

The role of beliefs about perception in perceptual inference

Matan Mazor^{1,2,3}, Rani Moran^{4,5}, & Clare Press^{2,3,6}

¹ All Souls College and Department of Experimental Psychology, University of Oxford

² School of Psychological Sciences, Birkbeck, University of London

³ Wellcome Centre for Human Neuroimaging, University College London

⁴ School of Biological and Behavioural Sciences, Queen Mary, University of London

⁵ Max Planck UCL Centre for Computational Psychiatry and Aging Research

⁶ Department of Experimental Psychology, University College London

Author note

Correspondence concerning this article should be addressed to Matan Mazor, All Souls College, High Street, OX1 4AL, Oxford. E-mail: matan.mazor@psy.ox.ac.uk

Abstract

According to Bayesian, “inverse optics” accounts of vision, perceiving is inferring the most likely state of the world given noisy sensory data. This inference critically depends on an internal model specifying how world states translate to visual sensations. Alternative accounts explain perceptual decisions as a rule-based process, with no role for beliefs about perception itself. Here, we contrast the two alternatives, focusing on decisions about perceptual absence as a critical test case. We present data from three pre-registered experiments where participants performed a near-threshold detection task under different levels of partial stimulus occlusion, thereby visibly manipulating the perceptual likelihood function going from external world states to internal perceptual states. We observe differential effects of sensory evidence and occlusion on decisions about presence and absence. Our normative model of visual detection accounts for these asymmetries, and further reveals robust individual differences, with some participants systematically incorporating beliefs about perception into perceptual inferences more than others. We discuss implications for the varied and inferential nature of visual perception more broadly.

Keywords: absence, probabilistic reasoning, Bayesian modelling, perceptual inference

Word count: 11,681

The role of beliefs about visibility in perception

Introduction

After checking Taylor Swift's Wikipedia page, we are confident that she hasn't announced her retirement from music. If she had, it would have been mentioned on her page. We also checked cellist Natalia Gutman's page and didn't see any mention of a similar announcement, but we are not so sure she hasn't made one since her Wikipedia page only gets updated irregularly. The absence of evidence on Wikipedia is enough to make a solid inference in the case of Swift but not in the case of Gutman because we know that information about Swift spreads more efficiently on the internet. In other words, we believe that something is not true when we believe that "if it were true, we would have heard about it" (Goldberg, 2011).

More generally, for a decision to be rational, it should depend not only on observation and prior beliefs, but also on our beliefs regarding the likelihood of the observation in hand given competing hypotheses about the world. This role for probabilistic inference is especially pertinent when the observation in question is that no evidence is available, as the absence of evidence for a signal can reflect the true absence of a signal, or a failure to obtain evidence for it (Altman & Bland, 1995; Locke, 1690). This leaves a critical role for the decision maker's internal model of how likely evidence is to become available when a signal is present (Oaksford & Hahn, 2004; Walton, 1992, 2010).

Here we ask whether people apply a similar reasoning in the domain of perception, focusing our investigations on visual detection. If such reasoning is used, decisions in the absence of a stimulus should be a function not only of what an agent sees (*perceptual evidence*) and what they expect to see (*prior*), but also of their beliefs about the probability that a stimulus would be

seen, if present (the perceptual *likelihood* function). We pit this account (which we term *INCORP*, to signify the incorporation of beliefs about the perceptual likelihood function, that is, about visibility) against the alternative (*IGNORE*) that decisions about absence are made in a rule-based fashion, and relative to an internal criterion which is set based on beliefs about the probability of stimuli, the payoff structure, and recent history – but crucially not the expected likelihood of evidence (and, importantly, the absence of evidence). Such a criterion can be set in sensory units (“respond *absent* for anything below this level of brightness,” Treisman & Williams, 1984) or temporal units (“respond *absent* if the target hasn’t been detected within this much time,” Chun & Wolfe, 1996), without a reference to probabilities or likelihoods.

To arbitrate between these two possibilities, we conducted three experiments where detection decisions about presence and absence were made under different levels of stimulus occlusion. Our task design allowed us to experimentally dissociate actual visibility (manipulated with occlusion and random fluctuations in the appearance of visual stimuli) from beliefs about visibility (manipulated with occlusion only), independently measuring the effects of each on perceptual decisions and confidence in presence and absence. We show that behavioural asymmetries in perceptual detection naturally emerge in an ideal observer model when only presence, but not absence, is positively represented in sensory channels. Using the same ideal-observer model, we show that detection decisions in the absence of a stimulus, their biases, timing, and confidence, are critically dependent on beliefs about the expected visibility of stimuli that are not physically present, broadly consistent with the *INCORP* model. Finally, qualitatively opposite behavioural effects in decisions about absence reveal reliable population heterogeneity in the incorporation of beliefs about visibility into perceptual decisions, independent of variability in visibility itself. We discuss the implications of possessing such beliefs about visibility, as well

as these qualitatively different patterns across individuals, for perception and cognition more broadly.

Results

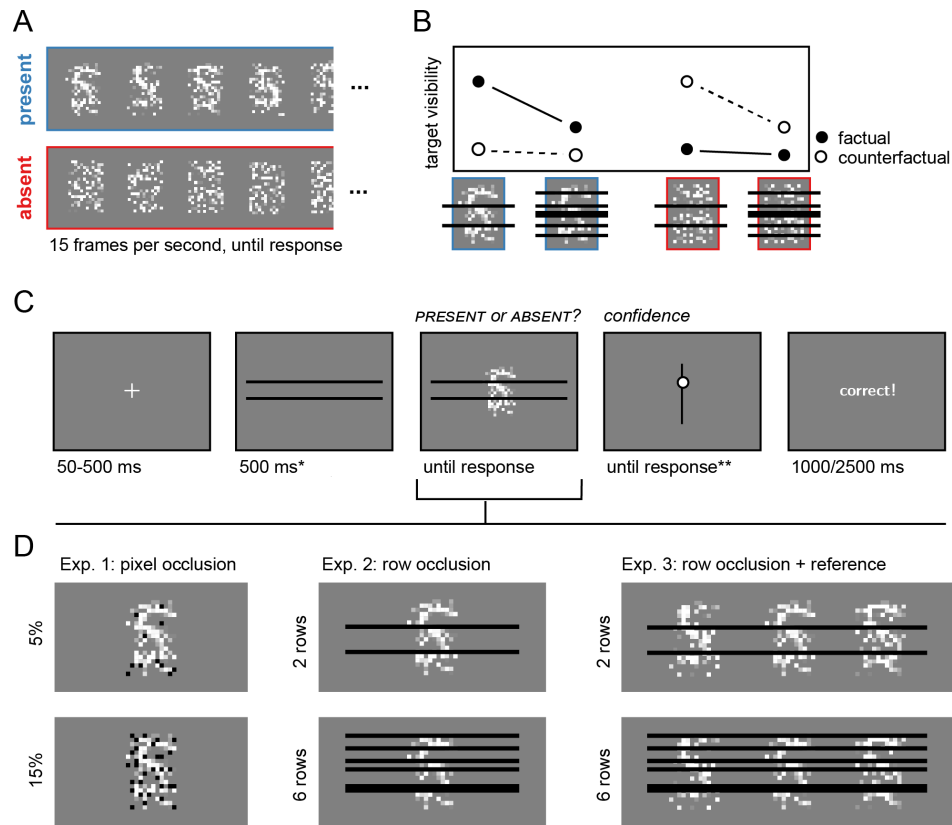
Task: visual detection under partial occlusion

In three pre-registered online experiments, participants performed a near-threshold detection task in which they made decisions about the presence or absence of a target letter (A or S, in different blocks) in a noisy, dynamic stimulus (Fig. 1A). The stimulus remained on the screen, refreshing at 15 frames per second, until a response was made. On different trials, random parts of the display were occluded by an overlaid layer of black occluders. Participants' task was to "ignore the black stuff, focus on the noise that is under it, and determine whether the letter appeared in it or not". We chose to manipulate stimulus visibility in this way because the effect of occlusion on visibility is relatively obvious: the more occluded objects are, the harder they are to see. This way, we assumed that occlusion may affect not only stimulus visibility, but also beliefs about visibility, in turn affecting perceptual decisions even when a target is absent (Fig. 1B). In contrast, random fluctuations in stimulus appearance are unpredictable and cannot be modeled in participants' beliefs about the perceptual likelihood function. As a result, we reasoned that these fluctuations should affect decisions when a target is present much more than when it is absent.

