“Top-down” effects where none should be found: The El Greco fallacy in perception research

Chaz Firestone & Brian J. Scholl

Yale University

Running Head : The El Greco fallacy

Address for reprints and correspondence : Brian Scholl
Department of Psychology
Yale University
Box 208205
New Haven, CT 06520-8205

Email : brian.scholl@yale.edu

Phone/Fax : 203-432-4629 (Phone), -7172 (Fax)

Word Count : 3996

Revision : 3/7/13 — In press, *Psychological Science*

Abstract (120 words)

A tidal wave of recent research purports to have discovered that higher-level states such as moods, action-capabilities, and categorical knowledge can literally and directly affect what we see. Are these truly effects on perception, or might some instead reflect influences on judgment, memory, or response bias? Here, we exploit an infamous art-historical reasoning error (the so-called "El Greco fallacy") to demonstrate in five experiments that multiple alleged top-down effects (ranging from effects of morality on lightness perception to effects of action capabilities on spatial perception) cannot truly be effects on perception. We suggest that this error may also contaminate several other varieties of top-down effects, and that this discovery has implications for debates over the (dis)continuity of perception and cognition.

Keywords

Cognitive penetrability, modularity, top-down effects, spatial perception, lightness perception

What determines what we see? According to a traditional view of human visual perception, the processes responsible for computing basic visual properties such as the lightness of a colored patch or the spatial layout of a room proceed without any direct influence from 'higher-level' cognitive states — for example from knowledge about the world, desires for how we'd like the world to be, or the ability to act on the world (Pylyshyn, 1999). On this view, many aspects of visual processing are driven largely or only by the patterns of light striking the eyes, and are thus "informationally encapsulated" (Fodor, 1983) and "cognitively impenetrable" (Pylyshyn, 1984).

This view of visual perception is motivated by at least two related and ubiquitous sorts of evidence. First, many models of visual processes have been developed over many decades that capture human performance across a wide array of situations without appealing to "top-down" effects. In such cases, the addition of higher-level factors to such models has simply been unnecessary to account for observed behavior. Second, examples of cognitive impenetrability abound in visual experience. Consider that every case wherein a visual illusion persists despite conflicting beliefs or desires just *is* a failure of higher-level factors to penetrate visual processing. (Such persistence may even be a defining feature of visual illusions.)

Despite these forms of evidence and the "modular" view of vision they support, a tidal wave of recent research purports to have discovered countless circumstances in which otherwise-extraperceptual states penetrate visual processing to literally and directly affect what we see. For example, it has been reported that wearing a heavy backpack makes hills looks steeper (Bhalla & Proffitt, 1999); that learning color/letter associations makes the letters appear tinged with that color (Goldstone, 1995); that holding a wide rod makes potentially passable apertures look narrower (Stefanucci & Geuss, 2009); and that reflecting on negative words or actions literally makes the world look darker (Banerjee et al., 2012; Meier et al., 2007). These and many dozens of similar reports offer a contrasting view of what perception is and how it works, according to which beliefs, desires, moods, and abilities play direct "top-down" roles in shaping what we see.

Evaluating Top-Down Effects on Perception

A central challenge in evaluating this emerging literature is to determine whether its reported effects are truly effects on perception (in which case they may well have the profound consequences they advertise), or whether they are effects only on perceptual judgments, memories, or responses, in ways that lie outside visual processing itself (in which case they may not refute the cognitive impenetrability of vision, though they may still be interesting for other reasons). Many previous claims for the penetrability of visual processing have been found wanting in this respect. In particular, a wave of such claims in the middle of the last century — collectively known as the "New Look" movement — foundered for exactly these sorts of reasons (Erdelyi, 1974; McCurdy, 1956). For example, initial claims that poorer children perceived coins as larger than richer children did (e.g. Bruner & Goodman, 1947) were later found to instead reflect biases in memory rather than in perception (e.g. Carter & Schooler, 1949).

Similarly, methodological critiques have been levied against several recent claims of top-down penetration of visual processing. For example, Durgin et al. (2009) replicated the study finding increased slope estimates for backpack-wearers, but then showed that this effect was due to task demands: when backpack-wearing subjects were given a compelling cover story justifying the backpack's presence (involving the need for heavy equipment to record ankle-flexion signals), the effect disappeared — and even in the initial replication, the effect only appeared in those subjects who both correctly guessed the experimenter's intentions (i.e. to see if backpacks affect slant estimates) and themselves predicted such an effect in their own performance (see also Durgin et al., 2010, 2012).

Even for studies with no such methodological problems, though, we suggest that the evidence adduced for top-down penetration of visual processing is frequently incomplete, in an important and particular way. In general, the predictions that can be used to test experimental hypotheses can be crudely divided into two types: First, you should observe an effect when your theory calls for it; second, you should *not* observe an effect when your theory demands its absence. Although both kinds of evidence can be independently decisive, it is perhaps unsurprising that the vast majority of empirical studies claiming top-down effects on perception fall squarely into the first category: some hypothesis is put forth that an otherwise-extraperceptual state can affect perceptual processing, and then such an effect is observed. In this paper, we explore the second category of evidence. We show how testing predictions about when top-down effects must *not* occur according to their theories can help adjudicate disputes over the relationship between perception and cognition. Our particular research strategy in this vein is especially well illustrated by the art-historical reasoning error that inspired it: the so-called "El Greco fallacy".

