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Theoretical approaches to lightness and perception†

Alan Gilchrist
Rutgers University, Department of Psychology, 101 Warren St, Newark, NJ 07102, USA;
e-mail: alan@psychology.rutgers.edu
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Abstract. Theories of lightness, like theories of perception in general, can be categorized as high-level, low-level, and mid-level. However, I will argue that in practice there are only two categories: one-stage mid-level theories, and two-stage low-high theories. Low-level theories usually include a high-level component and high-level theories include a low-level component, the distinction being mainly one of emphasis. Two-stage theories are the modern incarnation of the persistent sensation/perception dichotomy according to which an early experience of raw sensations, faithful to the proximal stimulus, is followed by a process of cognitive interpretation, typically based on past experience. Like phlogiston or the ether, raw sensations seem like they must exist, but there is no clear evidence for them. Proximal stimulus matches are postperceptual, not read off an early sensory stage. Visual angle matches are achieved by a cognitive process of flattening the visual world. Likewise, brightness (luminance) matches depend on a cognitive process of flattening the illumination. Brightness is not the input to lightness; brightness is slower than lightness. Evidence for an early (<200 ms) mosaic stage is shaky. As for cognitive influences on perception, the many claims tend to fall apart upon close inspection of the evidence. Much of the evidence for the current revival of the 'new look' is probably better explained by (1) a natural desire of (some) subjects to please the experimenter, and (2) the ease of intuiting an experimental hypothesis. High-level theories of lightness are overkill. The visual system does not need to know the amount of illumination, merely which surfaces share the same illumination. This leaves mid-level theories derived from the gestalt school. Here the debate seems to revolve around layer models and framework models. Layer models fit our visual experience of a pattern of illumination projected onto a pattern of reflectance, while framework models provide a better account of illusions and failures of constancy. Evidence for and against these approaches is reviewed.

Keywords: lightness, brightness, sensation, codetermination, anchoring, mosaic stage, luminance, Ganzfeld, adaptation, proximal mode, embodied perception, the new look, layer models, anchoring theory

1 Against two-stage theories of perception

Conventional thinking has long held that perception involves two stages: an initial sensory stage consisting of sensations that correspond to properties of the retinal image, such as luminance and visual angle, and a subsequent stage involving percepts that correspond to the distal properties of the scene, such as lightness and actual size. The transition from the first to the second stage is widely attributed to cognitive processes rooted in past experience. I will argue that these two stages do not exist. My analysis will include examples from size, motion, and lightness perception.

2 Lightness theories

I will begin with lightness, which is the perceived white, black, or gray shade of a surface. We are talking about the perception of an object property, a surface property. We are not talking about the perception of light; that would be brightness. Lightness theories are conventionally divided into three classes: high-level, low-level, and mid-level theories. But I want to suggest that actually there are only two classes because, in general, low-level theories tend to add a

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high-level component (Blakeslee & McCourt, 2003, pages 48 and 68; Hering, 1874/1964; Hurvich & Jameson, 1966, page 88); and, by and large, high-level theories tend to assume a low-level component (Helmholtz, 1866/1924). So the real choice is between one-stage and two-stage theories. The familiar distinction between sensation and perception goes back to the beginning of our field. And this idea is alive and well today.

3 Sensory stage

First, is there a sensory stage? Notice that even those people who have been ardent supporters of the idea of a sensory stage acknowledge that it is very difficult to observe these sensations. Here are some quotes by Helmholtz, von Kries, and Katz:

> "It might seem that nothing could be easier than to be conscious of one's own sensations; and yet experience shows that for the discovery of subjective sensations some special talent is needed ..." (Helmholtz, 1866/1924, page 6).

> "However, one must consider that these sensations can only be observed with difficulties and under special conditions. We do not doubt that they exist, but they are only in a limited way objects of our cognition, comparison, or conceptual apprehension" (von Kries, quoted in Gelb, 1929, translated by D Todorović).

> "The intensity of a retinal image, however, usually evades observation and can be compared with that of another only after reduction" (Katz, 1935, page 141).

They all believed in sensations, but they found it very difficult to spot them.

4 Difficult to match proximal properties

We know that matching proximal qualities is very difficult, as opposed to matching objective properties of the distal stimulus. I am indebted to Barbara Gillam for this example. In the photograph shown in figure 1, is the top edge of the left-hand wall collinear with the bottom edge of the right-hand wall? Subjects find this a very difficult task. Now you might find this difficult, but feasible, to do in a photograph, but when you are standing in the room the task appears impossible, perhaps even absurd. On the other hand, if you ask subjects whether those two edges are parallel in the world, that is quite easy to do. Subjects have no trouble with that task.

Figure 1. It is very hard to judge whether the top of the left-hand wall is collinear with the bottom of the right-hand wall. This should be an easy task if there is an early sensory stage that represents the retinal image.

Or consider Ted Adelson's checkered shadow, shown in figure 2. If we ask the question "Which of those two squares is brighter? What's the relative brightness of those two squares?", that is a very difficult task. But lightness is not difficult. When you look at this, you see immediately that the lower square is a lighter shade of gray than the upper square. But which one is reflecting more light? That is tough, although, once again, it is much easier in a photograph than in the real world.

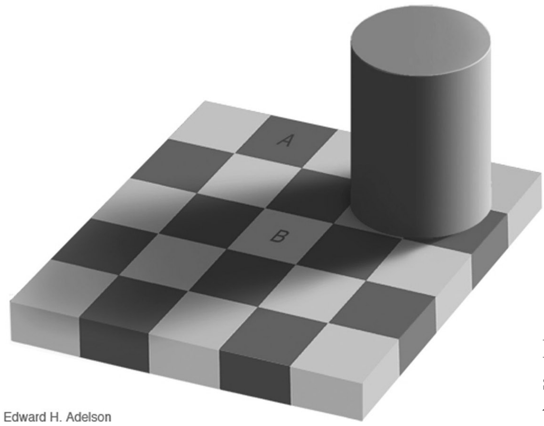

Figure 2. While it is easy to see the lightness of the squares marked A and B it is virtually impossible to appreciate that they are identical in luminance.