More specifically, 253 participants took part in Exp. 1, in which either 5 or 15 percent of the stimulus pixels were occluded by a static layer of randomly positioned black pixels (Fig. 1D, left panels). 252 participants took part in Exp. 2, in which we occluded 2 or 6 entire rows of pixels, and presented the occluders for an additional 500 ms before stimulus onset, to facilitate a

separation between the occluders and the noisy stimulus itself (Fig. 1D, middle panels). Exp. 2 was the only experiment in which participants also reported their confidence ratings on an analogue scale in blocks 3 and 4 (Fig. 1C). Finally, 260 participants took part in Exp. 3, in which the central stimulus was flanked by two stimuli, partly hidden behind the same row occluders, which, known to participants, always had the target in them (Fig. 1D, left panels). The rationale for this manipulation was to increase the availability of counterfactual visibility (“what would it look like if the stimulus had been present?”) in target-absent trials.

After applying our pre-registered accuracy and reaction-time based exclusion criteria, 251, 234, and 250 participants were included in the main analysis for Experiments 1, 2, and 3, respectively. Results from all pre-registered analyses are presented in the Appendix.



*Figure 1: Rationale and experimental design for Experiments 1-3. A) example frames from target-present (blue) and target-absent (red) unoccluded stimuli. B) occluding more of a target letter decreases its visibility (black markers). Occlusion has no effect on target visibility when the target is absent, but it may affect counterfactual visibility (white markers): beliefs about the expected visibility of the target, had it been present. C) trial structure in Exp. 2. D) occlusion conditions in the three experiments. In Exp. 1, on different trials we occluded a random subset of 5% or 15% of the pixels in the stimulus. In Exp. 2 and 3, on different trials we occluded a random subset of 2 or 6 pixel rows. In Exp. 3, the task-relevant stimulus was flanked by two reference stimuli that, known to the subject, always had the target letter in them. Participants performed two 32-trial blocks in which the target was the letter S and two blocks in which the target was the letter A. The order of the two letters was randomised between participants. *The occluder preview screen only appeared in Exp. 2 and 3. **Confidence ratings were given only in Exp. 2, blocks 3 and 4*

Findings: Presence-absence asymmetries

Before using decisions about absence as a critical test case for the role of beliefs about likelihood in perceptual decisions, we need to establish that these decisions are made based on

the absence of evidence for presence, and not based on direct evidence for absence. Crucially, if the absence of stimuli is perceived just like their presence, decisions about absence can be affected by occlusion simply because occlusion affects the visibility of evidence for absence (Gold & Shadlen, 2001), not because participants believe that it would affect the visibility of evidence for presence. Pronounced behavioural asymmetries between decisions about presence and absence provide initial, tentative evidence against a “direct perception of absence” account. First, correct decisions about absence were markedly slower than decisions about presence, by hundreds of milliseconds (Fig. 2A). This was true in Exp. 1 (pre-registered Hypothesis 1: 1.33 vs 1.79 seconds; $t(250) = -17.40$, $p < .001$), Exp. 2 (1.95 vs 2.33 seconds; $t(233) = -15.90$, $p < .001$), and Exp. 3 (pre-registered hypothesis 1: 1.91 vs 2.49 seconds; $t(248) = -14.64$, $p < .001$). Second, in Exp. 2, confidence ratings were significantly lower in decisions about absence (pre-registered hypothesis 7: 0.86 vs 0.84 on a 0.5 – 1 scale; $t(233) = 2.92$, $p = .004$; Fig. 2D). While one could conceive of direct perception for absence, but poorer perception of it, these data at minimum suggest that evidence accumulation for presence and absence is not a symmetric process.

Furthermore, exploratory reverse correlation analysis revealed that reaction time and confidence ratings were driven by different factors in decisions about presence versus absence. Since luminance values were randomly sampled per pixel and frame, the perceived similarity between the presented stimulus and the target letter fluctuated both within and between trials. This allowed us to directly measure the sensitivity of reaction times in decisions about presence and absence to random fluctuations in stimulus-target similarity, quantified as the Pearson correlation between unoccluded pixels and their corresponding pixels in the target letter, statistically controlling for the overall effects of target presence, occlusion level, and calibrated visibility (see Methods). Following previous reverse correlation studies of decision confidence

(Mazor, Maimon-Mor, Charles, & Fleming, 2023; Zylberberg, Barttfeld, & Sigman, 2012), we focused our analysis on the first 300 ms of the stimulus presentation, and extracted, per trial, the mean similarity between the display and the target letter in these first frames. We then computed the Spearman correlation between these trial-wise similarity measures and the reaction times, focusing our analysis on correct responses only (see Fig. 2C). Our reasoning was as follows: if perceptual evidence for both presence and absence equally contributes to perceptual detection decisions, effects of stimulus-target similarity on target-present reaction time and confidence should be mirrored by perfectly opposite effects of stimulus-target similarity in target-absent trials. If, however, only perceptual evidence for presence is represented, the negative effects of stimulus-target dissimilarity on target-absent decisions should be attenuated relative to the corresponding effects of stimulus-target similarity on target-present decisions.

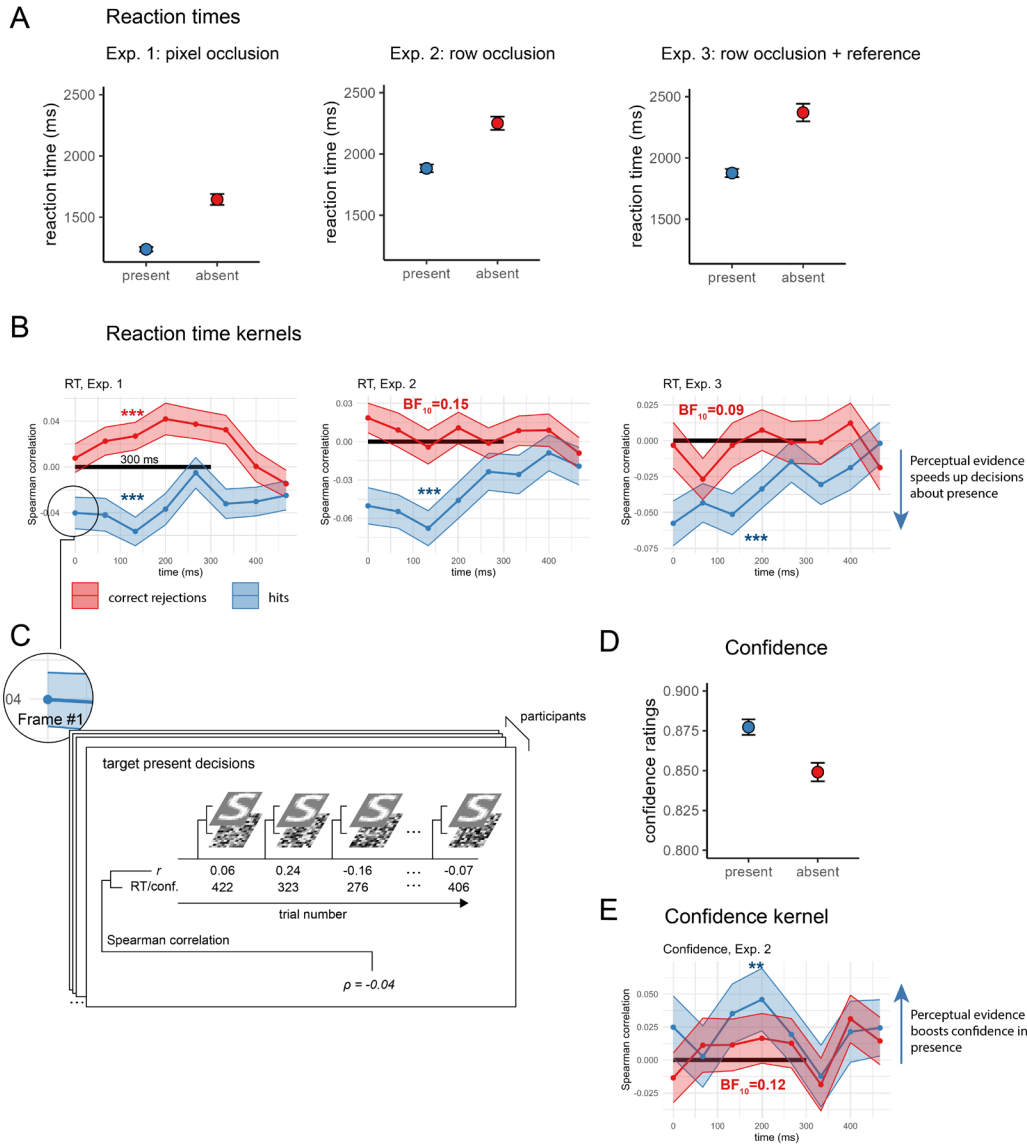


Figure 2: Presence-absence asymmetries. A) median reaction times in correct responses as a function of target presence. Error bars represent the standard error of the median, computed with bootstrapping. B) mean correlations between RT and stimulus-target similarity. Error margins are 1 standard error from the mean. C) Analysis approach, illustrated for a single frame number. For each frame, the correlation between pixel luminance values and the target served as an index of stimulus-target similarity. This correlation was mean-centered separately as a function of occlusion level, target presence, and calibrated visibility levels. We then extracted, for each frame number and participant, the correlation between these similarity measures and their corresponding trial-wise reaction times, or confidence ratings. This was done separately for correct target-present and target-absent decisions. Statistical tests were performed on average stimulus-target similarity values from the first 300 ms of the stimulus, represented by a black bar in panel B. D) mean confidence times in correct responses as a function of target presence. Error bars represent the standard error of the mean. B) mean correlations between confidence and stimulus-target similarity. ***: $p < 0.001$

