Guest Editorial

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The Integrity of Motivated Vision: A Reply to Gilchrist, 2020

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In the September 2020 edition of *Perception*, Alan Gilchrist published an editorial entitled "The Integrity of Vision" (Gilchrist, 2020). In it, Gilchrist critiques motivated perception research.[1] His main points are as follows:

(1) Motivated perception is compromised by experimental demand: Results do not actually show motivated perception but instead reflect subjects' desires to comply with inferred predictions.
(2) Motivated perception studies use designs that make predictions obvious to subjects. These transparent designs conspire with experimental demand to yield confirmatory but compromised results.
(3) Motivated perception research lacks guiding theory and cannot explain what appear to be contradictory results.
(4) Motivated perception presents an unsupportable assault upon the impermeability of perception.

The present commentary responds to these four assertions.

Alleged Design Flaws

Do motivated perception researchers overlook experimental demand and do they employ designs that are so transparent as to invite compliance to demand? These are central propositions of Gilchrist's critique, and both are incorrect. We will use as a counterexample the first author's research on distance and height perception (Harber et al., 2011) because this research features centrally in Gilchrist's editorial. Harber et al.'s Study 1 predicted and

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found that the perceived distance from the self to an object was determined by whether the object was a threat (a live tarantula) or not a threat (a cat toy)[2] and whether the perceiver's self-worth was affirmed, unchanged, or depressed. As expected, subjects whose self-worth was depressed saw the tarantula as closer than it was. In contrast, those whose self-worth was affirmed were much more accurate. For subjects in the nonthreatening cat toy condition, self-worth had no effect on distance perception.

Could these outcomes be due to experimenter demand and/or the obviousness of predictions, as Gilchrist argues? This is highly unlikely. The experiment, contrary to Gilchrist's claim, employed deception. Subjects were told that the study concerned the relation between mental imagery and physical perception, the "mental imagery" instruction being a cover for the self-worth manipulation. Thus, the purpose of the experiment was, in fact, disguised.

More important, the study employed a between-subjects factorial design which created six independent conditions: Tarantula affirmed, tarantula neutral, tarantula depressed; cat toy affirmed, cat toy neutral, cat toy depressed. To have discerned the Study 1 hypothesis and satisfied experimenter demand, subjects in the six separate conditions would have had to independently decode this 2×3 factorial design and then shaped their respective responses accordingly. Doing so would require an astounding mental ballet. But if results were due to experimenter demand and transparent design, as Gilchrist asserts, then something like this must have happened.

Harber et al.'s Study 2, which Gilchrist also critiques, tested whether an internal resource (trait self-esteem) and access to a physical resource (a balcony handrail) would jointly affect height perception when peering down from a five-story elevation. This experiment, again contra Gilchrist, employed deception; subjects were told that using/not using the balcony handrail was instituted to prevent measurement bias, although the true purpose was to supply or deprive them of a physical support. Also, subjects' self-esteem was measured weeks prior in a setting unrelated to the experiment. There was therefore little chance of subjects connecting their measured self-esteem to the experiment.

Gilchrist writes that "It doesn't take a genius to infer what the experimenters expect [in Study 2] (p. 1000)." But to have decoded Study 2, subjects would have had to deduce:

(1) That handrail access was not done to reduce measurement bias (as claimed in the cover story) but was done instead to induce/not induce threat.
(2) That some subjects could use the handrail while others could not.
(3) That their own self-esteem, measured weeks earlier in a venue unrelated to the experiment and embedded with many other psychosocial measures, was a principal predictor.
(4) That the expected outcome involved the interaction of self-esteem and handrail access such that only those who both lacked self-esteem and lacked the handrail would supply exaggerated height estimates.

The likelihood of subjects correctly identifying all four of these design attributes and then generating responses concordant with their randomly assigned manipulated condition and their measured self-esteem level, is slim. The more parsimonious explanation is that an internal support (self-esteem) can supplant external supports (e.g., a handrail) and thereby reduce fear-induced height distortions.