The El Greco Fallacy

Famously, the Spanish Renaissance artist El Greco painted subjects with oddly elongated figures. In works such as *Saint John the Baptist*, *The Repentant Magdalen*, and even a self-portrait, for example, the main figures inexplicably appear unusually long and thin (Figure 1). Art historians had long puzzled over the meaning and origin of this idiosyncratic style, but in the early 1900s, a simple explanation was advanced: perhaps El Greco suffered from uncommonly severe astigmatism (an ocular defect in which the cornea is slightly ellipsoidal instead of spherical; see Figure 2), which distorted his perceived environment as if by vertically 'stretching' it. If El Greco experienced a vertically stretched-out world, it was reasoned, then perhaps he simply painted what he saw.

Careful reflection on this theory reveals a conceptual confusion: if El Greco truly experienced a stretched-out world, then he would also have experienced a stretched-out *canvas*. In that case, the distortions should have 'canceled out': just as El Greco would have seen real-word figures as elongated, so too would he have seen his paintings as elongated, and so the real-world distortions he experienced would never have transferred to his reproductions. The distortions in El Greco's paintings, then, *must* have some alternative explanation beyond a literal perceptual distortion. Thinking otherwise has come to be known as the "El Greco fallacy" (e.g. Rock, 1966; see also Anstis, 2002).

The Current Experiments

Here, we apply the logic of the El Greco fallacy to alleged top-down effects on perception by exploiting the fact that distortions must 'cancel out' when the means of reproduction would be distorted in just the same way as the stimulus being reproduced. We used this logic to demonstrate that multiple prominent top-down perceptual effects obtain even when they shouldn't, and therefore cannot truly be effects on perception. In particular, we demonstrate instances of the El Greco fallacy in the context of alleged effects of action-capabilities on spatial perception (Experiments 1-3) and of morality on lightness perception (Experiments 4-5). These two case studies were chosen for their heterogeneity: both allege top-down influences on perception, but in very different contexts. Of course, our primary goal is not to criticize these particular studies; indeed, we suggest in the General Discussion that this error also threatens several other varieties of top-down effects. Rather, we aim to offer a proof-of-concept, demonstrating how this research strategy offers a new approach to questions about how perception and cognition do (and do not) interact.

Experiment 1: An Influence of Action-Capability on Spatial Perception?

The first case study involves a recent empirical report that holding a lengthy rod across one's body (Figure 3a) makes apertures look narrower — supposedly because doing so makes apertures less passable (Stefanucci & Geuss, 2009). This finding is one of several dozen similar empirical studies that have fueled a rich and highly influential research program claiming ability-based effects on spatial perception (for reviews see Proffitt, 2006; Witt, 2011). We first replicated this basic effect.

Method

Participants

20 members of the Yale University community participated in exchange for course credit or monetary reimbursement.

Apparatus

The experiment was conducted in a 5.25m x 4.65m testing room. The floor was covered in black felt, and the walls were covered from floor-to-ceiling in brown builder's paper. One of two apertures was used for each subject, consisting of two 159cm-tall poles with free-standing circular bases. One set of poles was 2.54cm thick with felt-covered bases 10.16cm in diameter; the other set was 0.64cm thick with exposed metal bases 8.89cm in diameter. There was 1.5m of clearance between the aperture and the nearest parallel wall, and an 'X' on the floor indicated the subject's standing position, 2.5m from the aperture. A wooden rod held by some subjects (3.18cm diameter, 114.3cm long) had two strips of black duct tape (5cm wide) on each end for grips. A 16′ retractable measuring tape was used to obtain aperture-width estimates.

Procedure and Design

Subjects were randomly assigned to either hold a rod (Figure 3a) or not, and to view one of the two apertures. In the Rod condition, subjects were shown the rod and told they would hold it throughout the session. No explanation of the rod's purpose was given (though see Experiment 3).

Subjects were told to get comfortable by first walking once around the testing room's perimeter (as in Stefanucci & Geuss, 2009), after which they estimated the width of an aperture on each of 35 trials — one trial each for 7 aperture-widths (76.2cm, 88.9cm, 101.6cm, 114.3cm, 127cm, 139.7cm, 152.4cm), repeated in different random orders within each of 5 blocks. On each trial, subjects were instructed to stand with shoulders square to the aperture, and then to imagine walking through it without turning their shoulders (a simulation thought to induce action-based perceptual 'scaling'; Stefanucci & Geuss, 2009; Witt & Proffitt, 2008). (To be extra sure that such scaling would occur, subjects were also told at the beginning of the session that they would actually walk through the aperture at some point during the experiment — though in fact this never occurred.) Immediately after the imagination task, subjects were instructed to turn 90° to their right. There, an experimenter stood 2m away, holding a measuring tape (Figure 3b). The experimenter slowly drew out the tape (not yet looking at the markings, which faced away from subjects) until subjects (who could still freely view the aperture) indicated that the tape's length "visually matched" the aperture's width. The experimenter encouraged subjects to request minor adjustments until they were satisfied with the match. The experimenter then recorded the estimate, and subjects faced a back wall while the aperture was repositioned for the next trial.

Results and Discussion

Subjects who held the rod judged the aperture to be narrower than did subjects who did not hold the rod (105cm vs. 112cm, $t(18)=2.57$, $p<.02$, all tests two-tailed; $d=1.212$), replicating the findings of Stefanucci and Geuss (2009).