As expected, higher levels of stimulus-target similarity made participants quicker to detect the target letter when it was present, and this was the case in all experiments (a one sample t-test on within-subject correlation coefficients, extracted separately for the two occlusion levels and averaged per participant, Exp. 1: $t(252) = -6.83, p < .001$; Exp. 2: $t(250) = -7.78, p < .001$; Exp. 3: $t(218) = -5.01, p < .001$; blue curves in Fig. 2B). In contrast, higher levels of stimulus-target similarity made participants slower to notice the absence of the letter when it was absent only in Exp. 1 ($t(252) = 4.77, p < .001$), but not in Exp. 2 ($t(250) = 1.25, p = .214, BF_{10} = 0.15$) and Exp. 3 ($t(219) = -0.62, p = .538, BF_{10} = 8.69 \times 10^{-2}$). In all cases, the effect of stimulus-target similarity on decision times was stronger in target-present compared to target-absent responses (Exp. 1: $t(252) = -1.68, p = .093$, Exp. 2: $t(249) = -4.62, p < .001$, Exp. 3: $t(217) = -4.84, p < .001$; red curves in Fig. 2B).

Confidence ratings in Exp. 2 further allowed us to test the relationship between stimulus-target similarity and subjective confidence, revealing a similar pattern. Confidence judgments in hits were positively correlated with stimulus-target similarity in the first 300 ms of the trial ($t(234) = 3.15, p = .002$; blue curves in Fig. 2E). In contrast, confidence in correct identifications of target absence showed no negative relationship to perceptual evidence ($t(242) = 0.79, p = .432, BF_{10} = 0.12$; red curves in Fig. 2E). Similar to reaction times, the difference between the effect of perceptual evidence on confidence in presence and the (negative) effect of perceptual evidence on confidence in absence was in itself significant ($t(232) = 3.13, p = .002$). Unlike confidence in presence, confidence in absence was not based on dissimilarity to the target letter.

An ideal observer model of visual detection

Presence-absence asymmetries in reaction time and confidence are expected if evidence is only ever available to support presence, leaving absence to be inferred tentatively and based on the absence of evidence. To formulate this asymmetry in the availability of evidence, we present a Partially Observed Markov Decision Process (POMDP, Littman, 2009) model of perceptual detection. Critically, our model has an asymmetric structure: it is equipped only with a presence-sensor, but not with an absence-sensor (Fig. 3A, right panels). We ask whether, when faced with this evidence structure, a rational agent would behave in ways that resemble the behaviour of our participants (Anderson, 1990). We provide a high-level description of the model here and a more detailed description in the Methods section. As we show, presence-absence asymmetries in decision time and decision confidence are borne out of rational evidence accumulation when the information value of evidence for presence and absence is itself asymmetrical.

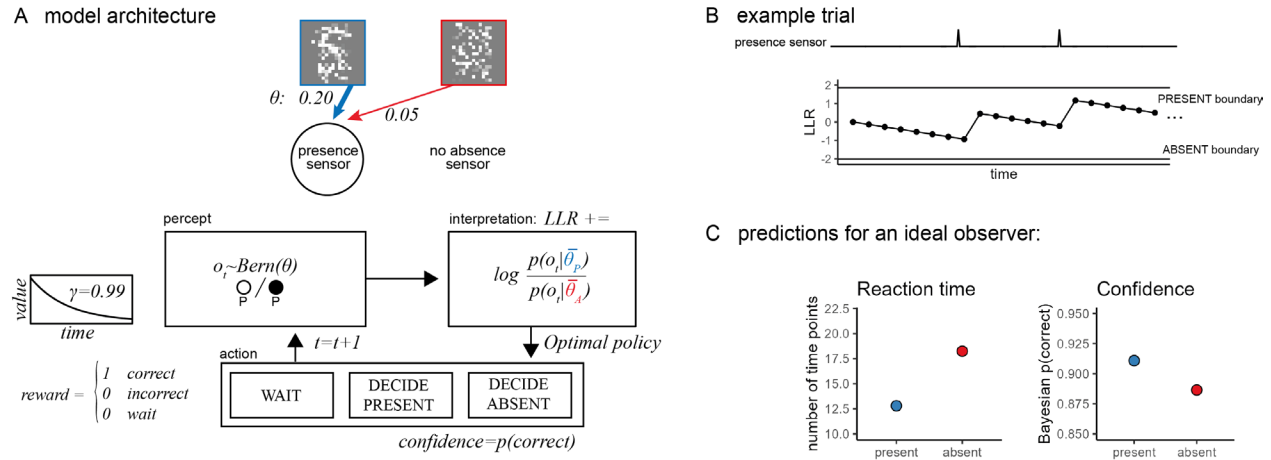


Figure 3: An ideal observer model of visual detection. A) model architecture. Target presence affects the activation probability of a presence sensor. The agent perceives a series of binary outcomes (sensor activations), based on which it attempts to guess the true world state (target presence or absence). This is done by extracting and accumulating the log likelihood ratio (LLR) for target presence versus absence and deciding whether to make a decision based on available evidence, or alternatively whether to accumulate more evidence. In making this decision, the agent balances the incentive to be as accurate as possible (only correct decisions are associated with an intrinsic reward) and the exponential discounting of the value of reward as a function of time. B) The first samples from an example trial, and their interpretation by the agent. Sensor activations are much more informative than sensor inactivations, as indicated by the shallow negative and steep positive slopes of the LLR sequence. C) behavioural predictions for an ideal observer. The model predicts that decisions about absence should be slower, and that they should be accompanied by lower levels of subjective confidence, than decisions about presence.

We model sensory observations as the binary (on/off) activations of a “presence sensor” which is probabilistically tuned to one state of the world. For example, in our illustration (Fig. 3A) the presence sensor has a higher activation probability when a target is present (0.20) than absent (0.05). The agent is intrinsically rewarded for making accurate decisions regarding the state of the world (stimulus presence or absence), given these observations. To increase the probability of being correct, the agent can choose to wait and accumulate more observations before making a decision. However, the intrinsic value of accuracy is subject to temporal

discounting, rendering the value of later correct decisions lower than that of earlier ones. Thus, from the agent's perspective, accumulating further evidence pays off exactly when the expected accuracy gain exceeds the discounting loss.

Given these settings, our agent implements an optimal policy, updating its beliefs about the state of the world by tracking the log likelihood ratio (LLR) between presence and absence given each observation, and committing to a decision only when the expected value of making a decision is higher than the expected value of making a decision later, assuming the same policy will be used in later time points. For our specified setting, the optimal policy is to commit to a “target present” decision once the accumulated LLR hits an upper boundary, commit to a “target absent” decision once the accumulated LLR hits a lower boundary, and continue to accumulate evidence otherwise (see Fig. 3B for the first time points from an example trial). As an additional measure, we assume that decision confidence equals the probability of being correct at the time of committing to a decision, given the accumulated evidence so far.

Since sensor inactivation is the more likely state both when a stimulus is present and absent, it is much less informative than sensor activation: activation is 4 times more likely when a target is present than absent, but inactivation is more likely by a factor of only 1.18 when a target is absent than present (see the smaller steps going down compared to up in Fig. 3B). This asymmetry in the information value of evidence for presence and absence results in slower decisions about absence and lower confidence levels in such decisions, compared to decisions about presence (Fig. 3C), as reported in the previous Section, and in line with the reaction time and confidence profiles found in perceptual detection experiments more broadly (Kellij, Fahrenfort, Lau, Peters, & Odegaard, 2021; Mazor, Friston, & Fleming, 2020; Mazor et al., 2023; Mazor, Moran, & Fleming, 2021; Meuwese, Loon, Lamme, & Fahrenfort, 2014).

