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Scepticism about unconscious perception is the default hypothesis

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In their recent contribution to this journal, Berger and Mylopoulos (2019; henceforth: B&M) offer a critical response to scepticism about unconscious perception, focusing on experimental work from Peters and Lau (2015; see also Knotts et al., 2018), and theoretical work of my own (Phillips, 2016, 2018; see also Phillips & Block, 2016; Peters et al., 2017). B&M's critique ranges widely, raising many important points which receive detailed consideration in sequel. But a core theme is that the existence of unconscious perception occupies a default status both within psychological science and folk psychology. As B&M see it, if the sceptic were right, we would not only have to 'reinterpret huge swaths of experimental results' but also 'revise our folk psychological understanding of perception' (2019: 10). Unsurprisingly, then, they see the burden of proof as resting on the sceptic's shoulders. Here, I argue to the contrary that a conscious-perception-only model is the appropriate default hypothesis. The believer in unconscious perception thus owes us compelling evidence of its existence—evidence which, I contend, remains wanting.

Discussion proceeds as follows. Section one introduces Peters and Lau's (henceforth: P&L's) study and offers my own critical analysis. Inspired by this discussion, section two introduces the paper's core contention: our default hypothesis should be that a single conscious signal underlies perceptual task performance. Section three turns to B&M's rather different criticism of P&L's study. I argue that B&M fail to provide adequate reason to favor their

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substantially more complex 'unconscious perception plus non-visual hunch' hypothesis. Section four responds to B&M's arguments that visually guided action involves unconscious yet genuinely individual-level perception. Finally, section five confronts an important dilemma which B&M raise for the sceptic: deny unconscious mentality in general or provide some reason for thinking that perception is special.

1. Peters & Lau, 2015

In P&L's basic paradigm, subjects are presented with two intervals. In one (target-present), a tilted Gabor is presented sandwiched between masks. In the other (target-absent), only masks are presented. Subjects must make a left/right orientation judgement in relation to each interval. They must also indicate which judgement they wish to bet on for an extra 'point' if correct. P&L describe this betting task as *type 2 two-interval force-choice confidence-rating*—a description to which I return shortly. Figure 1 provides details.

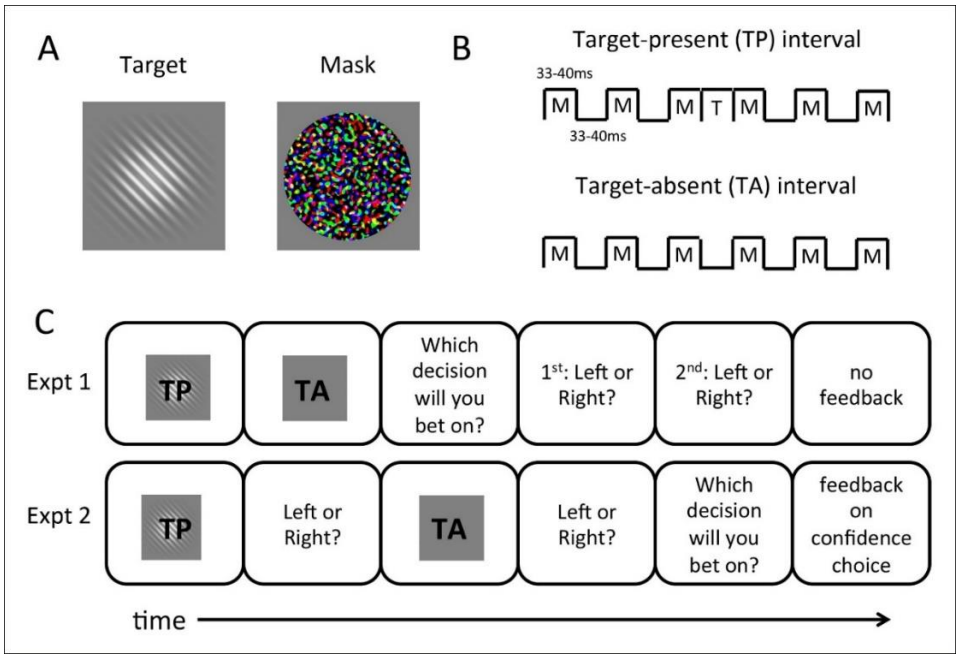

Figure 1. Stimuli and task design for P&L's Expts 1 and 2. (A) Targets are either 45º left- or right-oriented Gabors. Masks consist of random colored noise. (B) Trials involve the presentation, in random order, of target present and target absent intervals. In the former, a target is presented sandwiched between six masks. In the later, only masks are presented. (C) Example trials for Expt 1 and Expt 2. © P&L. Reproduced under a Creative Commons Attribution License.