Experiment 2: Applying the El Greco Fallacy

Experiment 1 confirmed that holding a rod decreased aperture-width estimates. Does this reflect a literal perceptual compression of apertures? Experiment 2 tested this by replicating Experiment 1 with one simple change: instead of a measuring tape, the "measuring device" manipulated by the experimenter was *itself* a potentially passable aperture. Subjects judged the length of the aperture (hereafter the "stimulus aperture") as before, but when they turned 90° degrees to the right, they saw a second adjustable aperture (hereafter the "matching aperture") and instructed the experimenter to widen or narrow the matching aperture until the two apertures looked to be the same width. If holding a rod really does perceptually compress apertures, then this variant should 'fail', because subjects should see *both* apertures as narrower.

Thus, if holding a rod still decreases width estimates, then this effect cannot reflect literal perceptual compression of apertures, and must be explained by non-perceptual factors (as verified in Experiment 3).

Method

This experiment was identical to Experiment 1 except as follows. 20 new subjects participated. Where an experimenter stood in Experiment 1 with a measuring tape, the matching aperture now appeared (2m away, with 1.5m of clearance to the nearest parallel wall) — with random assignment of which set of poles served as the stimulus vs. matching aperture. (When the matching aperture was described before the session began, it was called a "measuring device" — as was the measuring tape in Experiment 1). After imagining walking through the stimulus aperture, subjects turned 90º and imagined walking through the matching aperture. Subjects then instructed the experimenter to adjust the matching aperture, just as with the measuring tape in Experiment 1 (Figure 3c).

Results and Discussion

As in Experiment 1, subjects who held rods judged the stimulus aperture to be narrower (111cm vs. 106cm, $t(18)=2.33$, $p<.04$; $d=1.095$). But unlike Experiment 1, this result cannot be an effect on the perception of apertures — for if it were, there should have been no effect at all, per the El Greco fallacy. If holding the rod really made apertures look narrower, it should have made *both* apertures look narrower, and the effects should have 'canceled out'. That this did not happen entails that some other factor caused the decreased width estimates.

Experiment 3: So What Does Explain Aperture-Compression Effects?

If the aperture-compression effects (which are real and replicable) cannot reflect an influence on perception per se, then what explains them? Note that *this question does not have to be answered* to repel the challenge to cognitive impenetrability: we can conclude that the effect does not reflect literal perceptual compression, even if we remain uncertain about the effect's true, nonperceptual origin. This is worth emphasizing, since — in contrast to other research strategies — applying the El Greco fallacy relieves investigators of the burden of generating and testing various alternative hypotheses. (And in fact, we deliberately do not even attempt this with our second case study in Experiments 4-5.) For this first case study, however, we sought to provide an empirically supported positive explanation of the aperture-width effects, to reinforce the "El Greco" strategy's verdict on the perceptual vs. nonperceptual nature of such effects.

As it happens, previous research has implicated experimental demand characteristics as the explanation for related "top down" effects on spatial perception (Durgin et al., 2009). Such factors could also fuel the results of Experiments 1 and 2: perhaps subjects simply guessed the purpose of the (conspicuously unexplained) rod, and responded accordingly. If so, then the aperture-width effects should disappear when subjects believe the rod is being held for some other purpose — as provided by a compelling (but incorrect) cover story. The present experiment tested this possibility.

Method

This experiment was identical to Experiment 1 except as follows. 10 new subjects participated, all holding the rod. Whereas subjects in Experiment 1 received no information about the rod's purpose, subjects in this experiment were told explicitly that the rod was meant to improve their balance — as when stabilizing poles aid tightrope walkers during their stunts. Subjects were still instructed to imagine walking through the aperture (though here that instruction may have carried the implication that they should focus on their improved balance rather than their inability to pass through the aperture). To add to the cover story's plausibility, the experimenter also pretended to carefully choose the rod from a salient array of differently sized rods in the room, and it was explained that the researchers were testing poles of different sizes. (In fact, the same rod from Experiment 1 was chosen for each subject.)

Results and Discussion

The (rod-holding) subjects' width estimates did not differ from those of Experiment 1's subjects, who held no rod at all (111cm vs. 112cm, $p>0.65$; $d=0.194$). This supports a nonperceptual explanation for the aperture-width effects (as mandated by the El Greco fallacy), realized here in terms of demand characteristics.

Experiment 4: An Effect of Morality on Perceived Lightness?

To showcase the versatility of the El Greco strategy, the second case study presented here involves a different experimental method used to study effects of a different 'higher-level' state on a different perceptual property. In particular, we focus on a recent finding that reflecting on unethical (rather than ethical) deeds from one's past lowers estimates of lightness (Banerjee et al., 2012), as if thinking 'darker' thoughts literally makes the world look darker. We first sought to replicate this effect.

Method

Participants

89 subjects were recruited online through Amazon Mechanical-Turk and were monetarily reimbursed. Data from seven subjects who failed to follow instructions were excluded (without analysis).

Materials and Procedure

Subjects were randomly assigned to describe in writing an ethical or unethical action from their past, including details of the emotions they experienced in connection with this action. As a distracting task, they then completed four true/false math questions (e.g. (4x7)–6=24?). Finally, subjects used a scale from 1 ("Low") to 7 ("High") to rate the brightness of the room they were in (wherever that happened to be).