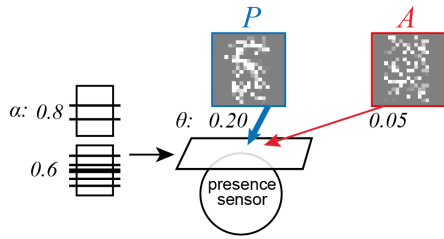
Modeling occlusion effects. In light of these marked presence-absence asymmetries in reaction times, confidence ratings, and evidence weighting, we proceed with the assumption that positive evidence for absence is not explicitly coded in sensory channels. We simulate stimulus occlusion as a scaling of the probability of sensor activation by a parameter $\alpha \in [0,1]$, such that $p(1 \mid \theta, \alpha) = \alpha\theta$. This way, α can be thought of as modulating the visibility of target-like patterns, with lower levels making the sensor less likely to activate (Fig. 4A). Importantly, in addition to the effects of α on stimulus visibility, beliefs about α (denoted $\bar{\alpha}$) also affect how sensory input is interpreted, and how much certainty agents seek before they commit to a decision. Fig. 3B illustrates the interpretation of the same sensory samples when the agent believes α to be 0.8 (corresponding to low occlusion, in black) or 0.6 (corresponding to high occlusion, in gray). Notably, the information value (measured as $|LLR|$) of sensor inactivation, but not sensor activation, is diminished when $\bar{\alpha} = 0.6$, making the same ambiguous sequence of samples appear more consistent with target presence if the display is known to be occluded.

In this model, occlusion affects the probability of obtaining positive evidence, but beliefs about occlusion have no effect on the interpretation of such evidence once obtained. This is true because, while the overall probability of obtaining positive evidence $p(1)$ diminishes with higher levels of occlusion, the *relative* probability of such evidence given target presence or absence, $\frac{p(1|present)}{p(1|absent)}$ remains unaffected. On the other hand, occlusion has little effect on the probability of obtaining negative evidence (in the form of sensor inactivation), but beliefs about the effects of occlusion affect the interpretation of such evidence once obtained. As a result, the timing and confidence of perceptual decisions in the absence of a target depend much more on beliefs about the effect of occlusion than on the true effect of occlusion on visibility.

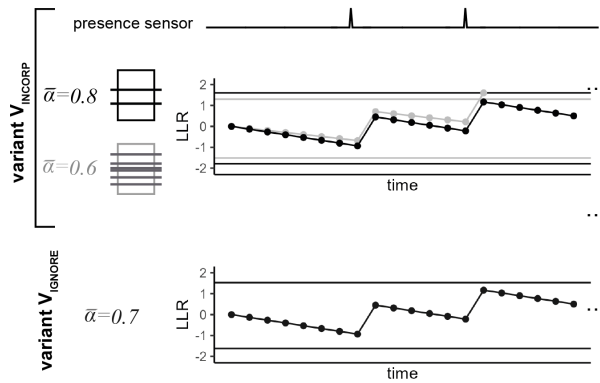
To exemplify the dissociable contributions of visibility itself (α) and beliefs about visibility ($\bar{\alpha}$) to behaviour, we consider two variants of the model (Fig. 4B). Variant V_{IGNORE} entirely ignores the expected effects of α on the probability of sensor activation, interpreting evidence in the same way in both high-occlusion and low-occlusion trials. This variant serves as our null model, in that it specified that occlusion affects perception only to the extent that it affects the activation of sensors, but not in how these activations are interpreted or accumulated over time. Crucially, this model variant can be implemented without reference to the perceptual likelihood function, by specifying weights on sensor activation and inactivation, and fixed decision thresholds.

V_{INCORP} , on the other hand, incorporates into its perceptual decisions fully accurate beliefs about the effect of α on stimulus visibility. This affects both the interpretation stage (when α is believed to be low, sensor inactivation, but not sensor activation, becomes less informative) and the action selection stage (by affecting the expected value of future evidence, making the agent more willing to settle for lower decision confidence when occlusion is high). Importantly, variant V_{INCORP} represents one point in the space of possible values $\bar{\alpha}$ can take relative to α : observers who incorporate beliefs about visibility into their perceptual decisions may underestimate the effect of occlusion on visibility, or overestimate it.

A model architecture



B example trials



C model predictions

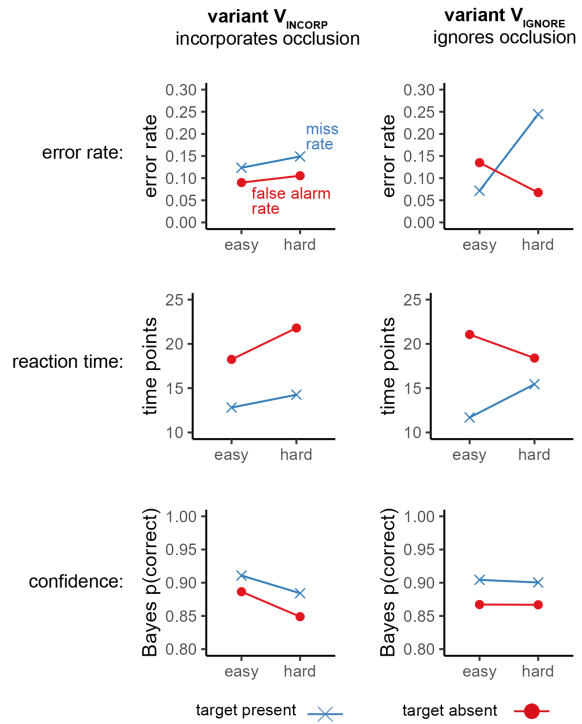


Figure 4: Modelling the effects of occlusion on visual detection. A) We model the effect of occlusion as scaling of the probability of sensor activation. B) The first sensory samples from example trials and their corresponding interpretations as a function of the believed visibility level. C) reaction time and confidence effects in simulated rational observers as a function of target presence and absence and level of occlusion, correct trials only. Variant V_{INCORP} considers the effect of occlusion when interpreting sensory evidence and making decisions, whereas variant V_{IGNORE} does not.

The two model variants predict different effects of occlusion on accuracy, decision times and confidence ratings. This is especially evident in target-absent trials (red lines in Fig. 4C). While occluding more of the display makes V_{INCORP} commit more false-alarms, it makes V_{IGNORE} make fewer of them. V_{INCORP} 's decisions about absence are slower when more of the display is occluded, whereas V_{IGNORE} 's decisions about absence are faster. Finally, V_{INCORP} is less confident in decisions about absence when more of the display is occluded, but this is not true of V_{IGNORE} . Together, both the size and direction of occlusion effects on decisions about absence are

dependent on meta-perceptual knowledge about the influence of occlusion on visibility, or the incorporation of such knowledge into perceptual decisions.

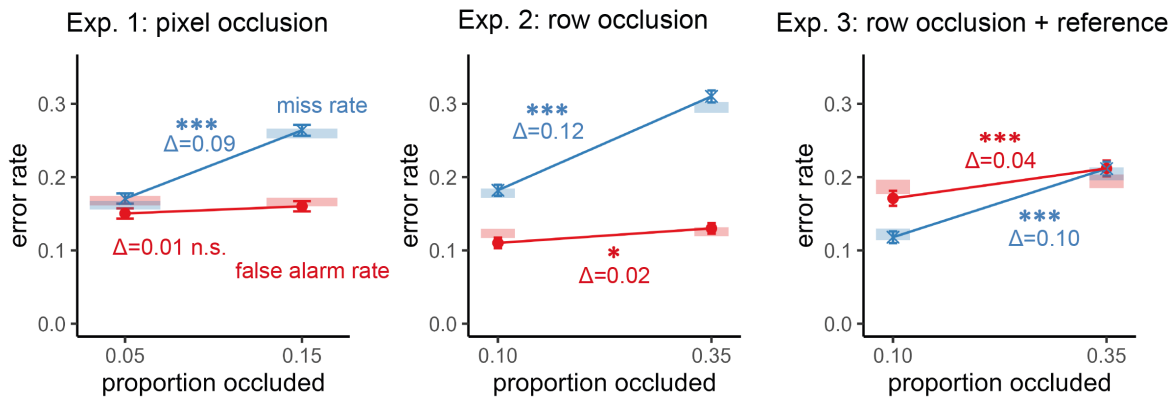
Findings: occlusion effects

Equipped with the predictions of the two model variants, we now turn to the experimental data.

Target-present trials. In all three experiments, occlusion had the expected effects on the detection of present targets (see Fig. 5A). Specifically, participants missed more targets when more of the display was occluded. In Exp. 1, the mean hit rate went down from 0.81 when 5% of the pixels were occluded to 0.72 when 15% of the pixels were occluded ($t(250) = 8.92$, $p < .001$; see Fig. 5C, blue lines). In Exp. 2, the mean hit rate went down from 0.78 when 2 rows of pixels were occluded to 0.66 when 6 rows were occluded ($t(233) = 11.83$, $p < .001$), and similar figures were obtained in Exp. 3 (from 0.85 to 0.75 in the two occlusion levels; $t(248) = 12.71$, $p < .001$).