4.1 *Sensations overridden by the percept?*

People have defended raw sensations in various ways. Stumpf (1906) argued that we have raw sensations but we just do not notice them. I recommend reading Köhler's (1913/1971) blistering critique of that idea. Another, more plausible, way of defending raw sensations is to argue that we have raw sensations but they are overridden by the percept—by that second stage, sort of masked out. Various people have claimed that they have found ways of revealing the existence of those sensations. Sekuler and Palmer, in an influential paper back in 1992, proposed that early on there is what they called a mosaic stage. For an occlusion stimulus in which a square partly covers a disk, they show that after about 200 ms the disk functions in priming experiments as a complete disk, but they argue that prior to that it functions like a piece of a mosaic, like a pac-man. I believe that a closer look at that paper will show that the data are really not very convincing.

The subject's task was to report as quickly as possible whether two test shapes presented side by side were the same or different. Prior to this task the subject was shown a priming shape. It is known that if the prime is similar to one of the test shapes, the reaction will be faster. Their response time data are shown in figure 3. At 740 ms, when a complete disk is used as a prime, the responses are faster to a pair of complete shapes than to a pair of mosaic shapes (upper left). The same result is obtained for an occluded disk (upper right). However, when the prime is a mosaic shape, then response is faster to a mosaic pair (upper middle). The four graphs in the lower panels show the effect of an occluded disk prime at four different exposure durations. Notice that at no stimulus-onset asynchrony is the response faster to a mosaic pair, even during the presumed mosaic stage (50 ms and 100 ms).

Now Sekuler and Palmer did report statistical tests consistent with their conclusions, but I believe that, consistent with these data graphs, the most reasonable conclusion is that, prior to 200 ms, the primes are presented too briefly to have any effect at all.

Then Rauschenberger and Yantis (2001), in a paper in *Nature*, claimed that they had found evidence for this mosaic stage. But only three years later Rauschenberger and colleagues (Rauschenberger, Peterson, Mosca, & Bruno, 2004) retracted that conclusion, writing, "Our results are quite incompatible with the two-stage model" (page 354). I think this sequence of events has happened frequently in the field, where people think they have found these raw sensations and then it turns out that … not so much.

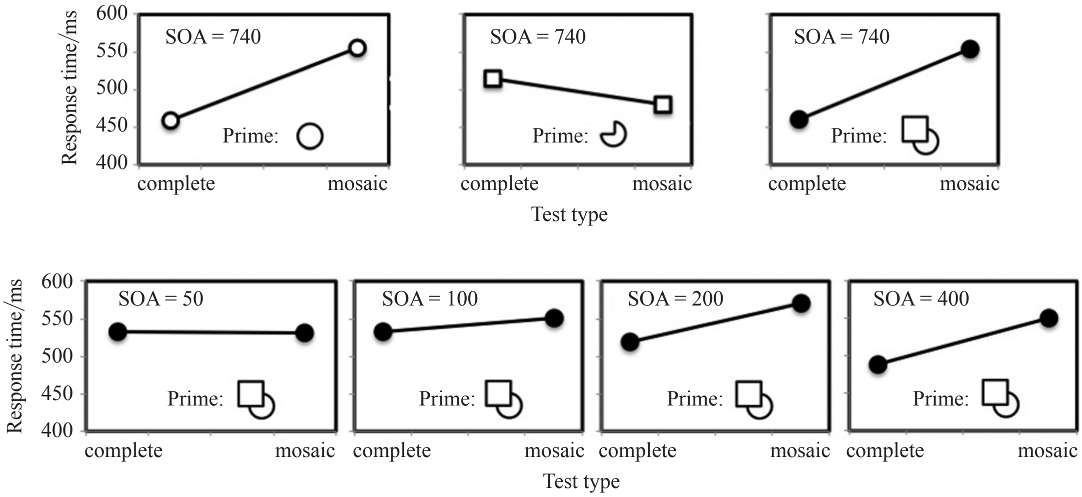

Figure 3. When the square occluding the disk is used as a prime (lower panels), at no stimulus-onset asynchrony (SOA) does one observe the negative slope produced by the mosaic prime (upper middle) (source: Sekuler & Palmer, 1992; adapted with permission).

Moore and Brown (2001) argued that they found evidence for an early representation of raw luminance. It was a visual search study. The task for the subject was to find the gray square in a group of other gray squares that was just slightly lighter or slightly darker than the others. But the trick is that half of the distractors were covered by a gray transparency. And this was arranged so that in two critical conditions (out of four) the target happened to have exactly the same luminance as half of the distractors. They found that in just those two conditions reaction time was slowed down by a small but statistically significant amount. They argued that this is evidence of a residual effect of the raw sensation of luminance. Well, that logic is valid only if you assume that lightness constancy is 100% perfect, and of course we know that it never is. And it turns out there is a much simpler explanation of this finding. If there is any degree of failure of lightness constancy—that is, if any element under the transparency (for example, the target in one condition and the distractors in another condition) appears even slightly darker due to imperfect constancy—then in the two critical conditions the target and half of the distractors will be more similar in lightness, and that is going to slow you down. This finding can be explained at the level of the percept with no need to talk about sensations.

5 Brightness models

Now we come to brightness models, and there are many of them. I will mention just three of them. Blakeslee and McCourt (1999) have a widely endorsed model. Shapiro and Lu (2011) have a model that is incredibly simple—just a high-pass filter—and does a lot of work. And János Geier (Geier, Bernáth, Hudák, & Séra, 2008; Geier & Hudák, 2011) has recently presented a model that is surprisingly effective.

By the way, if you are wondering—lightness is perceived reflectance and brightness is perceived luminance. One way to think about it is that lightness is to brightness as perceived size is to perceived visual angle.

Now I have always wondered why there are any models of brightness, let alone many. Luminance is not informative. Why does the visual system need to know how much light is coming from some sector of the visual field? Generally speaking, that is not very informative. It is very informative to know the reflectance of surfaces. Likewise in the domain of size and space, where are the theories of perceived visual angle? So it is not clear to me why we have any theories of brightness at all. Remember what Hering (1874/1964, page 23) wrote:

> "Seeing is not a matter of looking at light-waves as such, but of looking at external things mediated by these waves; the eye has to instruct us, not about the intensity or quality of the light coming from external objects at any one time, but about these objects themselves."

Now when I have asked my brightness-modeling colleagues, "Why study brightness?", the most common answer I get is that brightness is the first stage. The idea is that there is an initial representation of brightness and that is followed up by some cognitive process that turns that into lightness. So here are our sensory and cognitive stages again.