Harber et al.'s Study 2 was replicated by Huynh et al. (2014) who employed a design even more difficult for subjects to discern. Huynh et al., like Harber et al., showed that height perception (when looking down) was jointly affected by differing resources. One resource, self-affirmation (boosted or unchanged), was akin to the trait self-esteem used in Harber et al. The second resource was self-regulatory demand, which was manipulated by having

subjects either complete a mentally challenging Stroop test (high self-regulatory demand) or a nondemanding card sorting task (low self-regulatory demand). Because the Stroop test and card sorting task both concerned visual skill, they would—from the subject's perspective—sensibly fit with the subsequent height estimation task and thereby reduce suspicion.

As Huynh et al. predicted, height estimations were overestimated by the "Stroop test" subjects (i.e., those who were regulatory-depleted) but only if their self-worth had not been boosted. When their self-worth was boosted, they did not overestimate height. If design transparency and experiment demand produced these results, subjects in the regulatory-depleted/nonaffirmed condition would have had to recognize that the Stroop task sapped their executive resources, that they had been selectively deprived of a resource-restoring affirmation, and that these two experiences were engineered to produce exaggerated height estimations. This degree of deduction seems unlikely.

Gilchrist cites two additional design flaws in Harber et al. which stand correction. He contends that Study 2 results were compromised by the presence of a close friend who, by lending moral support to the subject, shielded the subject from experiment demand and thus promoted candid and accurate height reporting. "Did the social support enable resistance to social pressure or did it change the apparent distance?" Gilchrist asks (p. 1000). However, Study 2 did not include a friend during any phase of the experiment. In perhaps unintended confirmation of our work, Gilchrist saw a friend where none exists. A few sentences later Gilchrist erroneously states that self-esteem was experimentally induced in Study 2 subjects. As mentioned, self-esteem was actually a measured rather than a manipulated variable, and thus problems arising from a clumsy self-esteem manipulation are moot.

In sum, obvious predictions and coinciding demand were unlikely confounds to either of the Harber, et al., 2011 studies, to Huynh et al.'s (2014) replication, or to several independent replications of this research using different resources and different designs (Burrow et al., 2016; Oishi et al., 2012; Slepian et al., 2013). Other research on affect and perception similarly withstands Gilchrist's critique concerning confounds. It is impossible to review all such studies here. However, several examples drawn exclusively from vision and neuroscience indicate that emotional states affect very basic visual processes:

(1) Contrast sensitivity, indicated by identifying the angle of Gabor patches, is improved by prior and brief (75 ms) exposure to a fearful face versus a neutral face, indicating that emotion alters early visual processing (Phelps et al., 2006). Phelps et al. (2006) conclude that "... emotions actually affect how people see" (p. 294).
(2) The Ebbinghaus Illusion, wherein a central circle appears larger if flanked by smaller circles and smaller if flanked by larger circles, is moderated by imposing positive and negative emotional images on the circles (van Ulzen et al., 2008).
(3) Anticipated time to collision between the self and a directly approaching ("looming") object is moderated by whether the object is threatening, for example, a snake or a spider, or is not threatening, for example, a butterfly or a rabbit (Vagnoni et al., 2012). This effect was not due to incidental stimuli features.

Collectively these and other studies of motivated perception address a wide variety of perceptual experiences (distance, hill slants, size, weight, velocity, physical pain) and employ a variety of methods and measures (many nonobvious) to do so. Might some studies be compromised by experimental demand, transparent designs, or other failings? Perhaps. But there are too many that are not so compromised, arriving at mutually compatible outcomes through a diverse range of designs and manipulations including those crafted to disguise hypotheses and to deter demand and other confounds (see Stefanucci et al., 2011 for a

review). Transparent designs and experimental demand cannot account for this extensive and expanding body of research (see also Block, 2016 and Cañal-Bruland et al., 2016, for related arguments).

Absence of Theory

Gilchrist states that "Advocates of embodied perception show a curious lack of interest in theory. Like advocates of extrasensory perception, they spend more time stacking up evidence for their claims than seeking a coherent explanation." The ad hominem tone aside, this is a strange argument for a scientist to make. If all investigation were tethered to existing theory, science would stagnate. Serendipitous findings excite further investigation, patterns are then recognized, and theory develops (Kuhn, 1962). Gestalt psychology, which informs Gilchrist's research on lightness illusions (Gilchrist, 2014), was supposedly launched by Wertheimer's spontaneous experience of apparent motion during a train ride (Marx & Hillix, 1963). Research on motivated perception, no more than other phenomena, requires extant theory to be valid.

