Research Article

Psychological Science
2014, Vol. 25(1) 38–46

Reprints and permissions:
sagepub.com/journalsPermissions.nav
DOI: 10.1177/0956797613485092
pss.sagepub.com

"Top-Down" Effects Where None Should Be Found: The El Greco Fallacy in Perception Research

Chaz Firestone and Brian J. Scholl
Yale University

Abstract

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 how things look. Are these truly effects on perception, or might some instead reflect influences on judgment, memory, or response bias? Here, we exploited an infamous art-historical reasoning error (the so-called "El Greco fallacy") to demonstrate that multiple alleged top-down effects (including effects of morality on lightness perception and 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 continuity (or lack thereof) of perception and cognition.

Keywords

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

Received 10/22/12; Revision accepted 3/10/13

What determines how things look? 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 the world should 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 in which a visual illusion persists despite conflicting beliefs or desires is inherently 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 how things look. For example, it has been reported that wearing a heavy backpack makes hills look 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

Corresponding Authors:
Chaz Firestone, Department of Psychology, Yale University, Box 208205, New Haven, CT 06520-8205
E-mail: chaz.firestone@yale.edu

Brian J. Scholl, Department of Psychology, Yale University, Box 208205, New Haven, CT 06520-8205
E-mail: brian.scholl@yale.edu

the world look darker (Banerjee, Chatterjee, & Sinha, 2012; Meier, Robinson, Crawford, & Ahlvers, 2007). These and 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 people 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 that found increased slope estimates for backpack wearers (Bhalla & Proffitt, 1999) 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 appeared only in those subjects who both correctly guessed the experimenter's intentions (i.e., to see whether backpacks affect slant estimates) and themselves predicted such an effect on their own performance (see also Durgin, Hajnal, Li, Tonge, & Stigliani, 2010; Durgin, Klein, Spiegel, Strawser, & Williams, 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 article, we explore the second category of evidence. We show how testing predictions about when top-down effects must *not* occur 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 (Fig. 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 Fig. 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 each other 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., Anstis, 2002; Rock, 1966; for a historical account of this reasoning error, see Firestone, 2013b).

The Current Experiments

Here, we applied the logic of the El Greco fallacy to alleged top-down effects on perception by exploiting the fact that distortions must cancel each other 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 occur even when they should not and therefore cannot truly be effects on perception. In particular, we demonstrated instances of the El Greco fallacy

Fig. 1. Canonical examples of the elongated figures painted by Spanish Renaissance artist El Greco. Clockwise from left: *Saint John the Baptist*, ca. 1600, oil on canvas, 111.1 × 66 cm (reprinted with permission from the Fine Arts Museum of San Francisco); *The Repentant Magdalen*, ca. 1577, oil on canvas, 108 × 101.3 cm (reprinted with permission from the Worcester Art Museum); *Portrait of a Man*, ca. 1590–1600, oil on canvas, 52.7 × 46.7 cm (reprinted with permission from the Metropolitan Museum of Art).

in two case studies that allegedly show effects of action capabilities on spatial perception (Experiments 1–3) and effects of morality on lightness perception (Experiments 4 and 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 involved a recent empirical report that holding a lengthy rod across one's body (Fig. 3a) makes apertures look narrower—supposedly because doing so makes apertures less passable (Stefanucci & Geuss, 2009). This finding is from 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; but see also Firestone, 2013a). We first replicated this basic effect.

Method

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

Apparatus. One of two apertures was used for each subject; each aperture was formed by two 159-cm-tall poles with freestanding circular bases set at a variable distance from each other. One set of poles was 2.54 cm