Results and Discussion

Subjects who described an unethical deed judged their room to be darker than did those who described an ethical deed (3.72 vs. 4.19). Though only marginally significant (t(80=1.73, p=.088; d=0.386), this trend encouraged us to attempt a replication applying the El Greco fallacy.

Experiment 5: Applying the El Greco Fallacy

Is the effect of ethical reports on lightness judgments truly perceptual? This experiment replicated Experiment 4, except that the 7-point numerical-report scale was replaced with 7 grayscale patches (Figure 4), and subjects simply picked the patch that best matched the lightness of the room they were in. If reflecting on unethical deeds really makes stimuli look darker, then this variant should 'fail': the walls of the room should look darker, but the patches should look darker as well, and so these two factors should 'cancel out'.

Method

This experiment was identical to Experiment 4 except as follows. 91 people participated. Data from two subjects who failed to follow instructions were excluded (without analysis). The report scale's points were actual grayscale patches ranging from 50% gray to 7.14% gray, with 5 linear intermediate steps: 42.86%, 35.71%, 28.57%, 21.43%, 14.29%. Subjects were instructed to "pick the patch that best matches the room's brightness". Though of course we cannot report the patches' luminance values as subjects saw them, any variance introduced by each subject's home monitor would be uniform across conditions.

Results and Discussion

Subjects who recalled unethical deeds judged the room to be darker than did subjects who recalled ethical deeds (25.93% vs. 20.64% gray, $t(87)=2.13$, $p<.04$; $d=0.458$). This effect, then, cannot be perceptual: if stimuli look darker after recalling unethical deeds, the scale's patches themselves should have looked darker too, and the effects should have 'canceled out'. That they did not suggests that the underlying cause of this effect is something other than the literal perceptual darkening of the environment.[1]

General Discussion

Across two rather different case studies and five experiments, the El Greco strategy was exploited to rule out perceptual interpretations of putative top-down effects on perception. The underlying logic of the El Greco strategy is simple: when a constant-error distortion should affect equally the means of reproduction and the item reproduced, the effects should cancel out. (Note that this does not apply to distortions involving information *loss*. For example, it would not be fallacious to suggest that Monet made blurry paintings because of cataracts that blurred his vision.)[2] We appropriated this logic to demonstrate that certain reported 'distortions' in perception of space or lightness are exactly like the distortions in El Greco's paintings. They are real and reliable effects, but just as the distortions in El Greco's paintings cannot be explained by his literally seeing elongated figures, so too the explanation for these "top down" effects cannot be that apertures literally look narrower or that the world literally looks darker.

Additional Examples

In its art-historical context, the El Greco fallacy is famously counterintuitive. We think this applies in the present context as well. Indeed, we think the El Greco fallacy is so counterintuitive that several prominent and influential studies of alleged top-down effects on

---

[1] One alternative interpretation is that, despite instructions to pick the patch that best matched the room, subjects instead picked patches for their scale positions rather than for their perceived lightness. However, this possibility is ruled out by examining mean responses for the different scales used in Experiments 4 and 5. Collapsing over Ethical and Unethical conditions (and translating %-gray responses into equivalent numerical scale-points), subjects in Experiment 4 (who used the numbered scale) gave reliably lower responses than did subjects in Experiment 5 (who used the grayscale-patch scale; 3.96 vs. 4.75, $t(169)=3.50$, $p<.001$; $d=0.539$).

[2] Nor does the El Greco fallacy apply to Dilks et al.'s (2007) fascinating stroke patient who experienced vertical elongations in his lower-left visual field, as if he were a real-life sufferer of what might be called 'El Greco syndrome'. For in this case, the distortions were measured using (inter alia) pairwise comparisons of stimuli presented in the affected and unaffected quadrants, and so the 'means of reproduction' were *not* affected in the same way as the item 'reproduced'.

perception appear to have committed it themselves, by employing experimental designs that inadvertently set up conditions similar to the present experiments.

For example, consider a report that after repeatedly viewing one set of letters variously colored red and violet and a second set of numbers variously colored blue and violet, subjects judged token violet letters to look redder than they truly were and token violet numbers to look bluer than they truly were — as if the perceived hues of the violet letters and numbers were 'pulled' toward their respective category's mean hue (Goldstone, 1995). This effect was measured by having subjects adjust the hue of a stimulus until it perceptually matched the letter or number being tested. The trouble is that, in this study, the adjusted stimulus was a copy of the symbol being tested. For example, after repeatedly viewing a red 'T', a reddish-violet 'E', and a violet 'L', subjects judged the L to be slightly redder than it really was — *as measured by adjusting the hue of a second L*! This appears to be an instance of the El Greco fallacy: if Ls really look redder after seeing other red letters, then *both* the 'stimulus L' and the 'matching L' (to borrow terminology from Experiment 2) should have looked redder, and the effects should have canceled out. That such an effect was nevertheless obtained suggests that it cannot be perceptual.

Similarly, consider a report of the following pair of results (Meier et al., 2007): (1) subjects judged gray patches to be darker after reading negative words than after reading positive words; (2) subjects judged words printed in gray ink to be darker if the words were negative than if they were positive, as measured by selection of a darker grayscale patch when subjects had to choose a patch that matched the word's lightness. This also appears to commit the El Greco fallacy: if reading negative words really makes patches look darker (per the first result), then the patches from the second result should have looked darker as well, and the effects should have canceled out. This effect too, then, cannot truly be perceptual.