Participants were also slower to correctly detect targets when more of the display was occluded, with a mean difference of 66 ms in Exp. 1 (pre-registered hypothesis 2, $t(250) = -3.51$, $p < .001$), 134 ms in Exp. 2 ($t(233) = -5.13$, $p < .001$), and 101 ms in Exp. 3 ($t(248) = -5.19$, $p < .001$; see Fig. 5B, blue lines). In all three experiments, this effect remained significant when incorporating incorrect trials into the analysis. Finally, in Exp. 2, confidence in presence was lower when more of the display was occluded (pre-registered hypothesis 8: 0.84 vs. 0.88 on a 0.5-1 scale; $t(233) = 9.87$, $p < .001$; see Fig. 5C, blue line). Unsurprisingly, occluding more of the target made it more difficult to spot.

A error rates



B reaction times

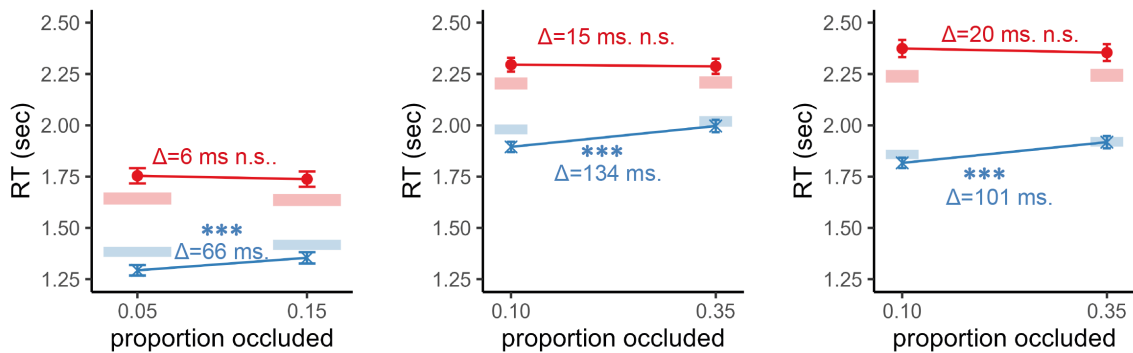
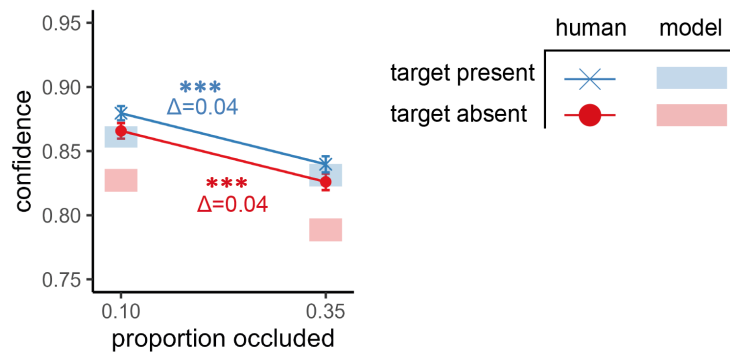
C confidence
(out of sample prediction)

Figure 5: Main results from Experiments 1-3. A) miss and false alarm rates as a function of occlusion level. B) mean median reaction times in target-present and target-absent correct responses, as a function of occlusion level. C) mean confidence in target-present and target-absent correct responses, as a function of occlusion level. Error bars represent the standard error. Semi-transparent rectangles represent data simulated from the model, fitted to accuracy and reaction time (but not confidence) data of individual participants (see model-fitting section). Rectangles are centered at the mean value, and their height is twice the standard error. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Target-absent trials. Having established that occlusion affected stimulus visibility, making responses slower, less accurate, and accompanied by lower levels of confidence when a target was present, we next examined the effects of occlusion on detection responses in the absence of a target. If participants were, like model variant V_{INCORP} , effectively incorporating counterfactual visibility into their criterion placement, we would expect to see an increase in the proportion of false alarms (the proportion of incorrect target-present reports out of all target-absent trials) when more of the target was occluded. If, however, they took evidence at face value like model variant V_{IGNORE} , occlusion should be expected to reduce the false alarm rate.

We did not observe an effect of occlusion on the false-alarm rate in Exp. 1 (from 0.17 to 0.18 when 5 or 15 percent of the stimulus pixels were occluded, $t(250) = -1.16$, $p = .249$). We therefore made the occlusion manipulation clearer to participants in Exp. 2 and 3, occluding entire rows and up to 35% of the display. An increase in the false-alarm rate with higher levels of occlusion, consistent with variant V_{INCORP} , was observed in Exp. 2 (from 0.13 to 0.15 when 2 or 6 rows were occluded, $t(233) = -2.26$, $p = .025$) and 3 (from 0.19 to 0.23 when 2 or 6 rows were occluded, $t(248) = -4.98$, $p < .001$). A significant increase in the effect of occlusion on the false alarm rates between Exp. 2 and 3 (a between-subject t-test, $t(480.40) = -2.09$, $p = .037$) is consistent with the use of beliefs about visibility to make decisions in the absence of a target: in Exp. 3, but not in Exp. 2, the central stimulus was flanked by two target-present stimuli which were hidden behind the same occluders, making the effect of occlusion on visibility visually available even in target-absent trials (see Fig. 1D, right panel).

In line with the predictions for variant V_{INCORP} , but not V_{IGNORE} , confidence in absence was lower when more of the display was occluded (pre-registered hypothesis 9: 0.83 vs. 0.87; $t(233) = 10.54$, $p < .001$). Furthermore, we find no difference between the effects of occlusion

on confidence as a function of target presence (pre-registered hypothesis 10: $t(233) = 0.01$, $p = .992$, $BF_{10} = 7.32 \times 10^{-2}$; see Fig. 5D). Participants were less confident in the absence of a target when it would have been harder to see.

Finally, the two model variants made opposite predictions for the effect of occlusion on reaction times in the absence of a target. While model variant V_{INCORP} predicted slower decisions about absence with more occlusion, model variant V_{IGNORE} predicted the opposite pattern. Intriguingly, an effect of occlusion on target-absent reaction times did not emerge in any of the three experiments. Specifically, we observed a mean difference of 6 ms in Exp. 1 (pre-registered hypothesis 3, $t(250) = 0.30$, $p = .765$; $BF_{10} = 7.39 \times 10^{-2}$), 15 ms in Exp. 2 ($t(233) = 0.79$, $p = .429$; $BF_{10} = 9.97 \times 10^{-2}$), and 20 ms in Exp. 3 ($t(248) = 0.68$, $p = .498$; $BF_{10} = 9.97 \times 10^{-2}$; $BF_{10} = 6.91 \times 10^{-2}$ when pooling data from all three experiments, reflecting strong evidence for the null hypothesis; see Fig. 5B, red lines).

Additional data reveals individual differences in occlusion effects on inference about absence

Occlusion affected the false-alarm rate and subjective confidence in a way that is consistent with the incorporation of counterfactual visibility into inferences about absence, but the absence of an effect on decision time was inconsistent with both models: model V_{INCORP} predicted a positive effect, and model V_{IGNORE} a negative one. We considered the possibility that this null group-level result may reflect population variability in the incorporation of beliefs about visibility into perceptual decisions, with some behaving more in line with the prediction of model V_{INCORP} , incorporating counterfactual visibility into their perceptual decisions about absence and slowing down when more of the display is occluded, and others more in line with the predictions

of model V_{IGNORE} , underestimating the effect of occlusion on stimulus visibility or ignoring it altogether, resulting in speedier decisions about absence for more occluded displays.

This population-mixture model predicts that despite a group-level null effect, some individual participants should show reliable effects of occlusion on “target absent” reaction times: negative for some participants, and positive for others. To test this prediction, we collected a large number of test trials from a random subset of ten participants who took part in Exp. 2 and 3 (see Fig. 6B). Over the course of five sessions we collected 896 trials per participant, with the exception of two participants in Exp. 3 for which we have 672 and 864 trials.

The high number of trials per participant allowed us to quantify the consistency of the effect of occlusion on target-absent RTs within individual participants. For each participant, we compared their target-absent response times in high- and low- occlusion trials with a t-test. If decision times were invariant to the effect of stimulus occlusion, this would be expected to result in a significant test statistic in 1 out of 20 participants, on average, corresponding to our significance level of 0.05. Strikingly, however, out of 20 participants the effect of occlusion on “target absent” decision times was significant in 8, split exactly half-half between significant positive effects (more consistent with model variant V_{INCORP}) and significant negative effects (more consistent with model variant V_{IGNORE}): much higher than the 1/20 probability expected by chance alone ($p < 0.001$ in a binomial test against $p = 0.05$).

As a more sensitive test of effect reliability, we employed the non-parametric *sign-consistency test* (Yaron, Faivre, Mudrik, & Mazor, 2023): randomly splitting individual participants’ trials into two subsets, and asking whether both subsets demonstrate the same type of outcome: either positive or negative (see Fig. 6A and methods). The group-level mean sign-

consistency, or the proportion of these random splits where the same outcome is observed in both subsets, is then compared against a bootstrapped null distribution to obtain a group-level p-value.