P&L's aim is to assess whether subjects exhibit above-chance orientation sensitivity, even when their betting behaviour is at chance. They think it 'straightforward' (3) that such a pattern of results should be interpreted as performance without awareness. However, P&L find no such pattern; as soon as subjects can discriminate target orientation above chance, they can bet above chance. For P&L: 'This surprising finding suggests that the thresholds for subjective awareness and objective discrimination are effectively the same' (1).[2] I am in strong sympathy with this conclusion. Nonetheless, questions need raising regarding P&L's methodology.

P&L associate awareness with 'subjective introspection (confidence)' (1), operationalizing this with their betting task which they call 'a measure of confidence; Type 2 judgment' (11). Rightly wishing to avoid the problem of the criterion (Eriksen, 1960; Merikle et al., 2001), they adopt what they take to be a '2-interval forced-choice' (2IFC) design to avoid response bias. However, it is questionable whether their betting task is properly described in these ways.

In a type 1 task, the subject must judge which stimulus event occurred (e.g., whether a stimulus was presented in interval 1 or 2). In a type 2 task, by contrast, the subject must judge

[2] Using a variation on this methodology, Knotts et al. (2018) argue that there is equally no evidence of unconscious discrimination under continuous flash suppression.

whether their own responses (i.e., type 1 judgements) were correct or not (e.g., whether their judgement that the stimulus was presented in interval 2 was correct) (Clarke et al., 1959; Galvin et al., 2003).[3] A 2IFC task (whether type 1 or type 2) must contain two-intervals, only one of which contains a target. In the case of a type 2 2IFC task, the two intervals must both contain type 1 tasks, only one of which should contain a target (correct) response. P&L's betting task does not conform to these strictures. In target-absent intervals, P&L code 50% of responses as 'correct' (11). This means that the task is not 2IFC since both intervals can contain 'correct' or 'incorrect'. The problem would not be resolved by coding the target absent interval as always 'incorrect' since both intervals could still be coded 'incorrect'.

P&L's data analysis also appears inappropriate for a genuine type 2 task. In that analysis, P&L define a type 2 hit as a 'correct orientation discrimination and bet on target-present interval' and a type 2 false alarm as an 'incorrect orientation discrimination and bet on target-present interval' (12). Yet if 50% of the target absent intervals are coded as correct, then a bet on the target absent interval should be coded as a hit 50% of the time. Similarly, bets on the target absent interval should count as false alarms when that interval is coded as incorrect. P&L's treatment of the data seems more appropriate to a *pseudo-type 1* task on which high-confidence judgements are treated as equivalent to type 1 decisions made using higher decision thresholds (and mutatis mutandis for low-confidence judgements).[4]

[3] Strictly, therefore, a type 2 task must always follow a type 1 task (Galvin et al., 2003: 847). Yet in P&L's Expt 1 it precedes it.

[4] This is standard procedure in the construction of confidence-based ROC curves (Macmillan & Creelman, 2005: chpt. 3), though see Galvin et al. (2003) for critical discussion.

Finally, it is questionable whether P&L's betting task is really a *confidence-rating* task as opposed to *type 1 2IFC detection*.[5] That would mean the overall task design involved two judgements: (1) Which interval is the Gabor in (type 1 2IFC detection)? (2) Is it titled to the left or right (identification)? (The other orientation judgement becomes irrelevant.) The task is thus one of simultaneous detection and identification. This design has been extensively studied (e.g., Thomas et al., 1982; Thomas, 1983; Macmillan & Creelman, 2005: 255ff.). Such studies have consistently found that 'detection and identification go hand in hand' (Thomas, 1985: 1465), i.e., that both tasks exploit the same underlying sensory signal. Arguably, P&L's results replicate this finding—a claim bolstered by the fact that their control study in which subjects indicated the 'more visible' interval (a task which they agree is 'akin to a 2IFC detection') yielded results 'closely mirror[ing]' those of the main experiments (24, Appendix 3). P&L's data thus do not provide novel reason for doubting the existence of unconscious perception.

