworth seems to suggest, incidentally, that *recognizing* the hues can get in the way of classifying them according to their *appearance*. I would be tempted to investigate the alternative hypothesis that recognizing the hue of a sample may actually change the appearance of that sample to a subject—so the

sample may look different after it has been recognized from how it looked before (and the post-hue-recognition classification may be perfectly true to the post-hue-recognition appearance).

⁸⁸ ‘Finally, it is also not a matter of indifference, how long the period is during which the colour is acting upon the eye. In order for the red sensation and the remnant of the green sensation that may be present to develop in the colour-blind person, a considerable amount of time must elapse if he is looking at a large coloured surface, and all the more, the smaller the surface is and the paler and the more impure the colour is. With a normal person it makes almost no difference at all. Even if a red and a green light flash up for just a fraction of a second, he recognizes the colour immediately; the recognition time is almost unmeasurably short.’ (Nagel 1908b, 23) Cp. also Wilson 1855, 30 (Mr. T., quoted at end of §8 above); Guttman 1907-08; and Montag 1994..

⁸⁹ Nagy & Boynton 1979 considered two different illumination levels in their surface colour naming experiments, and Montag 1994 three and more levels (investigating rod contributions).

⁹⁰ Boynton made a good case for going beyond colour-matching experiments -- ‘Class A’ experiments, to use G. S. Brindley’s term -- and investigating colour naming as well (see e.g. Boynton & Scheibner 1967, 206, 211, 220). My proposal is to take a further step and attend not just to basic colour-classifications (e.g. red, green, yellow, blue), but also to the higher-level (but no less perceptible) interrelations perceived among colours: and to do this for the colour-blind as well as the normal trichromat.

⁹¹ Among the deuteranopes of Smith and Pokorny 1977, for example, 10 refused an 8° large-field match of Y₅₈₉ with G₅₄₅, but one (CM) accepted it. Was the difference in appearance between the relevant half-fields less for CM than for the others? Or was he simply more tolerant in his acceptance of approximate matches? (And were there differences at the retinal level between him and the others—or only at the stages of later processing?) These questions can be investigated, even if the evidence is hard to assess. (One can ask, for example, on the first issue: ‘On a scale of 1 to 10, how well do these fields match?’; not just ‘Do these fields match?’) Of the two protanopes in Montag and Boynton 1987, one (P2) behaved quite similarly to a normal trichromat when presented with larger fields (3.8 cm square surface colours, viewed at 60 cm) and given up to 5 sec. for his responses; but the other (P1) used the term ‘Green’ quite indiscriminately even under those conditions—as much for reddish things as for greenish.

⁹² Lines of apparently equal saturation are strictly not circles in CIE *x, y* chromaticity space—see e.g. Wyszecki & Stiles 1982, 857 for the shape formed by a set of Munsell colours at Chroma 10 and Value 5: it is more like a round-cornered triangle, or a circle stretched outwards at three points (towards, approximately, Blue₄₆₀, Green₅₃₅ and Red₆₅₀).

⁹³ Under the direction of John Mollon.

⁹⁴ My first ordering of the caps (under a Macbeth lamp, ~200 lux) was: P 15 1 14 2 13 3 11 4 12 10 6 7 8 9. My later ordering (under Sylvania Luxline Plus F58W/860, ~500 lux) was: P 1 2 3 4 5 6 7 8 9 11 10 12 13 14 15. When the illumination was changed back to the first condition (Macbeth lamp) and the caps were reshuffled, my ordering was: P 1 2 3 4 5 6 8 7 9 11 10 12 14 13 15. I am most grateful to the experimenter for these tests; responsibility for the non-standard suggestions I am developing from them is of course entirely my own. There are phenomena here that I think

are worth examining rigorously: but the present comments are meant as only a first glance at some of that material which would have to be examined.

⁹⁵ ‘My own strong conviction, which is at variance with the opinion of some distinguished writers on optics, is, that red and green are both visible in favourable circumstances to the majority of the subjects of chromato-pseudopsis. ... All the colour-blind persons whose vision I have formally tested, could in favourable circumstances occasionally distinguish red from green.’ (Wilson 1855, 46-47) Maxwell responded in a letter, which Wilson included as an appendix to the book version of his publication.