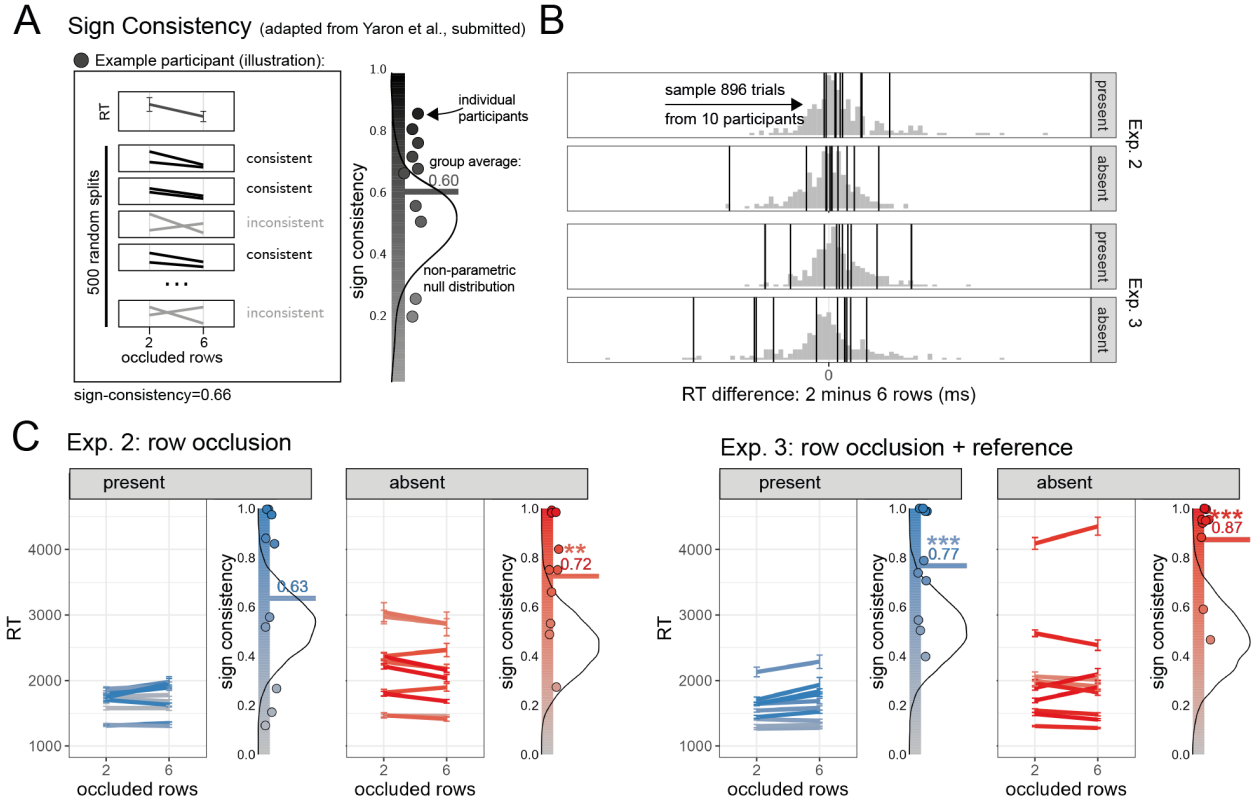


Figure 6: Sign consistency analysis. A) An illustration of the sign-consistency test, for a hypothetical participant. Sign consistency is the proportion of random splits, out of 500, for which both trial subsets show the same qualitative effect. Individual sign-consistency scores are then averaged and compared against a non-parametric null-distribution to obtain a p-value. B) Occlusion effect distributions in Exp. 2 and 3. In order to obtain sufficient statistical power, we collected hundreds of trials from a random subset of 10 participants (marked with vertical lines). C) Sign consistency results. Within each panel, we present median RT as a function of occlusion level for each participant on the left. Color saturation indicates sign-consistency. On the right, we present individual sign-consistency scores as circles, alongside the group-average sign consistency score (horizontal line), overlaid on top of the non-parametric null distribution. In both experiments, group-level sign-consistency was significantly above chance for the effect of occlusion on response-time in target-absent trials. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

In both experiments we find clear evidence for above-chance sign-consistency in the effects of occlusion on reaction times in target-absent trials (Exp. 2: sign consistency=0.72, $p = .003$; Exp. 3: sign consistency=0.86, $p < .001$; see Fig. 6C). Moreover, target-absent sign-

consistency scores were not significantly different from, and numerically higher than, target-present sign-consistency scores (Exp. 2: sign consistency=0.63, $p = .073$; Exp. 3: sign consistency=0.76, $p < .001$). These data provide clear evidence that an effect of counterfactual visibility on “target absent” response times was not absent in Experiments 2 and 3: it was masked by differences between individual participants who systematically exhibit positive and negative effects.

Model fitting reveals individual differences in the incorporation of expected visibility into perceptual decisions

In order to map this behavioural variability onto the model parameter space, we fitted model parameters to the response and response time data of participants (see Methods for details about the model fit procedure and Appendix for parameter recovery results). Of note, model fits were free to independently vary in the physical visibility of stimuli (parameters θ and α) and in participants’ beliefs regarding these quantities (parameters $\bar{\theta}$ and $\bar{\alpha}$).

As shown in Fig. 5 (semi-transparent rectangles), the model captures key aspects in the data, including the difference in reaction times as a function of target presence or absence, the increase in “target present” reaction times as a function of occlusion, and the invariance of “target absent” reaction times to occlusion. A constrained variant in which occlusion affected visibility, but not beliefs about visibility, fitted the data relatively well in target-present trials, predicting an increase in RT and in the error rate, but it made incorrect predictions about target-absent trials, predicting a decrease in RT and a reduction in the false-alarm rate (see Appendix). Notably, the full model captures the overall bias to report absence in Exp. 2 (0.56 of all observed and simulated responses) and presence in Exp. 3 (0.51 of all simulated responses, 0.52 of all observed responses) although the prior probability of target absence is assumed to be known to be 0.5 in

both experiments, and the incentive structure is fully symmetric with respect to false alarms and misses. A response bias emerges due to asymmetries in the likelihood function, and the different information value of evidence for presence versus absence.

Some aspects of the data were not captured by our model, including the group-level effect of occlusion on the false alarm rate in Exp. 2 and 3 and the relatively slower error trials (in the Appendix we show that allowing visibility and beliefs about visibility to vary between trials can account for the relationship between accuracy and reaction time). Furthermore, while our model captured the fact that “target-absent” responses are overall slower, it underestimated this effect (see Fig. 5B), suggesting that other processes, perhaps outside perception (Beltrán, Liu, & Vega, 2021; Zang, Vega, Fu, Wang, & Beltrán, 2022), contribute to the difference in response times between target-present and target-absent responses. However, this seems a valid assumption.

The model successfully captured some, but not all, population variability in the effects of occlusion on error rates and reaction times. Specifically, the correlation between the effect of occlusion on target-absent decision times in human data and in simulated data, generated using the parameters fitted to individual subjects, was $r = .34$, 95% CI [.22, .44] in Exp. 1, $r = .54$, 95% CI [.45, .63] in Exp. 2, and $r = .49$, 95% CI [.39, .58] in Exp. 3.

Inspecting the fitted model parameters revealed that overall, α and $\bar{\alpha}$ were correlated across individuals (Exp. 1: $r = .61$, 95% CI [.52, .68]; Exp. 2: $r = .56$, 95% CI [.47, .64]; Exp. 3: $r = .67$, 95% CI [.60, .73]), meaning participants’ beliefs about the effects of occlusion on visibility were proportional to the true effect of occlusion on stimulus visibility. Importantly, despite this strong alignment, participants had an overall tendency to act in accordance with a belief that occlusion affected visibility to a lesser degree than its true effect (Exp. 1: $t(250) = 5.56$, $p < .001$; Exp. 2: $t(233) = 6.72$, $p < .001$; Exp. 3: $t(248) = 5.04$, $p < .001$). While

occlusion had a similar effect on α in Exp. 2 and 3 ($t(475.56) = 0.59, p = .552$), the added reference stimuli in Exp. 3 affected $\bar{\alpha}$, bringing it closer to α itself (a contrast between $|\alpha - \bar{\alpha}|$ in Exp. 2 and 3: $t(461.16) = 2.14, p = .033$). This is in line with the theoretical interpretation of these two model parameters as being based in the true and believed effects of occlusion on visibility, respectively.