[5] P&L recognize this last concern and offer two replies. First, they argue that 'subjects were worse at orientation discrimination when they did not select the target-present interval' (7). However, this does not favour a confidence-rating interpretation. If subjects were engaging in 2IFC detection, we would equally expect them to be worse at orientation discrimination when they did not select the target-present interval. Detection errors indicate a weaker signal which in turn predicts poorer orientation discrimination. P&L also claim that 'subjects did not bet on orientation discrimination choices they expected to get wrong, even at high performance (i.e. high contrast) levels' on the basis that type 2 false alarm rate remained at chance-levels (≈ 0.5) even as orientation-discrimination accuracy increased. However, if a type 2 false alarm is defined (as above) as an 'incorrect orientation discrimination and bet on target-present interval', then this does not follow. Rather, such a pattern would suggest that as signal strength increases, subjects are increasingly likely to bet on the target-present interval (thereby increasing the false alarm rate so-defined) whilst simultaneously being more likely to get the orientation judgement correct (thereby decreasing the false alarm rate so-defined)—a trade-off which could easily lead to a relatively flat false alarm rate (again: so-defined).

2. Conscious perception only is the default hypothesis

Despite the concerns just raised, P&L's study remains of value. It adds to a wealth of psychophysical data indicating that performance in many different tasks and across a vast range of stimuli tap a single conscious signal. This includes not just objective discrimination tasks but also subjective detection, identification, awareness and visibility-rating tasks—*when performance is measured in an appropriately bias-free manner* (i.e., by a detection theoretic parameter such as *d'*). Of special relevance to issues of unconscious perception is rigorous psychophysical evidence showing that *subjective detection* and forced-choice discrimination are subserved by a single signal (Baldson & Azzopardi, 2015; Heeks & Azzopardi, 2015). In this work, 'subjective detection' describes tasks in which a subject must say whether or not they think a target has been presented. These judgments concern the world (i.e., target presence) not experience. However, it is a mistake to think that they do not bear on experience. Subjective tasks necessarily require the observer themselves to select a criterion for a positive response. Such responses thus reflect how things seem from the point of view of the subject, that is their subjective, experiential perspective (Nagel, 1974). This makes explicit why Baldson, Heeks and Azzopardi take their work to show that the 'same information is available to influence reports on phenomenological experience as for generating forced-choice responses' (Heeks & Azzopardi, 2015: 77).[6]

[6] Consider also Weiskrantz on blindsight, a condition in which it is widely thought that performance ('sight') may persist in the absence of awareness ('blindness'): 'This is exactly the difference implied in the meaning of blindsight itself: good performance when forced to assign a discriminant as opposed to making a 'yes'/'no' judgment, as in perimetry.' (2009: 58). Here, too, Weiskrantz implicitly treats 'yes'/'no' detection as indicative of phenomenal awareness. On the issue of whether blindsight does in fact involve such a dissociation see Phillips, 2016, 2020.

The hypothesis that a single conscious signal subserves perceptual task performance enjoys explanatory success across a broad and diverse range of paradigms, contexts, and populations. Plainly, such a hypothesis is more parsimonious than rivals. As Snodgrass notes: 'it postulates only one rather than two [or more] perceptual processes' (2002: 556). For these reasons, when considering any novel task, population or context: 'A conscious-perception-only model is the null hypothesis' (ibid.).[7] Correspondingly, scepticism about unconscious perception is the appropriate default hypothesis. The burden lies with the believer in unconscious perception to provide convincing evidence in favour of their rival view.