5.1 *Lightness derived from brightness?*

Well, is lightness really derived from brightness? Many years ago Jim Schubert and I did a kind of heroic experiment (Gilchrist, 2006, pages 135–140). We wanted to slowly ramp the luminance of a Ganzfeld. We had a subject lying on his back underneath an aquarium, we had a very bright light source above the aquarium, and we had inky water in the aquarium. We could gradually fill or empty the aquarium. And so as we changed the level of the water in linear fashion, it changed the amount of light transmitted in logarithmic fashion, which is exactly what we wanted. We had a huge range of luminance, and we did not change the color of the light, and the luminance changed at a constant rate. The subject had a translucent contact lens in each eye. The two eyelids were taped open to avoid blinking. And because we were doing trials that would last up to one hour, we covered each eye with a section of a table tennis ball. That worked very well to keep the eyeball moist, and it also added further diffusion of the light. I do not think anyone has ever produced a more homogeneous Ganzfeld. We changed the luminance of the Ganzfeld very slowly—at the rate of three log units per hour. The question was: at what point can the subject tell us whether the light is getting brighter or dimmer? And the results were that the change in absolute luminance was detected after about ten minutes—which is a threefold change in brightness before the subject could respond correctly.

In a follow-up condition, unfortunately done with a different apparatus, we had subjects looking at a stimulus consisting of a disk within a Ganzfeld, and we changed the luminance of either the disk or the Ganzfeld at that same slow rate (keeping the other part constant), and here the question was: how soon can the subject detect a change in relative luminance? That was detected in approximately one minute. So, if a change in relative luminance is detected ten times faster than a change in absolute luminance, it seems to me that this works against the idea that lightness comes from brightness.

Now you might say, is not relative luminance a relationship between two absolute values? Not necessarily. Take the example of a balance beam. You can find out the relative weight of two people without ever knowing the absolute weight of either one. You put them on opposite ends of a seesaw, and you move the fulcrum until you get a balance. Then the relative distance of each person from the fulcrum tells you their relative weight. But you never know the absolute weight of either one. I think that is the same thing that Yarbus (1967) was trying to say about the role of eye movement and stabilized images; that we are really directly picking up relative luminance without having to go through the luminance stage. Similarly, Runeson (1977) has described mechanisms by which stimulus relationships can be picked up directly, with no need to register absolute values.

I think we know the same thing about motion. The threshold for relative motion is much lower than the threshold for absolute motion (Wallach, 1976, page 87). If you have a single spot of light in a totally dark room, you have to move it pretty fast before people can tell reliably which way it is moving. But if you have two spots and one is moving relative to the other, you are extremely sensitive, at least to the relative motion. So I think the logic is the same in motion and in luminance. The relative motion between two points in the image does not seem to be derived from the absolute motion of each.

5.2 *Adaptation and lightness*

Steve Ivory and I have recently been doing some work in my lab, looking for effects of adaptation on lightness. As far as I know there is no work on adaptation effects on lightness. In studies of brightness perception it is typical that people dark-adapt their subjects. But in our investigations of lightness perception, adaptation does not seem to have any effect. Whether the observer has been sitting in total darkness for five minutes or in a very bright field before viewing a particular Mondrian does not seem to have an effect on the reported lightness values. Now we know that adaptation has a large effect on brightness. So if brightness is supposed to be the input to lightness, do we not have a problem here? If adaptation has an effect on brightness, should that not carry through to lightness? So it seems to me that this is another strong argument against the two-stage model.

Now, some people argue that not only are there two stages, but that the brightness stage represents true vision, and the lightness stage represents cognition. You know, goldfish have lightness constancy (Burkamp, 1923) and they have color constancy (Ingle, 1985), and do we want to impute cognitive functions to a goldfish? And by the way, I do not believe goldfish can do brightness matching. I am willing to bet that they cannot respond to luminance, when reflectance and illumination vary.

5.3 *Brightness in chicks?*

We are going to do some work with newborn chicks in collaboration with Giorgio Vallortigara. Katarina Jevtić recently went to his lab to learn how to run newborn chicks, and we are going to do some lightness and brightness work with these chicks. We will of course do a lightness constancy experiment, but there is nothing surprising there because it has already been done. Köhler (1917) has done this before and shown that chicks do have lightness constancy. But we want to go on and do some additional studies, including a test to see if they can do brightness perception. I actually do not think that they can. There are three corridors. We are going to train the chick to go to the square that has a certain absolute luminance. From trial to trial the shade of gray of the lids will change and the illumination in the corridor will change. What the chick has to learn is to go to the square that has the same absolute luminance. And I am willing to bet that they cannot do that—after thousands of trials—we will find out.

By the way, I want to make a prediction. I think you can probably train a chick on any property of the world, any distal property, including illumination level. But I do not think you can train these chicks to respond to a property of their own retinal image. That is just my opinion.

6 Proximal mode perception

Now, if sensations do not exist, it raises the question of how we explain proximal mode perception. How do we explain that people have some ability to judge visual angle and they have some ability to judge luminance? Where does that come from? Well first of all, of course, I want to argue that it is not read off of some early image-based representation. And I want to argue that it is actually a cognitive operation, which I would call "flattening the world". If you show somebody a large object far away and a small object nearby, and say, "Which one has the larger visual angle? Which one makes the larger image on your retina?", what do people do? They typically close one eye, and they often line themselves up with the two objects. So why are they doing that? I would suggest that they are trying to imagine the two objects as being at the same distance. In other words, they are sort of trying to flatten the world. And if they can imagine the two objects at the same distance, then they can just exploit their ordinary size perception mechanism, and read off the results and give you the answer. By the way, in the spatial domain anything that helps you to flatten the world will lead to better visual angle matching. So, for example, monocular viewing will probably help (Holway & Boring, 1941), as will using a synopter (Koenderink, van Doorn, & Kappers, 1994), and we know that inverting the scene will help you do better at visual angle matching.

Now I want to make the same analogy in lightness, and if I can use a figure of speech, I will talk about flattening the illumination. When you try to judge the relative brightness of objects in two different illuminations, you are trying to imagine the scene as homogeneously illuminated. I am actually trying to reverse the usual idea, and I want to suggest that lightness is true vision and brightness is actually cognitive.

We have been doing some experiments in my lab during the last couple of weeks. We put a light gray square on the wall and project a rectangular beam of light nearby. And it is very obvious to the subject that it is a beam of light, that there is an illumination edge. And in the bright illumination we have a grid of 16 squares. First of all we do lightness matching, and subjects are very good at that. We get more than 90% constancy. It is an easy task. They respond very quickly. And then we ask them to do a brightness match. Now, we have trained them up first. We spend 10 or 15 min training them on what we mean by brightness, what we mean by matching the amount of reflected light, and so forth. But they have a hard time doing it. They find it very hard to do. Their results are very far from a luminance match. I think in the end you have to say that they are guessing. When you talk to the subjects and they tell you what their strategies are, they are all over the map—they just do not know what they are doing.