Conclusions

The use of the El Greco fallacy in the present study is a particular example of a distinctly unpopular strategy that can nevertheless effectively test "top down" effects on perception: such effects should obtain when predicted, but they also should *not* obtain when their motivating theories demand they mustn't. One reason for this general strategy's unpopularity may be that, until now, most implementations would have required reporting null effects. But not so with the El Greco strategy: here the point is made with positive replications of the (real and reliable) effects in question. And as another distinct advantage, this strategy can adjudicate between perceptual vs. nonperceptual interpretations of such effects without needing to specify any particular nonperceptual explanation. We thus hope that the El Greco strategy as employed here may be generally applicable to foundational debates over the (dis)continuity of perception and cognition.

References

Anstis, S. (2002). Was El Greco astigmatic? Leonardo, 35, 208.

Banerjee, P., Chatterjee, P., & Sinha, J. (2012). Is it light or dark? Recalling moral behavior changes perception of brightness. Psychological Science, 23, 407-409.

Bhalla, M., & Proffitt, D. R. (1999). Visual-motor recalibration in geographical slant perception. Journal of Experimental Psychology: Human Perception and Performance, 25, 1076-1096.

Bruner, J. S., & Goodman, C. C. (1947). Value and need as organizing factors in perception. Journal of Abnormal and Social Psychology, 42, 33-44.

Carter, L. F., & Schooler, K. (1949). Value, need, and other factors in perception. Psychological Review, 56, 200-207.

Dilks, D. D., Serences, J. T., Rosenau, B. J., Yantis, S., & McCloskey, M. (2007). Human adult cortical reorganization and consequent visual distortion. Journal of Neuroscience, 27, 9585–9594.

Durgin, F. H., Baird, J. A., Greenburg, M., Russell, R., Shaughnessy, K., & Waymouth, S. (2009). Who is being deceived? The experimental demands of wearing a backpack. Psychonomic Bulletin & Review, 16, 964-969.

Durgin, F. H., Hajnal, A., Li, Z., Tonge, N., & Stigliani, A. (2010). Palm boards are not action measures: An alternative to the two-systems theory of geographical slant perception. Acta Psychologica, 134, 182-197.

Durgin, F. H., Klein, B., Spiegel, A., Strawser, C. J., & Williams, M. (2012). The social psychology of perception experiments: Hills, backpacks, glucose and the problem of generalizability. Journal of Experimental Psychology: Human Perception and Performance, 38, 1582-1595.

Erdelyi, M. H. (1974). A new look at the new look: Perceptual defense and vigilance. Psychological Review, 81, 1-25.

Fodor, J. A. (1983). The modularity of mind: An essay on faculty psychology. Cambridge, MA:

MIT Press.

Goldstone, R. L. (1995). Effects of categorization on color perception. Psychological Science, 6, 298-304.

McCurdy, H. G. (1956). Coin perception studies and the concept of schemata. Psychological Review, 63, 160-168.

Meier, B. P., Robinson, M. D., Crawford, L. E., & Ahlvers, W. J. (2007). When “light” and “dark” thoughts become light and dark responses: Affect biases brightness judgments. Emotion, 7, 366-376.

Proffitt, D. R. (2006). Embodied perception and the economy of action. Perspectives on Psychological Science, 1, 110-122.

Pylyshyn, Z. W. (1984). Computation and cognition: Toward a foundation for cognitive science. Cambridge, MA: MIT Press.

Pylyshyn, Z. W. (1999). Is vision continuous with cognition? The case for cognitive impenetrability of visual perception. Behavioral and Brain Sciences, 22, 341-365.

Rock, I. (1966). The nature of perceptual adaptation. New York: Basic Books.

Stefanucci, J. K., & Geuss, M. N. (2009). Big people, little world: The body influences size perception. Perception, 38, 1782-1795.

Witt, J. K. (2011). Action’s effect on perception. Current Directions in Psychological Science, 20, 201-206.

Witt, J. K., & Proffitt, D. R. (2008). Action-specific influences on distance perception: A role for motor simulation. Journal of Experimental Psychology: Human Perception and Performance, 34, 1479-1492.

Author Note

For helpful conversation and/or comments on previous drafts, we thank Frank Durgin, Gary Lupyan, and Bill Warren.

Address correspondence and reprint requests to:

Brian Scholl

Department of Psychology

Yale University

Box 208205

New Haven, CT 06520-8205

brian.scholl@yale.edu

http://www.yale.edu/perception/

Figure Captions

Figure 1. Canonical examples of El Greco's elongated figures.