Null hypotheses can of course be rejected on the basis of evidence.[8] And from B&M's presentation one might think that there were 'huge swaths of experimental results' supporting such rejection. Yet whereas B&M present scepticism about unconscious perception as a recent trend, it is anything but. Modern psychology is marked from its inception by waves of enthusiasm for unconscious perception, followed by waves of protest (Overgaard & Timmermans, 2010; Irvine, 2012; Michel, 2020). B&M note only the enthusiasm, remarking that 'perceptual psychology has

[7] P&L interpret their data as evidence that metacognitive judgements are subserved by the same signal as first-order discrimination. I raised doubts about this above. Nor is it obvious how metacognition and consciousness relate. Nonetheless, here too, the default hypothesis should be that metacognitive judgements tap the same signal as first-order discrimination. Standard psychophysical practice of using confidence-ratings to construct type 1 ROC curves arguably comports with this hypothesis. For an important recent discussion consistent with the present perspective, see Miyoshi & Lau, 2019.

[8] Indeed, Snodgrass himself offers psychophysical evidence against the hypothesis. See discussion in Phillips, 2018. Likewise, Heeks and Azzopardi (ibid.) contend that blindsight shows that 'performance *can* be dissociated from awareness' following brain damage (see Azzopardi & Cowey, 1997, 1998, 2001). For an alternative perspective on which the apparent dissociation in blindsight is consistent with a single signal hypothesis see Phillips 2016, 2020.

been professedly studying unconscious perception for decades' (10) citing Peirce and Jastrow (1884) and Marcel (1983). But they neglect the protest. In particular, it has been long been argued that the subjective measures used by Peirce and Jastrow (1884) and subsequent experimentalists to assess awareness are inadequate—this is a central lesson of detection theoretic critiques of so-called 'subliminal perception' (e.g., Eriksen, 1960). Less well-known is the fact that although Marcel (1983) professed to have employed an objective measure of awareness, he did not. Moreover, he chose to count performance below 60% as unaware (chance being 50%). Using a more rigorous assessment of awareness, Cheesman and Merikle (1985) failed to replicate his findings (see also Kouider & Dehaene, 2007).

What about folk psychology? Is unconscious perception part of our 'folk psychological understanding of perception' (10) as B&M contend? That is certainly disputable. Against it, Campbell insists: 'Our ordinary conception of seeing is the conception of a subjective state.' (2011: 277) Likewise, Farah notes: 'Most people would say that one has not perceived something if one is not consciously aware of that thing.' (1994: 203) Indeed, the *Oxford English Dictionary* defines perception as 'the process of becoming *aware* of physical objects, phenomena, etc., through the senses' (Simpson & Weiner, 1989, my emphasis). (For extensive discussion see my 2018.)

In support of their arguably heterodox view, B&M claim that 'ordinary people often say things that reflect a view on which we can perceive aspects of our environment without conscious awareness, such as: "I wasn't aware of that loud construction outside until just now, but it explains why I've felt distracted this whole time."' (10) However, 'awareness that' constructions attribute knowledge (or possession of evidence) not merely perceptual consciousness (e.g., Dretske, 1969, 1979; Williamson, 2000: 33-41; French, 2012). In the example, what is reported is a failure to

notice and so know about the loud construction. This is consistent with consciously hearing or seeing it, and so provides no support for folk belief in unconscious perception.

B&M also point to 'experimental evidence that laypeople believe that "unfelt" — that is, unconscious — pains exist' (10), citing Reuter and Sytsma (forthcoming). Reuter and Sytsma's seventeen studies are, however, highly problematic. To illustrate, consider their first study in which participants were presented with the following case and question:

> Doctors have observed that sometimes a patient who has been badly injured will get wrapped up in an interesting conversation, an intense movie, or a good book. Afterwards, the person will often report that during that period of time they hadn't been aware of any pain.
>
> Which of the following descriptions of this type of situation seems most appropriate to you?
>
> (A) The injured person still had the pain and was just not feeling it during that period.
>
> (B) The injured person had no pain during that period.

90% of subjects selected (A). Yet these options are plainly not exhaustive. Another possibility is that the patient was consciously aware of the pain but, being distracted from it, failed to mind it, remember it or consider it worth reporting. Forced responses between non-exhaustive options can hardly be regarded as probative.

Or consider study thirteen in which participants were asked to choose from one of four options:

i) When you have a pain, you feel that pain **all** of the time.

ii) When you have a pain, you feel that pain **most** of the time.

iii) When you have a pain, you feel that pain **some** of the time.

iv) When you have a pain, you feel that pain **none** of the time.

