more "than any other sensory modality, olfaction is like emotion in attributing positive (appetitive) or negative (aversive) valence to the environment" (Soudry et al. 2011, p. 21). Olfaction is a judgmental sense in which perceiving and judging are intertwined.

In summary, contrary to what F&S write, authors talking about "perceptual judgment" (sect. 4.2.3, para. 2) do not invite confusion about a foundational distinction between perception and judgment. Instead, they present evidence that there is no foundational distinction between perception and judgment. Consequently, the failure to disentangle perception from judgment is not a pitfall of flawed studies, but rather an acknowledgment that there is no clear division between the two. Although such a claim may seem revolutionary for vision, it is not a new idea for other modalities. We should not make the mistake of basing our understanding of perception exclusively on vision.

Cognition can affect perception: Restating the evidence of a top-down effect

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Abstract: We argue that Firestone & Scholl (F&S) provide worthwhile recommendations but that their critique of research by Levin and Banaji (2006) is unfounded. In addition, we argue that F&S apply unjustified level of skepticism about top-down effects relative to other broad hypotheses about the sources of perceptual intelligence.

We believe that Firestone & Scholl's (F&S's) target article represents a commendable standard for evaluating top-down effects, and we agree with many of their recommendations. We also agree that it is possible to overstate the power of fleeting abstractions to impact our immediate impression of the world, and we are skeptical of the view that perception is so saturated with belief that we can see whatever we wish. However, we also know from many decades of research that perception integrates sensory input with reliable world-knowledge. To deny such evidence would be to deny that humans are flexible learners. This is where we probably diverge from F&S: We do not think that evidence for top-down processes represents a surprising or dramatic departure from established theory, nor that the top-down hypothesis requires a higher standard of proof than any other hypothesis about the functioning of the mind in physical and social space.

Here, we focus on the impact of race on the perception of the lightness of faces. Levin and Banaji (2006) demonstrated that participants seem to perceive Black faces to be darker than White faces. This effect was present both when participants adjusted samples to match unambiguous Black and White faces, and when one group of participants judged an ambiguous face that they were told was Black while another group saw the same face, this time believing that it was White. In the target article, F&S review previous experiments (Firestone & Scholl 2015a) that focus on the former effect with unambiguous faces, arguing that it was a qualitatively more effective demonstration of a top-down effect. However, they also argued that this finding suffered a potential stimulus confound. F&S tested this confound using blurred faces (the left half of Fig. 1) to measure whether participants who were nominally unable to identify the faces by race still showed the lightness illusion (e.g., participants who indicated that the faces were of the same race still judged the White face to be lighter).

In our response (Baker and Levin 2016) we noted that F&S used a forced-choice question to obtain judgments about which face was darker, and used a nonforced choice to assess detection of race (participants selected from a menu of possible races independently for each face). What if the forced-choice lightness question was more sensitive to lightness than the race-detection question was to race? F&S may have underestimated participants' ability to detect race in the blurred faces, and may therefore have falsely classified some participants as unable to detect race. This seems particularly plausible given that the classification was based on one or two judgments about subtle, near-threshold information. Indeed, when we included a forced-choice question that directly asked participants to choose which face was White and which was Black, we repeatedly observed that 75%–80% of participants correctly assigned race. It is important to note that participants were just as successful in detecting the race of the faces when the faces were contrast-inverted (Fig. 1), so it seems unlikely that they detected the race of the faces by noting the brightness confound and by guessing that the lighter face was White.