Now in one condition we have them looking at the scene through an aperture. That seems to help them match luminance. It seems to help them flatten the illumination. So again, I think there are things you can do to help subjects make a brightness match.

6.1 *Phlogiston, ether, and raw sensations*

In the history of science we find several concepts that have finally had to be abandoned. I do not know if you remember the concept of phlogiston. Phlogiston was a substance that was supposed to be given off by things when they burn. Things that burn contain phlogiston. And in the end, of course, it turned out that there was not any phlogiston. Remember the luminiferous ether? How can light waves travel through empty space? Space must be filled with something. We are going to call it the ether. But despite a lot of searching, scientists could not find any evidence for the ether, and so they finally gave it up. I want to suggest that raw sensations fall in that same category. The time has come to give up the idea.

As Merleau-Ponty (1962) said, “The alleged self-evidence of sensation is not based on any testimony of consciousness, but on widely held prejudice.” MacLeod (1932) wrote:

> “Phenomenological description furnishes no basis for the assumption of a primary, invariant, stimulus-conditioned sensation, which might act as raw material upon which experience could operate.”

7 Cognitive stage

So what about the cognitive stage? Now, I am going to argue that there is no cognitive stage. I do not necessarily want to say that there is no effect of cognitive factors on perception, but in general I do not think there is a cognitive stage.

Let me start with this challenge. If a baby sees the wrong shade of gray, what is the feedback that is going to correct that? I think that is a very difficult question to answer. And I have asked a lot of people. People say, “The baby sees that if you move an object from the light into a shadow it changes its brightness just due to the illumination.” But that assumes lightness constancy, and you cannot assume that. You see, the baby does not know that the object is not changing in surface gray when it goes across that edge.

You can come up with an explanation here, but I think you are going to end up imputing a very sophisticated cognitive process to the baby. And is it not a lot simpler to just say that lightness constancy comes hard-wired, because fish do it anyway? *Is evolution so inefficient that, despite millions of years evolving in a world of light and shadow, each organism must figure it out from scratch?*

7.1 *Hills and backpacks*

We now come to a topic that can be very entertaining. It is sometimes called embodied perception, but that term is really broader than the work I have in mind. Back in the 1940s and 1950s we saw the 'new look' in perception. We were told back then that a quarter looks larger to a poor boy than to a rich boy (Bruner & Goodman, 1947). The work in this genre was poorly done and soon discredited. But it seems to be making a comeback, led by Dennis Proffitt (2006), who with his associates has subjects judging the steepness of the hill they are standing in front of. The claim is that if you are wearing a heavy backpack, the hill looks steeper. Not simply that you report it to be steeper, but that it actually looks steeper.

As you may know, Frank Durgin and colleagues (Durgin, Baird, Greenburg, Russell, Shaughnessy, & Waymouth, 2009) have replicated that study, and they found the same results. But after the steepness report was collected, the subject was asked a very interesting question: why did they suppose they had been asked to put on a heavy backpack? It is a good question. And twelve out of the thirteen subjects replied that the backpack was intended to alter their judgment of slope. And that already shows that the subjects were not naive, even though the method section in Bhalla and Proffitt (1999) says that they were naive. They are not naive. Then the subjects were asked whether they thought that the backpack did have any effect on their judgments. Five of those twelve indicated that the backpack had indeed affected their judgments, and the judgments of those five were significantly steeper than those of the other subjects. Subjects who wore a backpack in a third condition with a cover story about wearing the battery pack for some apparatus showed no effect on the steepness of the hill.

Chaz Firestone has been making some very interesting observations about the work of the Proffitt camp. He has taken the El Greco fallacy and turned it into a very powerful tool. If you are not familiar with the El Greco fallacy, briefly, El Greco drew these long slender figures, and it was argued in a book by an ophthalmologist (Beritens, 1914) that El Greco had an astigmatism that squeezed the retinal image horizontally. We call it a fallacy because when he would paint long slender figures and then look at his own painting, his astigmatism would stretch them out even more, and it would look wrong. But if he painted them correctly, his eye would squeeze the object and the painting equally and his painting would appear to match the object. That is why it is called a fallacy. Now Firestone and Scholl (2014) have shown by some very clever experiments that the same kind of data reported as supporting the claims of embodied perception can be obtained under conditions to which the El Greco fallacy applies.

For example, Stefanucci and Geuss (2009) claim that an aperture, such as a doorway, looks narrower if your arms are stretched out horizontally, or if your body is simply wider. They reported an experiment in which subjects stood in front of a doorway and instructed an experimenter to extend a tape measure horizontally until it appeared to match the width of the doorway. Half of the subjects stood with their arms extended to both sides, and half stood with their arms at the sides. The subjects with arms out reported that doorway to be about 9% narrower than the subjects with arms in.

Firestone and Scholl (2014) then repeated the experiment, but they replaced the tape measure with another identical doorway that was adjustable horizontally. Half the subjects held their arms out and half did not. The arms-out subjects set the adjustable doorway to be narrower than the standard doorway. But of course this makes no sense. If holding your arms out makes the standard doorway appear narrower, it should have the same effect on the adjustable doorway and the results should be veridical. Firestone and Scholl show that the Stefanucci and Geuss results are found under conditions that constitute the El Greco fallacy, and they conclude that, whatever these effects are, they are not effects on vision.

I want to go further than Firestone and suggest what kind of effects they might be. First, I think that the hypotheses in these experiments are pretty transparent. It is not rocket science figuring out the hypothesis when you are asked to put on a heavy backpack in front of a slope and judge the steepness of the slope. Second, subjects want to please the experimenter. That is, social pressure. Perhaps you have experienced this same thing. You visit a lab, your colleague shows you his or her latest effect, and you do not exactly see that effect. But you know what your colleague is looking for. You may not give in to the pressure; you may give an honest response. But you feel that pressure. And so I am suggesting that in these experiments most of the subjects do not give in to the pressure; most of them call it as they see it, which is fine. But 20% give in to the social pressure, and that is enough to give you significance, and you have got your publication.