Does the fact that only 8.5% of participants chose (i) show that the folk think that pain can occur unfelt? No. It is very natural to interpret (i) as the claim that, when you have a pain, you feel it *constantly* (i.e. without interruption). But there is nothing puzzling or unusual about the idea of intermittent pain. For the same reason, the fact that the vast majority (92.2%) of subjects agreed with the statements in study fifteen (e.g., 'Is it possible for a person to have a pain that they don't feel for a period of time?') in no way shows that a significant majority of participants believe it is possible to have an unfelt pain.

Much more might be said. But on the present evidence there is no reason to believe that folk psychology embraces unconscious perception. If it does not, scepticism is not revisionary, but common-sense.

3. Berger and Mylopoulos' rival hypothesis

With all this in mind, consider B&M's critique of P&L's paradigm. P&L's central claim, recall, is that whenever subjects can discriminate orientation above chance, they are conscious of the stimuli. Against this, B&M argue that 'there are good reasons to doubt that P&L's experimental

work establishes their conclusion' (18). In support they offer an alternative interpretation of P&L's data on which 'participants' unconscious perception of the stimuli causes them to have non-visual states — hunches — that register that a stimulus was present, which in turn causes participants to report that they are more confident in those trials' (18).

P&L themselves consider an alternative hypothesis along these lines, running a control experiment in which 'the subjective task was to indicate which interval appeared more *visible*' (7) to rule it out. As we saw, in this task too, no evidence was found of above-chance discrimination without above-chance visibility ratings. In other words, information available for discrimination appears to be fully available for betting *and* visibility judgements. B&M reply, however, that 'even these trials do not rule out the possibility that the relevant perceptual states were unconscious and that the relevant cognitive impressions were non-visual' (21). For, they continue:

> it is not implausible that participants would quickly or automatically infer that the non-visual impression that some stimuli were present was caused by their having seen those stimuli. … Moreover, since ordinary participants are typically not savvy regarding fine distinctions between types of mental state, it is plausible that they simply mischaracterized those hunches, which do *represent* visual states, as being themselves visual. (21)

Let us grant that this is a coherent account of P&L's data. Should we prefer it over P&L's interpretation? Consider how the two hypotheses compare. On the one hand, P&L suppose that a single conscious signal subserves objective discrimination as well as betting and visibility judgements—a view which I have argued merits (on grounds of parsimony and broad explanatory

success) the status of default hypothesis. On the other hand, B&M offer a complex picture on which unconscious visual signals, too weak to yield perceptual reports, nonetheless support (verbal) orientation judgements and are 'robust enough to inform metacognitive judgments' (19) due to the non-visual hunches they give rise to, and which in turn subjects are liable to confuse with genuinely visual states. The contrast in economy is striking. In its light, we sorely need a reason to take B&M's alternative hypothesis seriously and so pursue the corresponding research agenda.

B&M offer three such reasons. First, they argue that 'an appeal to hunches helps to explain why participants are typically reluctant to report that they saw something … and are seemingly at best able to make forced confidence judgments regarding their discriminations' (21). However, this is naturally accommodated on a conscious-perception-only model by noting 2IFC tasks are effectively criterion-free, whereas subjects operate with a naturally conservative response criterion in yes/no detection and awareness tasks in relation to weak signals (Björkman et al., 1993).[9]

Second, B&M note an 'obvious connection' (18) between their proposal and Type-2 blindsight. Blindsight is a large and controversial topic which I treat at length elsewhere (Phillips, 2016, 2020). It is very far from clear, however, that Type-2 blindsight involves non-visual hunches. Type-2 blindsight refers to residual performance in individuals with lesions to primary visual cortex when accompanied by acknowledged awareness. Weiskrantz (1998) insists that such awareness is not conscious vision since it does not involve conscious *seeing* or *visual qualia*. However, it certainly 'sounds suspiciously like residual conscious vision' (Eysenck & Keane,

[9] 2IFC tasks also provide twice as much information, so subjects will appear to perform better in them even when measured objectively unless the appropriate mathematical correction is applied.