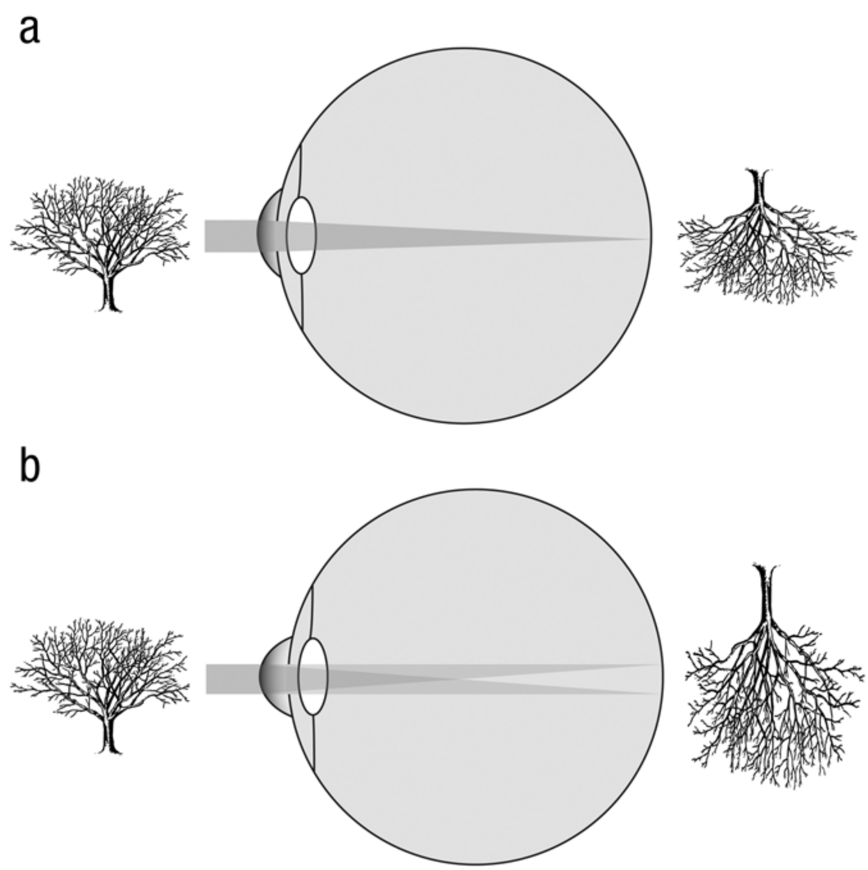

Fig. 2. Depiction of astigmatism, the underlying mechanism once thought to explain the elongated figures in El Greco's paintings. A normal eye with an approximately spherical cornea (a) produces a focused image on the retina. An astigmatic eye with an ellipsoidal cornea (b) has multiple focal points,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

thick with felt-covered bases 10.16 cm in diameter; the other set was 0.64 cm thick with exposed metal bases 8.89 cm in diameter. There was 1.5 m of clearance between the aperture and the nearest parallel wall, and an "X" on the floor indicated the subject's standing position, 2.5 m from the aperture. A wooden rod held by some subjects (3.18 cm in diameter, 114.3 cm long) had two strips of black duct tape (5 cm wide) on each end for grips. A 16-foot retractable measuring tape was used to obtain aperture-width estimates.

Procedure and design. The experiment was conducted in a 5.25 m × 4.65 m testing room. The floor was covered in black felt, and the walls were covered from floor to ceiling in brown builder's paper. Each subject was randomly assigned to either hold a rod (Fig. 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 seven aperture widths (76.2 cm, 88.9 cm, 101.6 cm, 114.3 cm, 127 cm, 139.7 cm, and 152.4 cm), repeated in different random orders within each of five 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, we told subjects 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 2 m away, holding a measuring tape (Fig. 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