7.2 *High-level lightness theories*

Let us talk for a moment about high-level lightness theories, which of course go back to Helmholtz and taking the illumination into account. There is another type of lightness constancy first studied by Katz (1930) that has been revived recently. MacLeod (1932) called it the method of anomalous orientation. If you take a piece of gray paper and rotate it relative to a light source, it will get brighter and darker but you tend to see it remaining relatively constant in lightness. It is not a high degree of constancy. But this has recently been taken up in the laboratories of Larry Maloney (Boyaci, Maloney, & Hersh, 2003) and David Brainard (Bloj et al., 2004; Ripamonti et al., 2004), and they both attributed the constancy that they found to the notion that the visual system is taking into account, is estimating, the direction and intensity of the light source and then plugging that into some physical formula that is embedded in your brain.

I find this account quite implausible. First of all, I want to point out that it is rarely the case that there is only one light source. In the situation shown in figure 4, for example, what is the direction and intensity of the light source on one of the round tables? I mean, there are lights on the ceiling, there is light coming in the windows, and do not forget, every surface

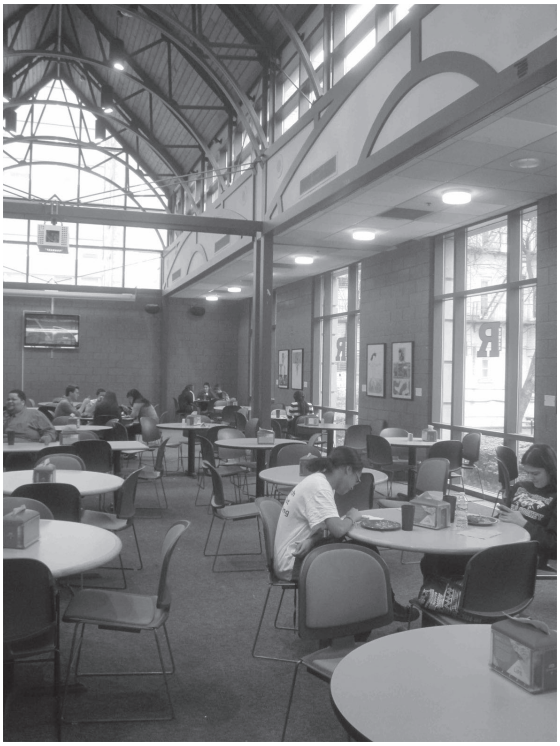

Figure 4. To compute the lightness of a surface lying on a table, the visual system would have to estimate the direction and intensity of light from all of the light sources, all of the windows, and all of the nearby reflecting surfaces, according to recent Helmholtzian proposals.

is reflecting light onto other surfaces. So even in the natural world where there is only the sun perhaps, there are always rocks and other things nearby that reflect light. So, in modern scenes like this I just do not find it plausible that you can estimate the direction and intensity of all those light sources. The computational load would be enormous. Besides, there is a much simpler way to go about things, but I will come to that in a moment.

Kanizsa (1979, pages 41 and 43) produced some fun paintings which show that past experience is easily overridden by cues from within the stimulus. One contains a knife that appears transparent even though you do not have any experience with transparent knives. Another shows peasants who appear to be tangled up in the fence, something you have never seen before. But this is really an example of the Petter (1956) effect, in which the visual system favors the interpretation that minimizes the length of subjective contour.

Now I have to make a concession to my colleague Karl Gegenfurtner and his associates (Hansen, Olkkonen, Walter, & Gegenfurtner, 2006) who have done some very clever experiments on memory color. In my book I reviewed, at that point, in 2006, all of the previous work on memory color and I concluded that there was no good evidence for memory color. But then these workers came along and created some really excellent methods. I cannot find anything wrong with their experiments—I am still working on it. They presented subjects with a still-life scene containing a banana. The color of the banana could be adjusted using a joystick. They asked subjects to set the banana to a neutral color. And the subjects actually set the color to a slightly bluish color. This implies that, when the banana is actually neutral, it appears slightly yellowish. Very nice method.

But to keep things in perspective, there are not that many diagnostic objects in the world. If I ask you to look around the room now and tell me where you could use memory color, there is probably nothing. You need bananas. If you have got bananas, you are in great shape.

8 Single-stage models of lightness

Let us move on. If we are to reject these two-stage models, we are left with single-stage models. And when we talk about single-stage models I think we are really talking about gestalt theory. Nowadays I think it is fair to say that single-stage models come in two different classes: layer models and framework models. So let us just take a quick look. Layer models emerged around the time of the cognitive revolution, the computer revolution. They can be called decomposition models because the visual system is thought to decompose the retinal image into those factors that combined to create the image in the first place. They follow the logic of inverse optics.

8.1 *Layer models*

Bergström (1977) has a nice theory that the image is decomposed into common and relative components in the reflected light, very much analogous to the theory of motion perception proposed by his mentor Gunnar Johansson (1977). Adelson and Pentland (1996) came along later on with a very similar model but couched in a wonderful theater workshop metaphor. Barrow and Tenenbaum (1978) coined the term intrinsic images, implying that the retinal image can be thought of as composed of multiple overlapping images. I produced my own intrinsic image theory, arguing in 1979 (and in Gilchrist, Delman, & Jacobsen, 1983) that the visual system encodes the edges directly—that is, it encodes the luminance ratios, classifies them as either a change of reflectance or a change of illumination, and integrates the edges once again, but only within classes. So basically, this classified integration is a method for parsing the retinal image into two overlapping layers, as if we perceive a layer of illumination projected onto a pattern of surface reflectance.

Layer models such as this have certain strengths. They are very good at explaining veridical perception; and, unlike the contrast models that preceded them, they explicitly recognize the perception of illumination and explain it. And I think they are consistent with visual experience because I think we do see the world as if there is a layer of illumination on top of the surface albedo.

But they have certain weaknesses as well. In those days they did not have an anchoring rule—that is, a rule by which relative luminances in the image are transformed into lightness values. Furthermore, they were not very good at explaining lightness illusions, and they were not very good at explaining failures of lightness constancy.

8.2 *An anchoring rule*

Now as far as the anchoring rule, that turned out to be a rather simple matter. My student Xiaojun Li and I (Li & Gilchrist, 1999) wanted to present observers with two shades of gray that fill your entire visual field. We wanted to test two existing suggestions that either the highest luminance always looks white or that the average luminance always looks middle gray. And so we put people into the dome shown in figure 5, and we painted the dome with two shades of gray: middle gray on the left, black on the right. Everybody saw the left side as white and the right side as middle gray. It is a very robust finding.

So we found that the highest luminance looks white. And the average luminance is not necessarily middle gray—that is not the anchor—the anchor is white.