2010: 64) and there are good reasons to think that this suspicion is right. For instance, whilst it is certainly true that some blindsight subjects report awareness in some conditions whilst being reluctant to report seeing, these patients nonetheless 'generally use visual language when asked to describe what their awareness is like' (Foley, 2015: 59). Patient GY has been found willing to make subjective matches between carefully selected visual stimuli in his blind and sighted field (Stoerig & Barth, 2001). And, indeed, GY sometimes straightforwardly reports seeing stimuli (e.g. Barbur et al., 1993: 1294f.; Kentridge et al., 1997: 194f) and even visual qualia on rare occasions 'when the stimulus is very bright' (Persaud & Lau, 2008: 1048; Phillips, 2016).

Type-2 blindsight does raise an important question: If patients are seeing as opposed to enjoying non-visual hunches, why do they deny it? Once again, the most parsimonious explanation points to conservative responding in relation to degraded vision. Here it must be emphasized just how impoverished blindfield vision is. The blindsighted subject does not see colors, objects, or visual form in their blindfield. Arguably their vision is limited to the perception of spatial or temporal differences in luminance or perhaps simply feature-non-specific differences (i.e., salience).[10] Given the dramatic contrast with their ordinary sighted-field vision, it is understandable that subjects might be reluctant to call it 'seeing' but willing to count it as 'mere' awareness (Foley, 2015). Consistent with this perspective is work comparing reports made using binary *seen* versus *guessed/unseen* options with reports made using a four-level perceptual awareness scale (Ramsøy & Overgaard, 2004) on which 1 = *no experience*; 2 = *weak experience*; 3 = *almost clear experience*; and 4 = *clear experience*. This work suggests that *guessed/unseen* responses collapse together both *no experience* and *weak experience* (Mazzi et al., 2016;

[10] Alexander and Cowey offer evidence that blindsight may be restricted to the ability to detect '"events"' varying in 'subjective salience' (2010: 532). For more general reviews see Kentridge, 2015; Phillips, 2020.

Overgaard et al., 2008). These brief points are hardly decisive, but they suffice to show that Type-2 blindsight does not provide strong independent evidence of non-visual hunches.

Third, and finally, B&M suggest assimilating hunches to metacognitive feelings, arguing that 'there is much independent experimental evidence that such feelings can track unconscious states' (20). Specifically, they cite Koriat (2000) on 'feelings of knowing' said to 'monitor ongoing memory retrieval processes without the states involved in those processes being conscious' (20). The topic of noetic feelings and metamemory is too large to engage with seriously here. But consider Koriat's basic contentions about memory and noetic feelings. In Koriat's view our only knowledge of our memories comes from retrieval processes. As he continues:

> Whenever we search our memory for a name or a word, many clues often come to mind, including fragments of the target, semantic attributes, episodic information, and a variety of subtle activations emanating from other sources. Although such clues may not be articulate enough to support an analytic inference, they can still act in concert to produce the subjective [noetic] feeling that the solicited target is available in memory. (2000: 159)

Now it is true that Koriat appears to view this 'subjective feeling' as a 'sheer' feeling produced by unconscious retrieval processes. However, Koriat is actually rather ambivalent concerning whether the retrieval processes which produce noetic feelings are unconscious. He talks equally of processes 'characterized by relatively low degrees of consciousness' (ibid: 153) or which 'operate below full consciousness' (158) or at a lower 'level of experience' (153). Whatever Koriat's considered view, this raises the question of what evidence there is that the processes generating

noetic feelings are strictly unconscious. Moreover, if they are in fact weakly conscious processes, the further question arises as to whether such feelings are not simply constituted by the kinds of fragmentary and subtle retrieval clues which Koriat lists. On that account, no hunches are in sight, simply different aspects of a conscious retrieval process.

To summarize: B&M offer a rival hunch-based hypothesis to account for P&L's data. However, this hypothesis is far less elegant than P&L's conscious-perception-only interpretation, and little independent reason is given to pursue it.

4. Unconscious perception and vision-for-action

At the heart of the issues above, lies the problem of the criterion. This problem only arises for paradigms in which objective discrimination is above chance. Yet, as I argue elsewhere, appeals to cases where discrimination is at chance instead face the problem of attribution: the challenge of explaining why a state which the subject themselves cannot use to make or guide a discriminative response should be considered a personal level, and so genuinely perceptual state (Phillips, 2018). In reply, B&M contend that genuinely 'unconscious perception can control and guide action in the ways required' (22) to meet conditions for being a personal-level state. Specifically, they follow Block and Kentridge (in Peters et al., 2017) in pointing to evidence of this in Milner and Goodale's influential work on dorsal-stream perception in visual-form agnosia (Milner & Goodale, 2006, 2008).