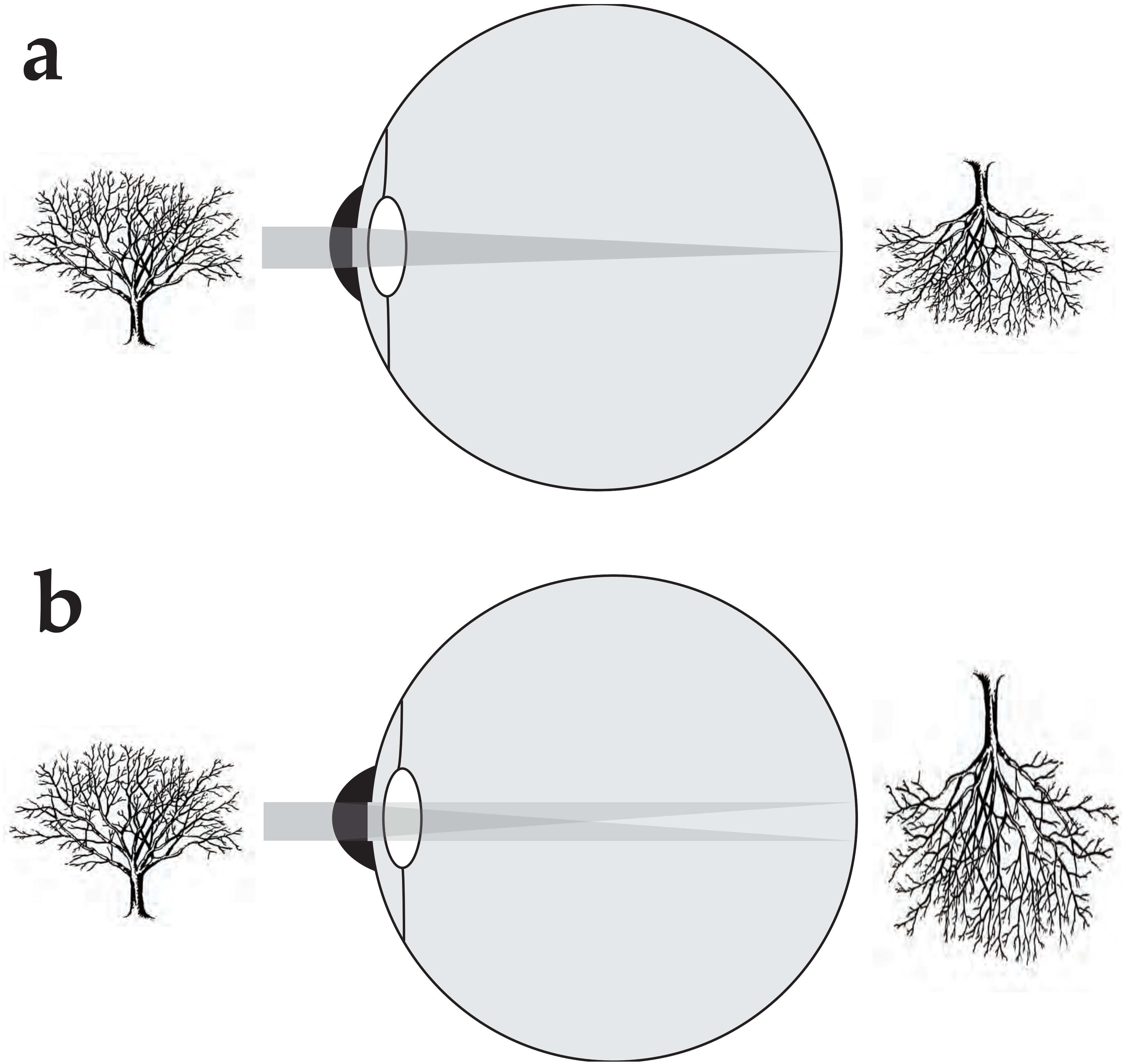
Figure 2. Depiction of the underlying mechanism once thought to explain the elongated figures in El Greco's paintings. (a) A normal eye with an approximately spherical cornea produces a focused image on the retina. (b) An astigmatic eye with an ellipsoidal cornea has multiple focal points, which can produce vertical blurring on the retina.