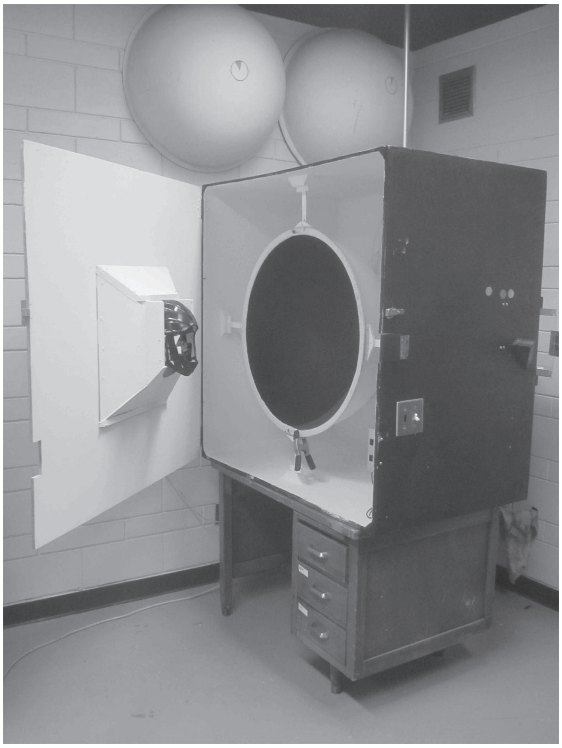

Figure 5. This apparatus allows an observer's head to be placed inside a diffusely illuminated, opaque hemisphere. One half of the interior is painted black, and the other half is painted middle gray, and these two surfaces fill the entire visual field. All observers perceive the gray half as white and the black half as middle gray, a result that decisively favors the highest luminance rule of anchoring.

By the way, if you follow lightness work, Bart Anderson and his colleagues (Anderson, Whitbread, & de Silva, 2014) have recently made the claim that they found a case in which the highest luminance looks middle gray, not white. I will mention just briefly what I think is happening in their experiment. According to their report, the conditions for getting that result are these: you have to have a laboratory that is painted entirely black; you have to have the illumination level set very, very low; and you have a Mondrian with a very truncated range. Now we have tried to replicate that in my lab and have not been successful. According to the authors, the subject is brought into the black room blindfolded, but then to make the match the subject is walked into a normally illuminated neighboring room without being blindfolded.

Then the subject goes back into the black room for the next trial, and back and forth and back and forth. Well, I think there is something in that walking back and forth that accounts for their results, because in my lab our observers memorize the Munsell scale. So they do not have to go between two rooms; they just go in there and tell us what it looks like. And the lowest setting we got was Munsell 8.3. Our illumination level was down to the lowest setting our photometer would read, so it took the subject a few minutes to even see anything. But as soon as they could see the Mondrian, they called the highest luminance a white, if a poor white.

We have also tested observers with Mondrians presented under a six million-to-one range of illumination. Except for the very lowest level of illumination, the curve of the lightness match for the highest luminance across the range of illumination is not significantly different from horizontal.

8.3 *Explaining failures of constancy*

Now as far as failures of constancy go—can you explain those with layer models? There have been several attempts (Ross & Pessoa, 2000). I made one attempt (Gilchrist, 1988). You could possibly do it with a thing called partial integration. You assume that edges are not exclusively categorized as either reflectance or illumination, but there are errors in the edge classification process. You can get some mileage out of that. It helps to explain that a piece of gray paper will look darker in shadow than otherwise. But you cannot explain gamut compression.

8.4 *Gamut compression*

What is gamut compression? If you present a row of five squares, ranging from black to white, suspended in midair and illuminated by a bright spotlight, you do not perceive the full range of gray shades. The black square looks middle gray or even lighter. So even though the actual values cover the whole gray scale from black to white, you perceive a range of grays no greater than the upper half of the gray scale. In other words, the perceived range is compressed relative to the actual range.

Gamut compression was a serendipitous finding that fell out of an experiment done for a very different reason. In a classic experiment Gelb demonstrated that a piece of black paper would look white when it is suspended in midair and illuminated by a spotlight. But when you put a piece of white paper in the spotlight next to the black paper, the black paper that had appeared white by itself now appears gray. This effect had traditionally been called brightness induction, and it was attributed to lateral inhibition (Cornsweet, 1970). I had begun to suspect that this effect is better understood as an effect of anchoring to the highest luminance. So to tease apart the anchoring explanation from lateral inhibition, I exploited the spatial function of lateral inhibition. Lateral inhibition is known to weaken dramatically with retinal distance. So I broke down the Gelb effect into a series of steps. First I placed a dark gray square next to the black square within the spotlight. Then I added middle gray, then a light gray, and finally a white. Each time I added a brighter square, the prior squares appeared to get darker in surface color. But the crucial question was, would the darkening effect get weaker as each new highest luminance got farther and farther away from the black target square? The results said no. The darkening of the target square was simply a function of the degree to which each new square raised the highest luminance.

But the gamut compression came as a surprise. I had expected that, when the final white square was added, the black square would look black. Instead, it appeared as a light middle gray.

This was a real blow to my intrinsic image model (Gilchrist et al., 1983). The stimulus should have been a no-brainer for that model. There is no question that the system can encode the edge ratios. Classifying the edges should be easy. The edges between the squares are sharp, and they divide coplanar regions, so they should be classified as reflectance edges. The occlusion boundary around the whole group must represent an illumination change. So what is the problem? Why did the model fail so badly?

8.5 *Staircase Gelb effect*

In working with this effect, which Ross and Pessoa (2000) have dubbed the staircase Gelb effect, we found that it occurs only when the spotlight exists in the context of a more dimly illuminated room. That is, the effect requires the simultaneous presence of two different regions of illumination. And this led me to propose what is called anchoring theory, according to which each square is seen partly in relation to a local framework composed of the five squares and a global framework consisting of your whole visual field. The concept is illustrated in figure 6. If each of the squares were anchored solely within the local framework, the data would lie along a diagonal line of slope 1. But if each of the squares were anchored globally, then every square would appear white, because every square has a luminance at least as high as white, and the data would lie along a horizontal line. And when you do a weighted average for each square, you find a compromise between the two theoretical lines that nicely fits the empirical data. And you actually see that the errors bars show a gradient in size, consistent with the concept of a compromise between local and global values.