However, there are theories that provide guidance and coherence to motivated perception research. Dennis Proffitt's economy of action theory (Gross & Proffitt, 2013; Proffitt, 2006) argues that the perceptual system scales conscious percepts to one's physical capacity. Thus, a hill slope appears steeper and a traverse seems wider for those whose abilities are diminished by exertion, age, or other constraints. Proffitt's economy of action theory helped guide a motivated perception study (Schnall et al., 2008) wherein hills were seen as less steep when accompanied by a friend (Study 1) or when simply first imagining a friend (Study 2). Gilchrist indirectly acknowledges economy of action theory at the end of his editorial. He rhetorically asks what adaptive advantage would be gained by elevating energy conservation "above all other human needs" (p. 1002). Economy of action does not claim such grandiose status for itself but rather regards modified perception as but one source of important feedback upon which behavior is shaped. In any case, Gilchrist cannot have it both ways, first complaining about the absence of theory and then disparaging theories that do exist.

Another emerging theory related to motivated perception is the first author's Resources and Perception Model (RPM), which is outlined in the Harber et al. (2011) paper that occupies much of Gilchrist's critique. According to RPM, threatening objects and events are perceptually distorted, but psychosocial resources reduce the experience of threat. Therefore, those who have sufficient resources should perceive threatening things in a less distorted way. RPM is supported by an increasing number of studies (Gorman et al., 2016; Harber et al., 2008, 2011; Huynh et al., 2014; Schnall et al., 2008). RPM itself draws heavily on Proffitt's economy of action, Hobfoll's Conservation of Resources theory of resources and coping (Hobfoll, 1989), and classic and revised New Look theory (Easterbrook, 1959; Erdelyi, 1974; Greenwald, 1992).

Conceptual frameworks emerging from motivated perception address another of Gilchrist's complaints; why are both desired and threatening stimuli perceived as closer than they actually are? The explanation is that distance perception informs adaptive behavior. For a thirsty person, seeing a water bottle closer than it is (e.g., Balcetis & Dunning, 2010) could motivate approach behavior. For a frightened person, seeing a tarantula as closer than it is (e.g., Harber et al., 2011) could motivate avoidance behavior. Adaptive action likewise explains why perceived distance is increased for a height and shortened for a tarantula. Both convey threat, which their respective distortions amplify, leading to avoidance—moving away from the amplified height, moving away from the looming tarantula.

Thus, the sensory system, per Proffitt's (2006) economy of action, Balcetis's (2016) approach/avoidance framework, and Harber's RPM, guides action adaptively.

In sum, there are emerging frameworks designed to structure the abundant evidence of motivated perception, and which explain what might superficially seem to be contradictory results. And if these patterns are as counterintuitive as Gilchrist suggests, then how could they be produced by obvious designs and experimental demand as he also claims?

The Integrity of Motivated Perception Research

Gilchrist concludes his editorial with an argument against the cognitive (or nonperceptual) penetrability of perception. The penetrability debate is too rich and complex to be examined here, although we think that compelling cases have been made for theoretically important cognitive influence on perception.[3] In any event, the penetrability question is distinct from whether demonstrations of motivated perception are empirically valid. As we have argued, research supporting motivated perception is too broad, varied, and methodologically complex to be dismissed as the residue of experimental demand or other such confounds. The interesting questions are not whether such experiences occur—that issue seems well settled—but instead when, why, and how they occur.

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Notes

1. For the sake of economy, we use the term *motivated perception* to cover all research studying the effects of motives, needs, resources, and emotions on perceptual experience.
2. There were actually two cat toys, a furry mouse, and a fuzzy ball to control for object artifacts.
3. For just a handful of many recent examples, see Lupyan, 2015; Lupyan and Clark, 2015; Lupyan et al., 2013; Macpherson, 2012; Marchi & Newen, 2015; Phelps et al., 2006; Stokes, 2012, 2018; Wu, 2013. See Stokes, 2013 and Silins, 2016 for two fairly recent review pieces. And for a recent article, critical of the possibility of cognitive penetrability of perception, see Firestone and Scholl 2016 and the related peer commentary.

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