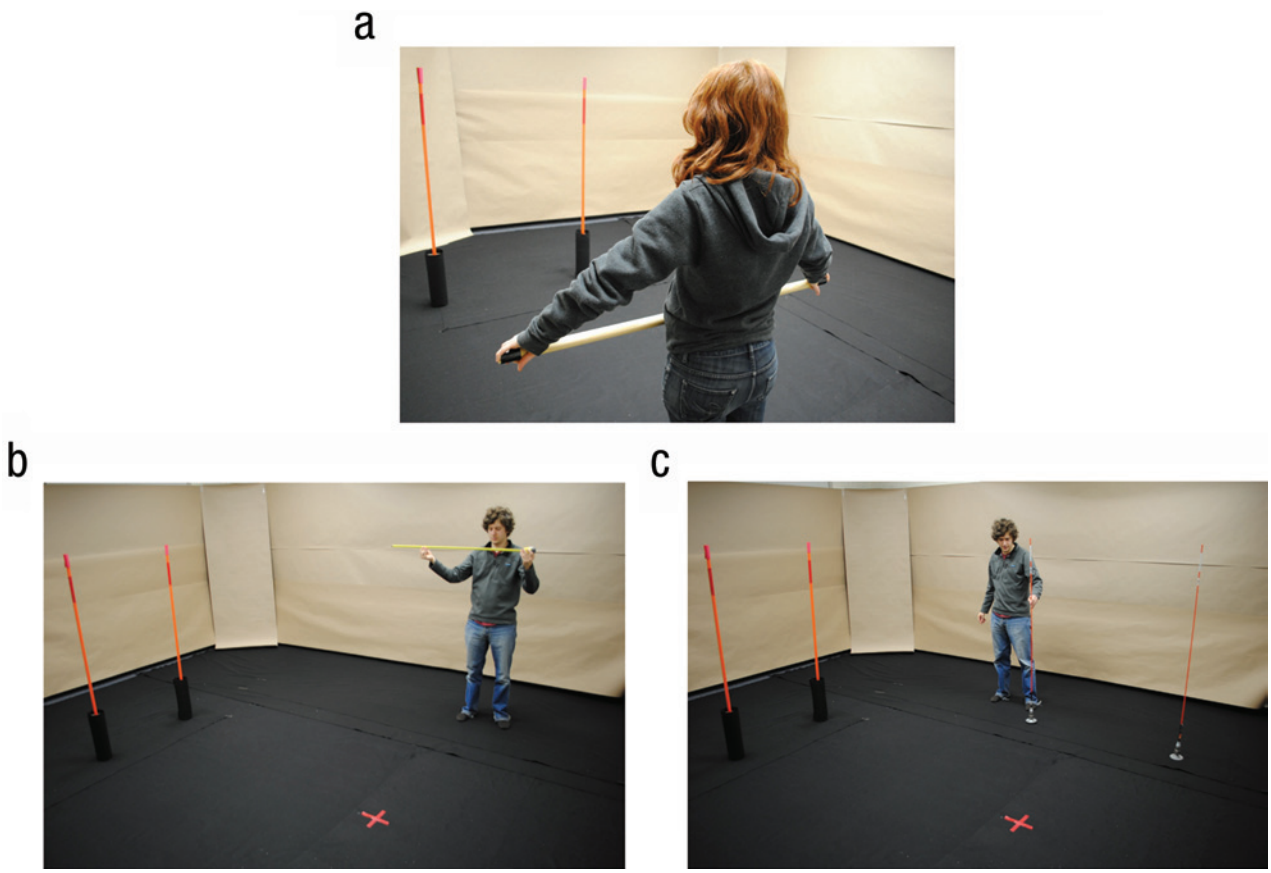

Fig. 3. Photos of materials and procedures used in Experiments 1 through 3. Subjects in the rod condition of all three experiments (a) viewed an aperture while standing with a rod held lengthwise across their bodies. Afterward, the experimenter in Experiments 1 and 3 drew out a measuring tape (b) until subjects indicated that the tape's length visually matched the aperture's width. In Experiment 2, the experimenter adjusted the width between poles forming a second aperture (c) until subjects said it matched the width of the first aperture.

they were satisfied with the match. The experimenter then recorded the estimate, and subjects turned their backs 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 (*M*s = 105 cm vs. 112 cm, respectively), $t(18) = 2.57$, $p < .02$;[1] $d = 1.212$ (see Fig. 4), which replicated 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 possibility 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 width 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 to produce the results of Experiment 1, 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 nonperceptual factors (as verified in Experiment 3).

Method

This experiment was identical to Experiment 1 except as follows. Twenty new subjects participated. Where an experimenter stood in Experiment 1 with a measuring tape, the matching aperture now appeared (2 m away from the subject, with 1.5 m of clearance to the nearest

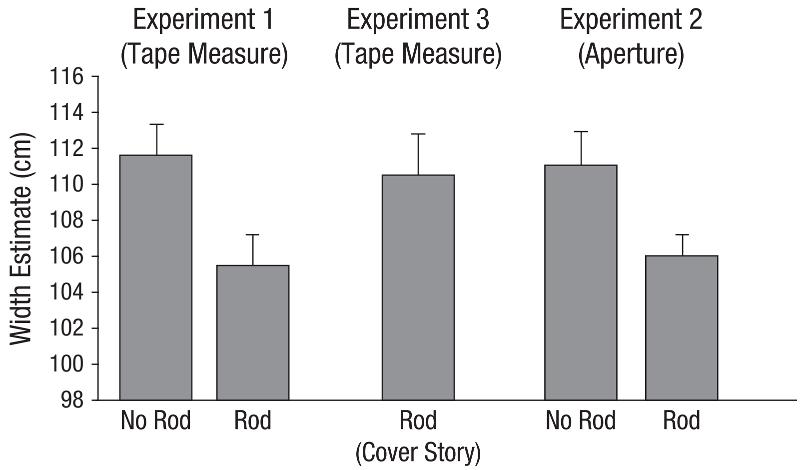

Fig. 4. Results from Experiments 1 through 3: mean aperture-width estimate as a function of whether subjects held a rod while making their estimates. The experimenter used either a measuring tape (Experiments 1 and 3) or a second aperture (Experiment 2) to record the estimates that subjects dictated. In Experiment 3, subjects were given a cover story purportedly explaining the rod's purpose, whereas in Experiments 1 and 2, they were given no information about the rod. Error bars show standard errors.

parallel wall)—with random assignment of which set of poles served as the stimulus versus 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 90° 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 (Fig. 3c).

Results and discussion

As in Experiment 1, subjects who held rods judged the stimulus aperture to be narrower than did those who did not hold rods (*M*s = 106 cm vs. 111 cm, respectively), $t(18) = 2.33$, $p < .04$; $d = 1.095$ (see Fig. 4). But unlike the findings in 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 each other 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) do not 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, because—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 did not even attempt this with our second case study in Experiments 4 and 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 versus 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 have fueled 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. In the present experiment, we tested this possibility.