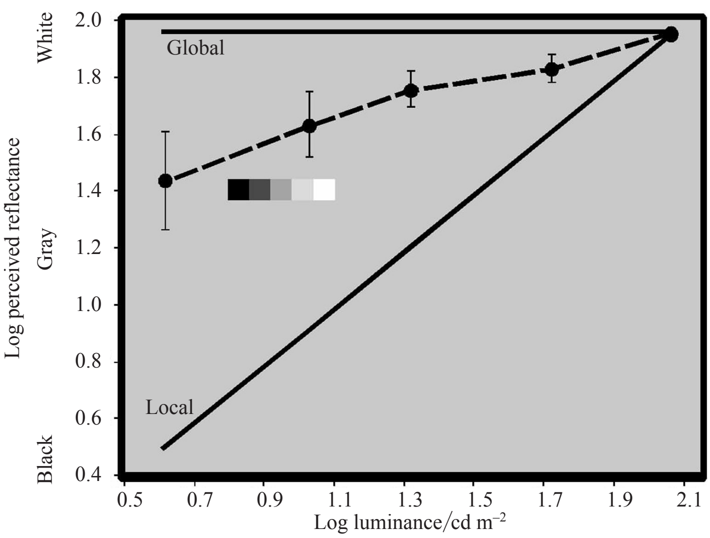

Figure 6. Perceived lightness (log reflectance) plotted against actual luminance (in cd m^{-2}) for five gray squares in a spotlight. The solid lines show lightness values computed within only the local framework (spotlight), or within only the global framework (source: Gilchrist et al., 1999; adapted with permission).

8.6 *Kardos and codetermination*

Not long after this, Dejan Todorović took on the task of translating into English the book *Object and Shadow* by Lajos Kardos (1934), the brilliant but neglected gestalt theorist. I discovered that Kardos had proposed roughly the same idea, which he called codetermination. In figure 7 we see his idea applied to the basic lightness constancy experiment by Katz (1935). For the target in shadow, the shadowed field is the relevant field and that produces the constancy. But there is some influence from the foreign field, and that is what induces the failures of constancy. Kardos really had the first theory of failures of lightness constancy.

At that point I saw a way to put the idea of codetermination in a head-to-head test with the concept of classified edge integration (Gilchrist et al., 1999). A large black rectangle is suspended in midair. It contains two square targets, a middle gray target on the left and a white target on the right. A bright beam of light is projected onto the left half of the tableau, covering the middle gray square. Now the square on the left appears completely white, and slightly lighter than the square on the right, which appears as a very light gray, even though the penumbra at the border where the beam of light cuts across the black rectangle reveals the true conditions of illumination.

According to classified edge integration, such an illusion should not occur. The visual system should integrate only the reflectance edges that fall along a path between the two

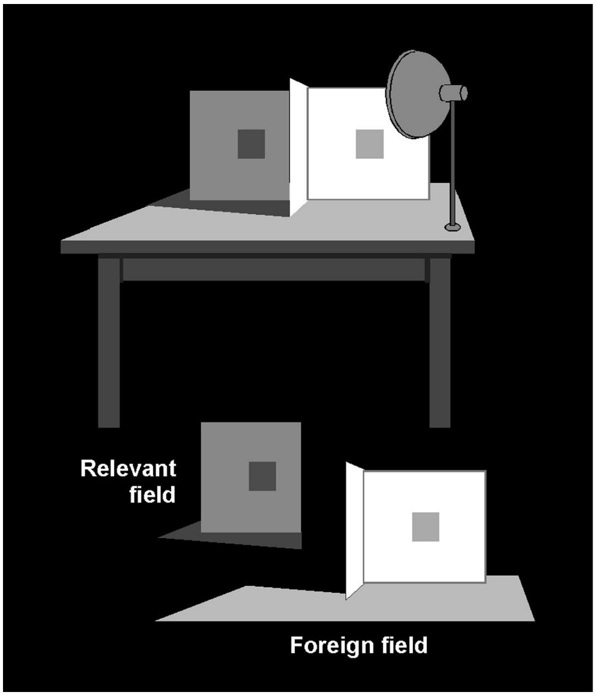

Figure 7. According to Kardos (1934), in this typical light/shadow arrangement used by Katz, the lightness of the target on the left is determined partly by its relationship to its relevant field and partly by its relationship to the foreign field.

squares, ignoring the obvious illumination edge. This integration should reveal that the left square is much darker in lightness than the right square. On the other hand, according to anchoring theory, the square on the left is not only the highest luminance in its local field of illumination but also the highest luminance in the whole display. Thus it should look clearly white. The square on the right is the highest luminance in its local framework but not in the global framework, so it should appear as a kind of off-white. I did not think we would get that result. I expected that the results would support intrinsic image theory. And when it did not, I became more convinced of the importance of codetermination.

8.7 *Is the illumination level needed?*

Getting back to Helmholtz, he said that we unconsciously take the illumination into account. But we do not have to know the illumination level. This insight was inspired by something Irvin Rock (1977) observed regarding my experiments on depth and lightness (Gilchrist, 1977, 1980). We need to know only which surfaces get the *same* illumination. That is the idea of a framework: a group of surfaces sharing a common illumination level. If you can segregate a group of patches in the retinal image that are standing in a common illumination, then, having held illumination constant within that group, any further differences have to be reflectance differences.

We need to distinguish this notion of grouping from the familiar idea of perceptual grouping from gestalt psychology. Wertheimer (1923) talked about the need to organize the retinal image in order to segregate objects. As you can see in figure 8, there are two parts of the image that must be grouped together to make a book. This might be called grouping by reflectance. But we can also talk about grouping by illumination. To compute the lightness of the book, we need to group the two regions in higher illumination, or the two regions in shadow.

So we have two kinds of perceptual grouping. And they both follow the gestalt laws of grouping. And they are complementary. And this is important for the crucial question of how regions of common illumination are found within the retinal mosaic. If you can group the two halves of the book together to create the percept of a gray book on a white background,