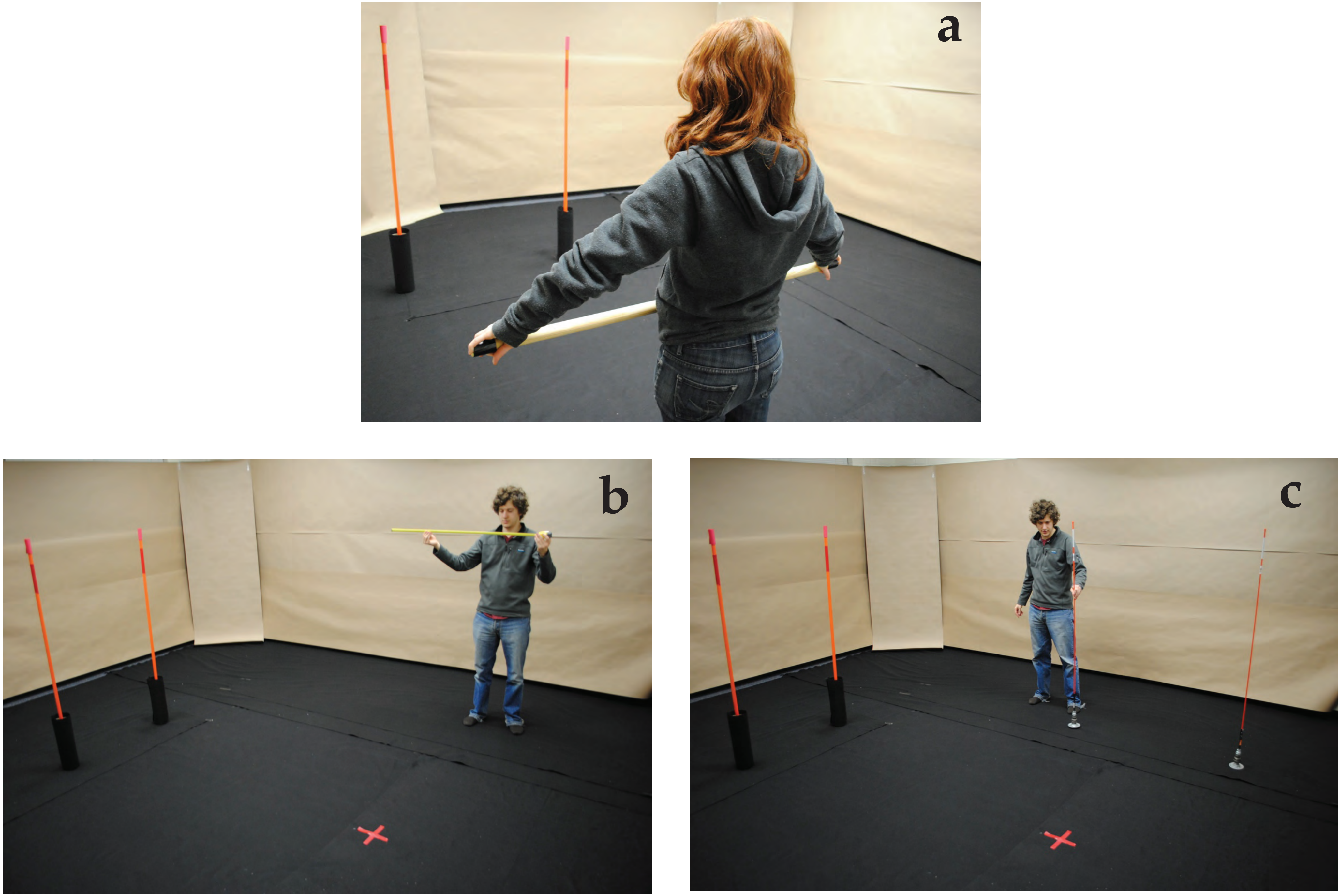
Figure 3. Illustrations of materials from Experiments 1-3. (a) A subject in the Rod condition. (b) A subject's-eye-view of the experimenter during the report phase of Experiment 1 (replicating Stefanucci & Geuss, 2009). (c) A subject's-eye-view of the experimenter during the report phase of Experiment 2 (applying the El Greco strategy).

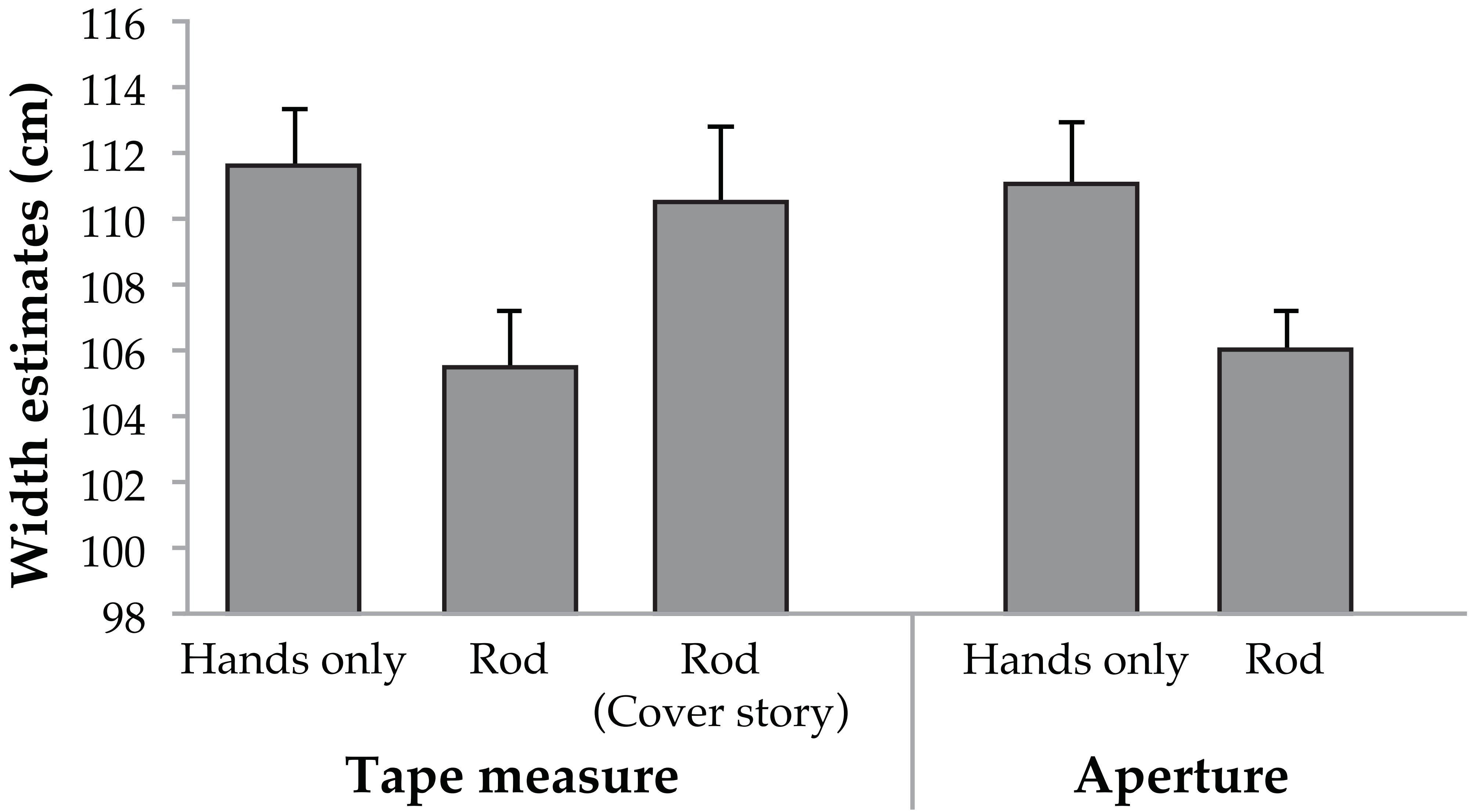
Figure 4. Results from Experiments 1-3, applying the El Greco strategy to effects of passing ability on perceived aperture width. See text for details.