Method

This experiment was identical to Experiment 1 except as follows. Ten 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 (*M*s = 111 cm vs. 112 cm, respectively), $p > .65$; $d = 0.194$ (see Fig. 4). This finding 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, we conducted a second case study using 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. Eighty-nine subjects were recruited online through Amazon's Mechanical Turk and were monetarily reimbursed for their participation. Data from 7 subjects who failed to follow instructions were excluded from analysis.

Materials and procedure. Each subject was randomly assigned to describe in writing an ethical or unethical action from his or her past, including details of the emotions experienced in connection with this action. As a distractor task, they then completed four true/false math questions (e.g., $(4 \times 7) - 6 = 24$). 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).

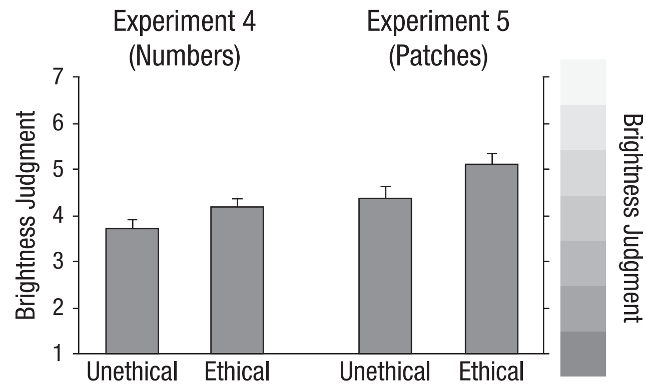

Fig. 5. Results from Experiments 4 and 5: mean rating of room brightness as a function of whether participants wrote about ethical or unethical memories. Brightness ratings were made either on a numerical scale (left *y*-axis) or by choosing from a range of gray-scale patches (right *y*-axis). Error bars show standard errors.

Results and discussion

Subjects who described an unethical deed judged their room to be darker than did those who described an ethical deed (*M*s = 3.72 vs. 4.19, respectively; see Fig. 5). 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 seven gray-scale patches (Fig. 5), 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 to reproduce the effect reported in Experiment 4: The walls of the room should look darker, but the patches should look darker as well, and so these two factors should cancel each other out.

Method

This experiment was identical to Experiment 4 except as follows. Ninety-one people participated. Data from 2 subjects who failed to follow instructions were excluded from analysis. The report scale's points were actual gray-scale patches ranging from 50% gray to 7.14% gray, with five linear intermediate steps: 42.86%, 35.71%, 28.57%, 21.43%, and 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 (*M*s = 25.93% gray vs. 20.64% gray, respectively), $t(87) = 2.13$, $p < .04$; $d = 0.458$ (see Fig. 5). 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 each other out. That they did not suggests that the underlying cause of this effect is something other than the literal perceptual darkening of the environment.[2]

General Discussion

Across two rather different case studies and five experiments, the El Greco fallacy 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 each other 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.[3]) 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 it appears to have been committed in several prominent and influential studies of alleged top-down effects on perception, in which experimental designs were employed 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 one sees 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 each other 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): (a) Subjects judged gray patches to be darker after reading negative words than after reading positive words, and (b) 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 gray-scale patch when subjects had to choose a patch that matched the word's lightness. This pattern of results also implies an 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 each other 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 occur when predicted, but they also should not occur when their motivating theories demand their absence. 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 was made with positive replications of the (real and reliable) effects in question. And, as another distinct advantage, this strategy can help researchers adjudicate between perceptual versus 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 continuity (or lack thereof) of perception and cognition.

Author Contributions

C. Firestone and B. J. Scholl designed the research. C. Firestone conducted the research and analyses. C. Firestone and B. J. Scholl wrote the manuscript.

Acknowledgments

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

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

Notes

1. All *t* tests reported in the article were two-tailed.
2. 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 was ruled out by examining mean responses for the different scales used in Experiments 4 and 5. Collapsing over ethical and unethical conditions (and translating percentage-gray responses into equivalent numerical scale points), we found that subjects in Experiment 4 (who used the numbered scale) gave reliably lower responses than did subjects in Experiment 5 (who used the gray-scale-patch scale; *M*s = 3.96 vs. 4.75, respectively), $t(169) = 3.50$, $p < .001$; $d = 0.539$.
3. Nor does the El Greco fallacy apply to Dilks, Serences, Rosenau, Yantis, and McCloskey'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."

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.

Firestone, C. (2013a). How "paternalistic" is spatial perception? Why wearing a heavy backpack doesn't—and *couldn't*—make hills look steeper. *Perspectives on Psychological Science*, *8*, 455–473.

Firestone, C. (2013b). On the origin and status of the "El Greco fallacy." *Perception*, *42*, 672–674.

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, NY: 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.