Finally, we compared the effects of α (true visibility) and $\bar{\alpha}$ (beliefs about visibility) on accuracy and reaction times in target-present and target-absent trials by extracting correlations between fitted parameters and individual-level contrasts of interest. A clear picture emerges: while α explains more variance than $\bar{\alpha}$ in decision accuracy and decision times in target-present trials (first two columns in Table 1), the opposite is true for target-absent trials (last two columns in Table 1). Together, model fit results confirm that beliefs about visibility play a role in perceptual decisions, that this is especially true in decisions about target absence, and that this is subject to significant population variability.

	exp	misses	RT present	false alarms	RT absent
α	1	0.68***	-0.15*	-0.036	0.057
	2	0.7***	-0.19**	-0.11	-0.052
	3	0.53***	-0.17**	-0.068	0.052
$\bar{\alpha}$	1	0.14*	-0.036	0.33***	-0.088
	2	0.12	-0.041	0.44***	-0.22***
	3	0.059	0.016	0.38***	-0.11

Table 1: Correlations between model parameters and occlusion effects on accuracy and reaction times. Darker shades of blue indicate lower p-values. The visibility parameter is correlated with occlusion effects when a target is present (first two columns), but the parameter controlling beliefs about visibility is correlated with occlusion effects when a target is absent (left two columns).

Discussion

Much focus has been placed on the role of prior expectations in perceptual inference (Kok, Brouwer, Gerven, & de Lange, 2013; Powers, Mathys, & Corlett, 2017; Press, Kok, & Yon, 2020; Summerfield & Egner, 2009; Weilhhammer, Stuke, Sterzer, & Schmack, 2018; Yon et al., 2023; Yon, Zainzinger, de Lange, Eimer, & Press, 2021), with important discussions regarding the (im)penetrability of visual perception to such effects from cognition (Firestone & Scholl, 2016; Pylyshyn, 1999) and potential implications for models of delusions and schizophrenia (Corlett et al., 2019; Haarsma, Deveci, Corbin, Callaghan, & Kok, 2023; Powers et al., 2017; Stuke, Weilhhammer, Sterzer, & Schmack, 2019). Here we focus on the other component of Bayesian reasoning, often neglected in such discussions: beliefs about the likelihood function going from world states to sensory input. Unlike prior expectations about the world (e.g., the probability that someone would be knocking on my door), beliefs about the likelihood function describe the perceptual system itself (e.g., the probability that I would be able to hear the knock if

someone was knocking on my door). Previous work postulated a role for such meta-perceptual beliefs in metacognitive confidence judgments (Hellmann, Zehetleitner, & Rausch, 2023; Olawole-Scott & Yon, 2023; Rausch, Hellmann, & Zehetleitner, 2018), but not in perceptual decisions proper. Focusing on a visual detection task, and specifically on trials in which no target was present, we show that beliefs about the perceptual likelihood function affect decision times and decision criteria of the detection judgments themselves. In short, beliefs about perception influence perceptual inferences.

Our novel ideal observer model of perceptual detection, formalized as a POMDP, successfully accounts for key signatures of perceptual detection by formalizing the idea that decisions about presence are made once a target is perceived, whereas decisions about absence are made once the subject believes that the target would be perceived, if present. In doing so, the model distinguishes between two classes of parameters to describe actual visibility and beliefs about visibility, with the first set contributing mostly to decisions in the presence of a target, and the second having a stronger influence on decisions when a target is absent. With these parameters, the model accounts for reliable heterogeneity in the effect of partial occlusion on individual participants' decisions as revealing differences in metacognitive beliefs about the manipulation, or in the tendency to incorporate these beliefs into the perceptual decision making process.

Importantly, our model cannot decide between these two equally valid interpretations. Indeed, knowing that occlusion affects stimulus visibility is a precondition for rationally incorporating this knowledge into the decision-making process. While inaccurate meta-perceptual knowledge about occlusion may seem like an unlikely account, an increase in the effect of occlusion on the false alarm rate between Exp. 2 and 3 indicates that having direct access to the

effects of occlusion on stimulus visibility does facilitate the use of counterfactuals in perception. Importantly, however, the fact that target-absent decision times in Exp. 3 were subject to the same population variability, with some participants making reliably faster decisions about absence when more of the display was occluded, suggest that incomplete meta-perceptual knowledge cannot be the full explanation. Some participants failed to use beliefs about counterfactual visibility even when it was directly presented to them.

Our findings fit with inferential, “inverse optics” accounts of perception, according to which vision is the inversion of an internal generative model of how world states translate to perceptual states, in light of noisy sensory data (Alhazen & Smith, 2001; Friston, 2010; Gershman, Vul, & Tenenbaum, 2012; Helmholtz, 1866). Given our participants’ overall successful incorporation of beliefs about occlusion into perceptual decisions, previous reports of a failure to adjust a detection criterion as a function of expected visibility are more likely to reflect limited meta-perceptual knowledge (for example, of the lower vision acuity in the visual periphery, Solovey, Graney, & Lau, 2015), or a limited ability to use recently acquired knowledge in a flexible manner (Gorea & Sagi, 2000), rather than a blanket inability to incorporate beliefs about the perceptual likelihood function into perceptual decisions.

Our new model raises some questions for the ways in which perceptual detection decisions are typically modelled and conceptualized. Drift diffusion models, for example, often conceive of presence and absence evidence accumulation as a symmetric process, where evidence is similarly accumulated for the two options (Gold & Shadlen, 2001; Ratcliff & McKoon, 2008; Ratcliff, Smith, Brown, & McKoon, 2016). Moreover, the parameters of such models— the drift rate, starting point bias and the boundary separation, do not explicitly correspond to the agent’s beliefs and preferences, but to the nuts and bolts of the decision-making process (which can be linked to

beliefs and desires, assuming optimal decision-making, Moran, 2015). Our ideal observer model provides such a description in terms of the subject's beliefs and goals, allowing for a separation between the perceptual likelihood function and the agents' beliefs about this function. Our findings suggest that these beliefs must be incorporated into models of perceptual decisions, and that they affect not only participants' decision threshold, but also the interpretation of incoming evidence. We provide a model and accompanying code which makes explicit the separation between perceptual input and its interpretation, with the hope that it can prove useful in modelling of perceptual tasks more broadly.

We see this work as establishing that many—but possibly not all—individuals use beliefs about expected or counterfactual visibility to inform their perceptual decisions, and that traces of these beliefs can be identified in the way detection decisions are made, especially in the absence of a target. For our purpose here, we used the most obvious visibility manipulation that we could think of: partial stimulus occlusion. A promising direction for future research would be to use target-absent trials to infer what people know and believe about their own perception. For example, it may turn out that some manipulations affect visibility without affecting expected visibility, or vice versa: affecting expected visibility with no real effect on visibility. This way, inferences in the absence of a target can provide a window into implicit metacognitive knowledge about perception (Mazor, 2021).

Finally, the robust individual differences we reveal here raise fundamental questions for future research. Does a tendency to incorporate beliefs about visibility into perceptual decision making, specifically when no stimulus is in fact visible, covary with other aspects of perception and cognition apart from the ones we observe here? Specifically, does it relate to the tendency to consider counterfactuals outside perception, for example, in making inferences based on vignettes

(Byrne & Tasso, 1999), or from the absence of evidence (Hsu, Horng, Griffiths, & Chater, 2017)? Such covariation would imply some domain generality to mechanisms supporting counterfactual representation. Does it covary with susceptibility to other effects of beliefs on perception, for example in cue-stimulus conditioning (Kok et al., 2013; Powers et al., 2017; Press et al., 2020), suggesting that beliefs about the likelihood and content of sensations are mediated by similar mechanisms? Are those participants whose behaviour suggests less incorporation of meta-perceptual beliefs simply less rational, or can their disregard of beliefs about visibility be especially adapted to some perceptual and environmental settings, for examples ones where the perceptual likelihood function is unpredictable or unstable? We are eager to find out the answers to these questions as future research will elucidate the theoretical significance of these individual differences.

Conclusion

Overall, analysis of decision criteria, reaction times and decision confidence, followed by a more focused examination of individual differences, indicate that people generally take into account beliefs about visibility when making perceptual detection judgments and when rating their subjective confidence in such decisions. Furthermore, the incorporation of beliefs about visibility into perceptual decisions is subject to substantial variability, independent of variability in physical visibility itself. This variability is especially evident in decisions when a target is absent. Our novel model of perceptual detection fits these data, and provides a useful tool that importantly extends current models of perceptual decisions to incorporate beliefs not only about the world, but, crucially, about perception itself.

Methods

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study. Experiments 1, 2, and 3 correspond to Experiments 3, 4, and 6 of a project looking at the effects of different manipulations on inference about absence. Experiments 1, 2, and 5 used a context manipulation, and will be reported separately.