In addition to evidence that the blurring left some race-specifying information in the images, Baker and Levin found that participants who correctly distinguished the race of the noninverted faces also were more likely to judge that the Black face was darker. This result supports the hypothesis that there is a relationship between participants' ability to

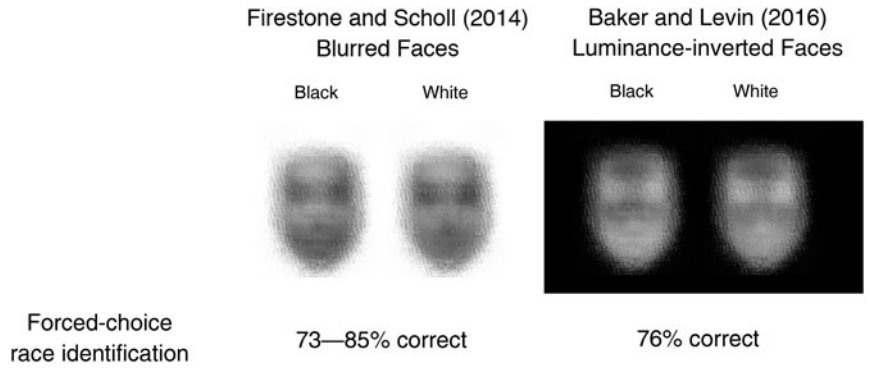

Figure 1 (Levin et al.). Illustration of accuracy on forced-choice identification of race for blurred stimuli employed by Firestone and Scholl (2015a). Firestone and Scholl argued that participants who could not identify the race of the stimuli on left nonetheless showed a brightness effect. However, we demonstrated that participants were able to accurately judge the race of the faces both for the original blurs and in luminance-inverted versions of the faces (Baker and Levin 2016).

perceive the race of the faces and how light each face appears to them.

Space constraints prevent us from reviewing all of F&S's critique and all of the logic underlying our response, but the complexity of the issue leads us to a key point: The original Levin and Banaji report foresaw the difficulty in fully eliminating confounds inherent to two different stimuli, and so it included the abovementioned ambiguous face experiment, along with an experiment in which RT on same–different judgments was slowed when a relatively lightened Black face was compared with a White face (thus equalizing their apparent lightnesses). These additional experiments cast serious doubt on F&S's conclusion "that the initial demonstration of Levin and Banaji (2006) provides *no evidence* for a top-down effect on perception" (sect 4.4.1, para. 5; emphasis added). The casual reader might be forgiven for assuming that Levin and Banaji's entire study can be dismissed unless they realize that the word "initial" means that only one of several experiments are at issue and read the footnote describing one of these other experiments. We think that this quote reveals a fundamental problem with F&S's approach. The categorical conclusion implies that experiments must either provide unambiguous proof of top-down effects by avoiding all of the pitfalls they describe, or the work falls to zero weight in tipping the scale to the top-down side of a debate that is complex enough to have been raging for a long time.

We prefer a more nuanced approach to advancing research on this topic for several reasons. First, there are many different kinds of top-down effects, some in which momentary thoughts influence how things look, and some more subtle effects where a more-sophisticated perceptual process influences a less-sophisticated one, perhaps as the result of long-term experience. This is especially evident in the social domain, where category-informed reactions to skin color can clearly be consequential. Of course, researchers' specific interests might lead them to isolate the truly perceptual sources of judgments about experience, but at some point it becomes an exercise in purity that provides license to focus exclusively on relatively artificial stimuli and tasks designed a priori to reveal phenomena that will confirm evidence of bottom-up processing. In all cases rigor is crucial, and F&S provide some good recommendations in achieving that. But rigor should not be an excuse to ignore the study of important phenomena. We believe that discovery is best served by exploring the full richness of human perceptual capacities that may or may not reveal cognitive penetration rather than dwelling exclusively on simpler perceptual process from a penchant for tidiness.

Not even wrong: The "it's just X" fallacy

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Abstract: I applaud Firestone & Scholl (F&S) in calling for more rigor. But, although F&S are correct that some published work on top-down effects suffers from confounds, their sweeping claim that there are no top-down effects on perception is premised on incorrect assumptions. F&S's thesis is wrong. Perception is richly and interestingly influenced by cognition.