⁹⁶ ‘All in all, one should really say, therefore, that the factual basis for the much mentioned view, according to which dichromats are red-green blind and yellow-blue seeing, is thoroughly scanty.’ von Kries (1905, 166)

⁹⁷ Among other problems, Collins makes no serious use of the distinction between dichromats and anomalous trichromats, where she needs it, though she knows it as one of Nagel’s special interests.

⁹⁸ Even more strangely, Judd (1948, 255) says ‘a reasonable search of the literature has failed to uncover any reliable evidence against’ his view that dichromats see only yellow and blue—whereas Judd himself has cited George Wilson’s book and three articles of Nagel’s, and Nagel’s work at least (even if one might think of it as something that in the end was outweighed by opposing evidence) could not by any good judge be dismissed as simply unreliable.

⁹⁹ ‘On the basis of the opponent-color model that has been presented, one can make reasonable predictions about what dichromats should see, assuming that the replacement hypothesis is correct and that the visual pathways are normal except for the r-g channels. For spectral colors, there is a division at the neutral wavelength that should cause shorter wavelengths to appear blue and longer ones yellow ...’ (Boynton 1979, 380)

¹⁰⁰ See Figure 2 in Jameson & Hurvich 1978, which gives the ‘Locus of D-15 test colors in perceived color space’ for their deuteranopes and protanopes: the colours are all presented as lying on the blue-yellow axis, with absolutely no element of apparent redness or greenness. The diagram is not the product in any way of the experiments reported in the article: it is an expression of a piece of *theory* that, of course, has some evidence to support to it, as well as evidence (as we have seen) against. The authors present it as simply straightforward established fact—which it most certainly is not.

¹⁰¹ Some of them also imagine that the issue is almost impossible to investigate scientifically, so their own views—whatever they may be—are safe at least from falsification. The strangest view of all is perhaps that of those who proclaim that the issue is actually meaningless (‘sensations are not logically comparable between different individuals’ (Boynton 1979, 381)), while yet making affirmations—often undauntedly combative or dismissive—on the issue itself. A variant of the first view is Judd’s: ‘The consequences of this choice [of 575 nm and 470 nm as the sole hues sensed by the protanope or deuteranope] are *bound to appear correct* to binocular red-green confusers. These consequences [presented in eight double-column pages of Tables, in which Judd specifies, in the Munsell system, the supposed appearance of some 360 ordinary colours to protanopes and deuteranopes—they all, of course, turning out to be merely varieties of 5Y or 5PB or N—] *can be contradicted only* by observers having *unilateral* defects; and the probability of such contradiction arising may be estimated from the fact that the eight cases recorded so far ... fail to contradict it.’ (1948, 255, my emphasis) This is false at every stage: Nagel is a binocular dichromat, and can give very

good reasons for rejecting the only-yellow-and-blue view—for example his report (1910, 9-10; see §4 above) of the difference between his ordinary colour-experience and the extraordinary experience of *really* seeing the world in merely yellow, blue and neutral, after having ‘exhausted’ the red sensation. It is evident that there is no necessity at all for Judd’s two-hue view to ‘appear correct’ to Nagel the bilateral dichromat. On the contrary, Judd’s proposal of 2-dimensional structure of dichromat colour space can perfectly well appear incorrect to a dichromat and the discrepancy can perfectly well be communicated. Even among 2D spaces, there are ways, I think, to distinguish experience of e.g. yellow and blue from experience of, e.g., yellow and red. (For example: the former are complementary and contrasting colours, their mixture is a neutral, and the shortest path between them goes through neutral; the latter do not have those features. There is no such thing as yellowish blue, but there is such a thing as yellowish red.) And there are ways to distinguish experience of yellow and blue from experience of, e.g., orange and turquoise. (The former (or at least, *certain* yellows and certain blues) are ‘unique’ hues—looking to contain no hint of any other hues—, the latter are not.) And it may even be (and I think it is) that experience of yellow and blue can be distinguished from experience of red and green. (See my 2007.) As for the second part of Judd’s claim: the eight unilateral cases that he studies actually give us plenty of reason to suppose that Judd’s view *cannot* be correct—but I have said enough on that already in §4.

¹⁰² I have in mind, among other things, the rethinking of these issues under way, but I think, not completed, in the work of Adelson, Brainard, Maloney, Zaidi and others—as also, in the last century, in David Katz, Adhémar Gelb, and Merleau-Ponty—which I wish I could say more about here.