Whether such work really involves chance-level objective discrimination is a delicate question (see further below). Nonetheless, in previous work, I argued that, even granting Milner and Goodale's view of dorsally-based 'vision-for-action' as unconscious, the dorsal system could be seen as akin to an 'automatic pilot' and on that ground as failing to evidence personal-level

perception. Specifically, I cited findings from Pisella et al. (2000) showing that fast, dorsally guided, pointing movements are subject to 'irrepressible corrections' (732) outside of subjects' intentional control. Against this, B&M insist that on-line motor control is sometimes sensitive to conscious intention. Specifically, they argue that although Pisella et al. show 'that movements with a time course of under 300 ms are encapsulated from intention', the very same study reveals 'cases of vision-for-action involving slower movements' which are sensitive to conscious intention (24). B&M are right that in Pisella et al.'s studies, movement times over 300ms 'allowed voluntary control to fully prevail over automatic visual guidance' (2000: 730). But neither Pisella et al. (nor B&M) offer any reason to think that these longer-timescale, voluntarily controlled actions are driven by wholly unconscious, exclusively dorsally based processing. Indeed, Pisella et al. also examined an optic ataxic patient (IG) with damage to her dorsal system, and found that she did not examine any fast, automatic corrections, leading them to specifically associate the dorsal system with automatic online control. In other words, IG appears to be missing the automatic pilot. Pisella et al.'s results thus fully support my previous argument.

B&M also cite a study by Liu and Todorov (2007) as evidence that 'the motor system will … automatically adjust its outputs to compensate for task-relevant but not task-irrelevant perturbations' (23). On its basis, they conclude that since task-relevance is determined by conscious intentions, there is 'no good reason to hold that because dorsal stream vision proceeds automatically it is thereby subpersonal' (24). The reasoning here is not clear. Liu and Todorov show that subjects in a condition where a target is easily displaceable will quickly learn to reach more slowly and undershoot when correcting for target perturbations as compared to a condition where the target is hard to displace. This indicates a stability-accuracy trade-off in reaching. However, their study does not speak directly to questions of consciousness, conscious intention or

automaticity. Liu and Todorov's paper mentions none of these notions; in all cases targets and their displacements are plainly visible; and movement times extend well beyond 300ms. Suppose though we are convinced that reaching in Liu and Todorov's study is guided by an unconscious dorsal visual signal. Does the fact that reaching exhibits stability-accuracy trade-offs demonstrate that this signal constitutes personal-level perception? Not obviously. An alternative interpretation is that our dorsal 'automatic pilot' computes a composite cost function which includes stability in addition to accuracy, duration and energy cost (cf. Liu & Todorov, 2007: 9366-7).

B&M apparently rest most weight on their 'candidate case of unconscious vision-for-action' (24), namely visual form agnosia. They argue that patient DF's movements are far slower than those studied by Pisella et al., leaving us, 'no reason' to think they are insensitive to intention (24). This is mistaken. As already noted, Pisella et al.'s results precisely suggest that (ventrally based) conscious information becomes available to guide neurotypical action at timescales over 300ms. Yet DF is supposed to lack such conscious information. As a result, there is every reason to think that her actions will remain insensitive to conscious intention even at longer timescales.

I wish to raise a somewhat different point here, however. Minded of the central contention of the present paper, that a conscious-perception-only account should be our default hypothesis, it needs recognizing that Milner and Goodale's two systems picture is very far from unassailable. Arguably, just as with blindsight, theorists have been too quick to reject a conscious-perception-only model on the basis of apparent performance/awareness dissociations (Phillips, 2018: 500, fn.46). Consider B&M's primary example: the fact that DF fails verbal and manual tests of size constancy ('matching') but yet accurately scales her hand when reaching for objects ('grasping') (Goodale et al., 1991; Milner & Goodale, 2008). Milner and Goodale interpret this as evidence of grasping being guided by a separate, unconscious dorsal visual system with access to size

information unavailable to the ventral system subserving conscious vision. However, as forcefully argued by Schenk (2012), there is a fundamental difference between grasping and matching. Grasping provides visual and haptic feedback at the end of the grasp. Such feedback can be exploited to produce better grasp estimates in subsequent trials (Bingham et al., 2007). This learning may suffice to explain the apparent dissociations observed in DF. To test this, Schenk (2012) built a mirror apparatus allowing him to compare DF's grasping with and without visual and haptic feedback. Without feedback, DF performed no better than predicted from her manual size estimation performance. In other words, the dissociation disappeared.