Disagreements arise when people argue with different facts. But disagreements can also arise when people argue from different starting assumptions. F&S and I share all of the same facts, but F&S come to the wrong conclusions because they have the wrong assumptions.

Many of the studies F&S review indeed suffer from stimulus and experimenter-demand confounds, But many others are well-controlled investigations using gold-standard psychophysical methods. These studies show that expectations and knowledge affect virtually all aspects of visual perception. For example, knowledge of surface hardness affects amodal completion (Vrins et al. 2009), knowledge of bodies affects perceiving depth from binocular disparity (Bulthoff et al. 1998), expectations of motion affect motion perception (Sterzer et al. 2008), and knowledge of real-world size affects perceived speed of motion (Martín et al. 2015). Meaningfulness – a putatively late process – affects putatively earlier processes such as shape discrimination (Lupyan & Spivey 2008; Lupyan et al. 2010) and recovery of 3-D volumes from two-dimensional images (Moore & Cavanagh 1998). Color knowledge affects color appearance of images (Hansen et al. 2006) and even color afterimages (Lupyan 2015b). Hearing a word affects the earliest stages of visual processing (Boutonnet & Lupyan 2015; see also Landau et al. 2010; Pelekanos & Moutoussis 2011).

How can F&S, who are aware of all of this work (some of which they discuss in detail in the target article), still argue that there are no top-down effects on perception? They dismiss all of those studies on the grounds that they are "just" effects of attention, memory, or categorization/recognition. This "it's not perception, it's just X" reasoning assumes that attention, memory, and so forth be cleanly split from *perception proper*. But attentional effects can be dismissed if and only if attention simply changes input to a putatively modular visual system (sect. 4.5). Memory effects can be dismissed if and only if memory is truly an amodal "back-end" system. Recognition and categorization effects can be dismissed if and only if these processes are wholly downstream of "true" perception (sects. 3.4, 4.6). All of those assumptions are wrong.

Some aspects of attention really *are* a bit like changing the input to our eyes. Attending to one or another part of a Necker cube is kind of like shifting one's eyes. If we dismiss the latter as an interesting sort of top-down effect on perception, we should likewise dismiss the former. But as we now know, attention is far richer. We can, for example, attend to people or dogs, or the letter "T" (across the visual field) – a process of deploying complex priors within which incoming information is processed. In so doing, attention warps the visual representations (e.g., Çukur et al. 2013; sect. 5.2 in Lupyan 2015a for discussion). Aside from the simplest confounds in spatial attention, attentional effects are not an alternative to top-down effects on perception, but rather one of the mechanisms by which higher-level knowledge affects lower-level perceptual processes (Lupyan & Clark 2015).

Some top-down effects can be dismissed as being effects on memory. Someone might remember a $20 bill as being larger than a $1 bill, but not see it as such. But F&S's "just memory" argument goes much further. For example, Lupyan and Spivey (2008) found that instructing participants to view the meaningless symbols ⊓⊔ and ⊔⊓ as meaningful – rotated numbers 2 and 5 – improved visual search efficiency. F&S argue that this might be merely an effect on memory, citing Klemfuss et al. (2012) as having shown that decreasing the memory load by showing participants a target-preview caused the meaningfulness advantage to disappear. But actually, the largest effect of the target-preview was to *slow* search performance for the meaningful-number condition, bringing it in line with that of the meaningless-shape condition.

But suppose Klemfuss et al. actually found that showing a target-preview to participants improved search as much as the instructional manipulation we had used. Would this mean that meaningfulness does not affect perception? Not at all! If telling people to think of ⊓⊔ and ⊔⊓ as 2s and 5s is as effective as showing a target preview in helping them to find the completely unambiguous target in a singleton search, that would mean a high-level instructional manipulation meaning can affect visual search efficiency as much as an overtly visual aid. A top-down effect that can be partially ascribed to memory does not mean it is not (also) an effect on perception, because part of what we