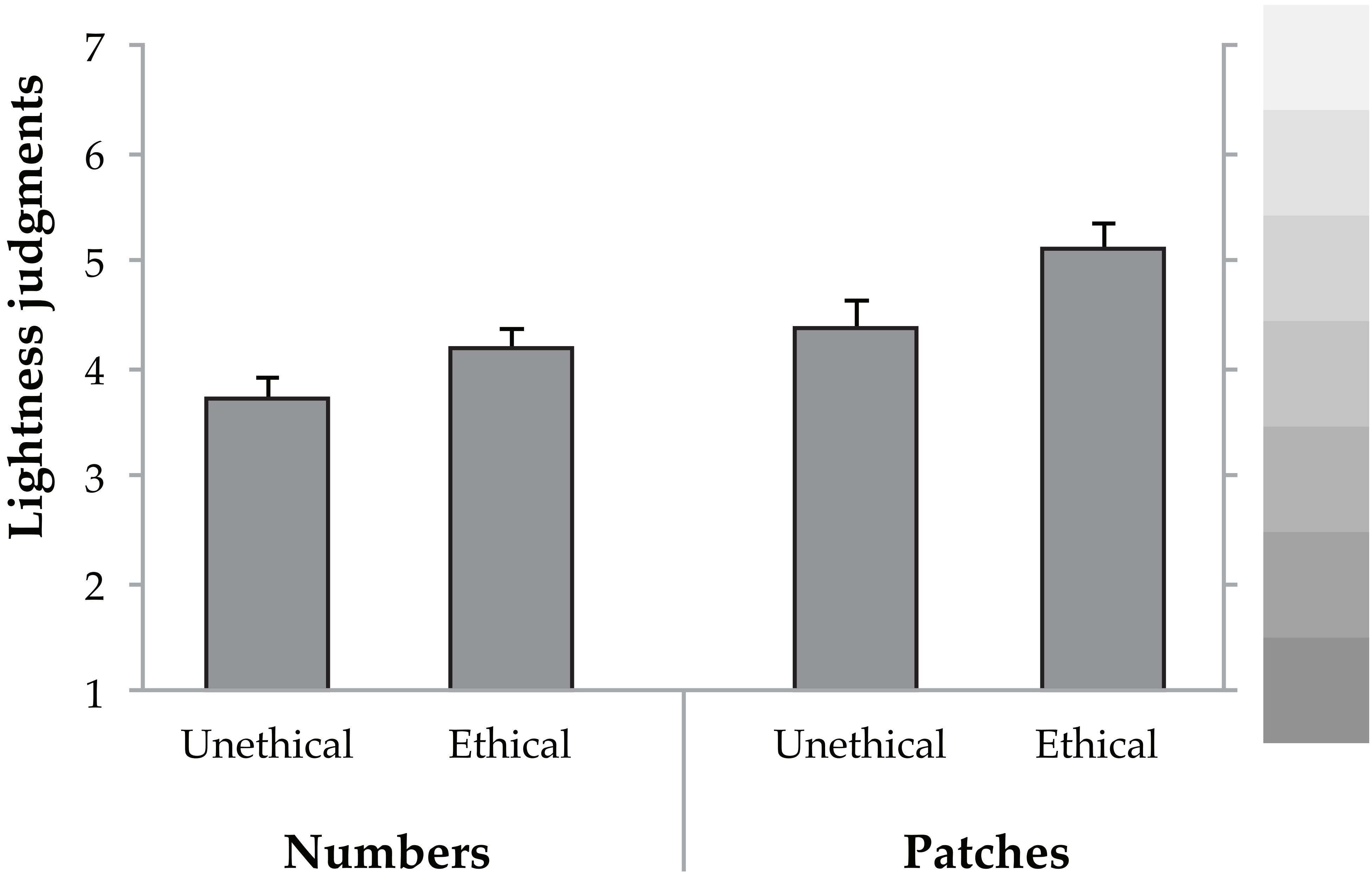
Figure 5. Results from Experiments 4-5, applying the El Greco strategy to effects of morality on perceived lightness. See text for details.




a
b


a
b
c


Width estimates (cm)
116
114
112
110
108
106
104
102
100
98
Hands only
Rod
Rod
(Cover story)
Tape measure
Hands only
Rod
Aperture


Lightness judgments
7
6
5
4
3
2
1
Unethical
Ethical
Unethical
Ethical
Numbers
Patches