Similarly, DF's successful posting of a card through an oriented slot despite her apparent lack of conscious orientation perception (Milner et al., 1991; Goodale et al., 1991, 1994) can be explained by her exploitation of an obstacle avoidance strategy which does not require orientation perception (Hesse et al., 2011). This again is wholly consistent with a single conscious process story. These issues remain highly controversial. The present point is only that a conscious-perception-only account must not be relinquished lightly.

5. A dilemma for sceptics about unconscious perception

In this final section, I address an important dilemma which B&M raise for the 'strong' sceptic who outright denies the existence of unconscious perception. The dilemma runs as follows:

> there would seem to be much common-sense and experimental evidence that *other* kinds of non-perceptual mental states — such as beliefs, desires, and emotions — can occur without being conscious, the strong sceptic must either explain why perceptual states are unique in the mind in so

> far as they cannot occur unconsciously, or maintain that these other kinds of mental states cannot, despite appearances, occur unconsciously. (25)

In reply, we first need to make a distinction between mental states and mental occurrences. Mental occurrences are events or processes. They unfold in time in virtue of the occurrence of different temporal parts at different times. As such, they are candidate elements in the Jamesian stream of consciousness. Mental states, in contrast, do not unfold in time. They do not have temporal parts. They can persist through dreamless sleep, and indeed through what O'Shaughnessy calls a complete 'mental freeze' (2000: 43) in which all occurrent mental processes and events are halted. Not being episodic, they are not candidate elements in the stream of consciousness. Mental states can *manifest* in conscious episodes, potentially including actions, bodily sensations, and conscious judgments and thoughts. But talk of conscious belief is a misnomer. As Crane puts it: 'beliefs are never phenomenally conscious, though episodes of *thinking* are (2013: 157). Beliefs, desires and emotions I suggest are best classified as *non*-conscious states which can manifest in one's stream of consciousness, in thoughts and feelings (e.g., a sudden urge or pang of desire, a hot flush of anger).[11]

With this distinction in place, we can see that beliefs, desires, and emotions are not the kinds of thing which could even potentially be conscious. However, this does not make perception unique. It only requires that perception can be distinguished from beliefs, desires and emotions in some important way. And it is: perceptual experience is an event or process. As a result, it has the right ontological shape to help constitute a stream of consciousness. But it is not the only kind of

[11] On this picture of emotions, see Wollheim, 1999. For broader discussion, see O'Shaughnessy, 2000; Soteriou, 2013.

mental event or process so suited: thoughts, feelings, and arguably actions are also just such events or processes.

For B&M's challenge to have force it must be that these kinds of mental events can occur unconsciously. Yet, in contrast to the case of belief, that is far from obvious. Certainly, changes in mental states are possible without consciousness. A belief may be lost or corrupted. An emotion may fade. Yet such changes can plausibly be analysed in terms of changes to mental states (e.g., the event of belief-loss, the process of emotional fading). A principled distinction thus remains to be drawn between mental episodes which can be analysed in terms of states, and those that cannot. In this light, it is not unreasonable to propose that all genuinely mental episodes other than those which can be wholly analysed in terms of transitions between non-conscious states are conscious (cf. Soteriou, 2009). This claim is undoubtedly controversial and requires much greater development. But it suffices to indicate one way in which the sceptic about unconscious perception might answer B&M's challenge.

6. Conclusion

B&M offer numerous considerations against scepticism about unconscious perception. Here, I have explained why I remain unconvinced. More importantly, I have argued that scepticism about unconscious perception is not somehow outlandish or revisionary. Instead, it is the appropriate default position both from the perspective of empirical and folk psychology.

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