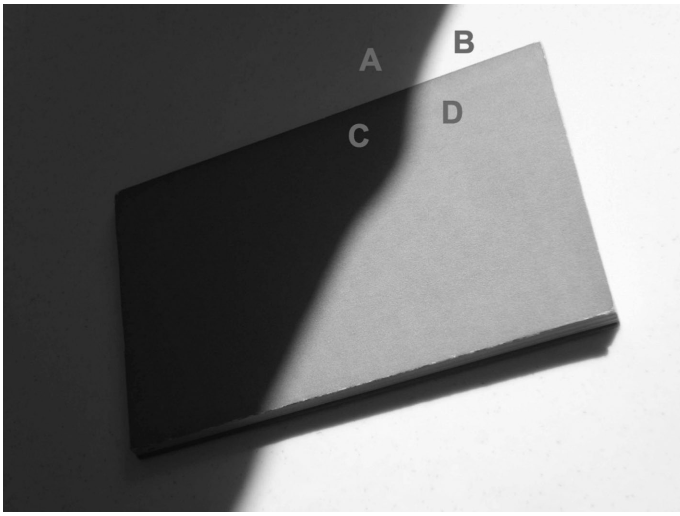

Figure 8. To segregate a book from the indifferent retinal mosaic, regions C and D are perceptually grouped together, as Wertheimer (1923) suggested, using factors including the good continuation along its edge. But to compute the lightness of the book, regions of common illumination are perceptually grouped. Thus C and A are grouped together, as are regions B and D, using the same grouping factors. The ratio-invariant properties of the X-junction specify that an illumination border is crossing a reflectance border. Thus, the segregation of the illumination frameworks is part and parcel of the grouping responsible for perception of a book.

then the orthogonal grouping gives you the two illumination frameworks. Note that there are two X-junctions, one where the boundary of the shadow crosses each edge of the book. These X-junctions have the property of ratio-invariance; the luminance ratio along each of the two edges is preserved in crossing the junction. This does not happen when an X-junction is created by two intersecting illumination edges—there the difference is preserved, not the ratio. The ratio-invariance reveals that there are two different kinds of edges intersecting: a reflectance edge and an illumination edge. Grouping the two parts of the book together implies a reflectance edge. Thus the other edge must be an illumination edge, and hence it must be a boundary that encloses a region of common illumination.

Framework models have strengths and weaknesses as well. They explain constancy and failures of constancy. And they have rather successfully been applied to lightness illusions. They have several weaknesses. For example, the anchoring model in its current form makes no mention of perceived illumination, surely necessary for a complete theory of lightness.

If you want to read more about the failures of anchoring theory, I have a section in my book (Gilchrist, 2006) called “Shortcomings of the theory”. And by the way, I want to recommend that everybody include such a section in his or her publications.

Now as for illumination perception, it turns out that it will probably be quite easy to incorporate that into the model. In work I have been doing with Alessandro Soranzo, the observer views two windows and the illumination level can be independently controlled in each of the windows. Subjects are asked to set the illumination levels in the two windows to be equal. We find that subjects clearly match the two windows for highest luminance, not for average luminance. This contradicts several authorities, such as Helmholtz (1866/1924) and Katz (1935), who suggested that perceived illumination is based on average luminance. But it is consistent with work on perceived illumination by Kozaki (1973) and Noguchi and Masuda (1971).

8.8 *Framework segregation*

So what are the factors in the retinal image that enable the visual system to find these frameworks of illumination? Consistent with Kardos (1934), I would suggest three factors—namely, cast edges, attached edges, and occlusion edges. I do not mean to suggest that the framework segmentation problem is easy. It is a thorny and very challenging problem. But I want to emphasize that this is not just a problem for anchoring theory. Most theories face this problem, although this is not always recognized. Consider Helmholtz, who proposed that we take the illumination into account. Helmholtz was thinking about a change of illumination over time. But the real problem involves changes of illumination over space. In figure 9 we see a scene that includes three different regions of illumination. If you are Helmholtz and you are going to estimate the illumination, you have to estimate a different level of illumination in each of those regions. And that means you have to find out where the regions are. So this is not just a problem for anchoring theory.

Figure 9. Three ellipses of identical luminance have been pasted onto this photograph. They appear white, gray, and black depending upon which framework of illumination they appear in.

8.9 *Hypercompression*

Another challenge for anchoring theory has been the problem of hypercompression. It goes like this. In the theory, the compression of the five squares has been explained by global anchoring; and if there were only local anchoring, the five squares would be seen veridically. However, based on the global framework only, the squares should all look like white surfaces. And it is this horizontal line, the equal lightness values of the squares, that, when combined with the local values, results in compression. But this means that if, as in our experiments, the spotlight is thirty times brighter than the room light (such that the luminance of a black in the spotlight equals that of a white in the room), the compression is maxed out. Making the spotlight brighter than that should not produce any further compression. But empirically it does. Compression continues to increase in proportion to spotlight intensity.

We recently tested this. First we ran the basic staircase Gelb condition in which the illumination on the squares was thirty times that of the ambient illumination. Then we ran two additional conditions one in which we increased the spotlight intensity by a factor of four and one in which we reduced the room illumination by a factor of four. In both the two latter conditions we got substantially more compression than in the baseline condition. It is ironic that anchoring theory fails to explain compression with a brighter spotlight given that anchoring theory was designed, first and foremost, to explain compression in the staircase Gelb effect.

So to explore this problem, we built a new vision tunnel. The interior was covered with either a high luminance range checkerboard (white to black) or a low range checkerboard (white to gray). And the five squares could have either a high range (black to white) or low range (middle gray to white). This allowed us to test six different suggested metrics that might be the cause of the compression. For example, David Brainard has suggested that compression results from a high overall luminance range in the visual field. Well, we got a clear winner. The compression was predicted by the ratio between the highest luminance in the five squares and the highest luminance in the tunnel. That is not what anchoring theory says. According to the theory, you do codetermination for each one of the squares. And that model failed. The only metric that predicted the amount of compression, and did so very well, was the ratio R/F, where R and F refer to the highest luminances of Kardos's 'relevant' and 'foreign' frameworks, respectively. Notice that this ratio is a surrogate for the perceived illumination difference between the spotlight and the rest of the room, given that we know that perceived illumination depends on the highest luminance (Kozaki, 1973; Noguchi & Masuda, 1971).

So to summarize, both framework models and layer models have important strengths. And even though my current work occurs within a framework model, I believe there is a lot of value in the layer models. I gave up my layer model, somewhat reluctantly, because the data seemed to require it. There are some tests in which a layer model wins out over a framework model. I think it must be possible to integrate these two kinds of models, although it is not obvious at present how this can be done, given that the units of analysis, frameworks versus layers, seem so different. Suggestions are welcome.

9 Conclusions

So to conclude, I argue that sensations do not exist; it is time to abandon this vacuous and unproductive notion. Putative cognitive influences on perception have been greatly exaggerated. Two-stage models, despite long-standing and widespread use, lack support. I agree with Pylyshyn (1999) and Fodor (1983) that vision is modular and not easily penetrated by cognition. The illumination level does not need to be estimated; grouping by illumination is not only sufficient but in fact more consistent with empirical data. And finally, I suggest that framework and layer models should be integrated.

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