Experiments 1, 2, and 3 were pre-registered prior to data collection (Exp. 1: osf.io/e6x82, Exp. 2: osf.io/5yr9e, Exp. 3: osf.io/mfd2w). The long versions of Experiments 2 and 3 were not pre-registered. To ensure pre-registration time-locking (in other words, that pre-registration preceded data collection), we employed randomization-based pre-registration. We used the SHA256 cryptographic hash function to translate our pre-registered protocol folders (Exp. 1: <https://github.com/matanmazor/counterfactualVisibility/blob/main/experiments/Exp1pixels/version2/protocolFolder.zip>; Exp. 2: https://github.com/matanmazor/reverseCorrelation/blob/cbba2d43c2ddfb0c021ee0c15b7d5b03edd34d8/experiments/Experiment2/protocol_folder.zip ; Exp. 3: <https://github.com/matanmazor/counterfactualVisibility/blob/main/experiments/Exp3reference/protocolFolder.zip>) to strings of 256 bits (protocol sums; Exp. 1: e420455976659d9a46582ea0f7a64ba9e33810d90786c5157e2a188e8dcdd7c0; Exp. 2: bf72004d226b7a89a2085b0d6238a8d9b9c638513127a47fd44c6a7d00112b2f; Exp. 3: 2be4e2548db0a221a06c936fbba47cecd28894e0400477ac4f580222b77a4a44). These bits were then combined with the unique identifiers of single subjects, and the resulting string was used as seed for initializing the Mersenne Twister pseudorandom number generator prior to determining the order and timing of experimental events. This way, experimental randomization was causally

dependent on, and therefore could not have been determined prior to, the specific contents of our pre-registration document. (Mazor, Mazor, & Mukamel, 2019).

Participants

The research complied with all relevant ethical regulations, and was approved by the Research Ethics Committee of Birkbeck, University of London (study ID number 1812000). In all experiments, participants were recruited via Prolific, and gave informed consent prior to their participation. To be eligible to take part, their Prolific approval rate had to be 95% or higher, their reported first language English, and their age between 18 and 60. Our pre-registered plan was to collect data until we reach 250, 210 and 250 participants for Exp. 1, 2, and 3, respectively. Due to an error in the pre-processing script, we ultimately collected data from 234 included participants (after applying our pre-registered exclusion criteria) in Exp. 2. We opted to keep the additional participants, noting that their inclusion does not change the pattern of the results. The experiments took ~12 minutes to complete, and participants were paid according to an hourly wage of £7.50.

For the long versions of Exp. 2 and 3, we contacted all participants who had accuracy of 70% or higher, and who did not require more than one iteration over the instructions before passing the comprehension check. The first 10 participants from each study to accept our invitation were invited to take part in 5 20-minute experiments, which they could complete in their own free time.

Procedure

Participants detected the presence or absence of a target letter (S or A, in different blocks) in a patch of dynamic grayscale noise presented at 15 frames per second. In each frame, noise

was generated by randomly sampling grayscale values from a target image I . Specifically, for each pixel S_{ij} , we displayed the grayscale value for the corresponding pixel in the original, noise-free, image I_{ij} with some probability p , and the grayscale value of a randomly chosen pixel $I_{i'j'}$, (sampled with replacement) with probability $1 - p$. On target-absent trials, p was set to 0, such that grayscale values of all pixels were randomly shuffled, with replacement. On target-present trials, the probability p was set to a positive number between 0 and 1. In Exp. 1 and 2, p was calibrated online to achieve performance levels of around 80%, following a 1-up-3-down procedure, starting at $v = 0.35$ and following a multiplicative set size of 0.9, which moved closer to 1 following each change direction in the calibration process. In Exp. 3, p was set to 0.3 throughout the entire experiment. Responses were delivered using the F and G keyboard keys (counterbalancing response mapping across subjects).

After reading the instructions, participants completed four practice trials. In case their accuracy in these four practice trials fell below 3/4, they were reminded of task instructions and given additional practice trials, until reaching the desired accuracy level. Otherwise, they continued to the main part of the experiment. Here, their task was exactly the same, but the noise patch was partly occluded. In Exp. 1, occluders were randomly positioned static black pixels, which covered 5% or 15% of the stimulus on different trials. In Exp. 2 and 3, occluders were randomly positioned rows of black pixels (2 or 6 rows, on different trials) which extended beyond the stimulus. In order to make clear that the occluders are not part of the main stimulus, occluder rows in Exp. 2 and 3 preceded the main stimulus by 500 ms. Finally, in Exp. 3, two similar “reference” stimuli were presented on both sides of the central stimulus. In these reference stimuli, the target letter was always presented with $p = 0.3$ regardless of the presence of a letter in the central stimulus. Participants were explained that they should respond based on

the central stimulus only, and continued to the main part of the experiment only once they had passed a comprehension check.

The main part of the experiment comprised four blocks of 16 trials. For approximately half of the participants, in blocks 1 and 2 the target letter was S and in blocks 3 and 4 it was A. The order of letters was reversed for the other half. In blocks 3 and 4 of Exp. 2, participants used their mouse to rate their confidence on a vertical analog scale immediately after deciding whether the letter was present or absent. To move on to the third block, participants had to respond correctly on at least 3 out of 4 trials, and to correctly answer a multiple-option comprehension question about the use of the confidence scale.

Finally, at the end of Exp. 2, participants were asked to report whether occlusion affected how difficult it was to detect the letter. 197 participants reported that occluding more of the stimulus made detecting the target letter harder, 5 reported it made detecting the target letter easier, and the remaining 32 reported it had no effect on difficulty.

Data exclusion

We followed our pre-registered exclusion criteria. Participants were excluded if their accuracy fell below 50%, and for having extremely fast or slow reaction times in more than 25% of the trials. Too fast reaction times were defined as below 100 milliseconds in all experiments. Too slow reaction times were defined as above 5 seconds in Exp. 1 and 2, and above 7 seconds in Exp. 3 and in the long versions of Exp. 2 and 3.

Trials with too slow or too fast response times according to the above criteria were excluded from the response time analysis.

Reverse correlation analysis

For each individual frame, we computed the correlation between pixel luminance values and the corresponding values in the target image. Occluded pixels were omitted from both stimulus and target image. This resulted in an uncorrected stimulus-target similarity metric, which was naturally higher for frames from target-present trials, and for frames with higher levels of stimulus visibility (p , see Procedure subsection). We therefore mean-centered these correlation values as a function of stimulus presence, occlusion and p , to obtain an unbiased measure of stimulus-target similarity r .

For each individual subject, we averaged these frame-wise correlations across frames 1-5, roughly corresponding to the first 300 ms of the trial. We then extracted, for each participant, the Spearman correlation between these average values and trial-wise reaction times. This was done separately for target-present and target-absent trials. These subject-level correlations were then subjected to a group-level t-test.

In Exp. 2, a similar procedure was used to reveal correlations between stimulus-target similarity and subjective confidence. For each participant, we extracted the Spearman correlation between trial-wise values of r , averaged across frames 1-5, and trial-wise confidence ratings, separately for target-present and target-absent trials. We then subjected the resulting correlations to a group-level t-test.

Model simulations

Model specification

A POMDP is a 7-tuple: $\langle \mathcal{S}, \mathcal{A}, \mathcal{T}, \Omega, \mathcal{O}, \mathcal{r}, \gamma \rangle$. The state space \mathcal{S} comprises two states describing target presence or absence and two additional states for trial endings: correct and incorrect. The action space \mathcal{A} has three possible actions: “wait”, “decide present”, and “decide absent”. The transition function $\mathcal{T}: (\mathcal{S}, \mathcal{A}) \rightarrow \mathcal{S}$ specifies the effect of actions on state transitions. “wait” maps states to themselves, and deciding maps states to the terminal “correct” or “incorrect” states depending on the accuracy of the decision, which have no associated actions with them. Ω is the set of possible observations. We assume these are $[0,1]$, that is, perceptual evidence has a binary form. $\mathcal{O}: \mathcal{S} \rightarrow P(\Omega)$ is a probabilistic function from states to observations, which we describe in more detail below. $\mathcal{r}: \mathcal{S} \rightarrow R$ maps states to reward values. We set the values of all states to 0, except “correct” which is associated with a value of 1. Finally, the temporal discount factor γ affects the subjective value of anticipated rewards. We set $\gamma = 0.99$, meaning that a reward obtained in the next time point is worth 0.99 of its worth if obtained now.

The observation function \mathcal{O} is a Bernoulli function, such that the probability of observing 1 equals the bias parameter θ which depends on target presence. Specifically, we set

$$\theta := \begin{cases} 0.05 & \text{absent} \\ 0.2 & \text{present} \end{cases}$$

Importantly, for any choice of θ such that $0 < \theta_{\text{absent}} < \theta_{\text{present}} < 0.5$, positive evidence (that is, sampling a 1) is more informative than negative evidence (that is, sampling a 0). For example, for the values we use here, after sampling a 0 an agent should update their

subjective belief that a target is present only by a small amount, from 0.5 to 0.46. In contrast, after sampling a single 1, belief update is much steeper: from 0.5 to 0.8.

Agents need to infer target presence from noisy observations. Their belief state can therefore be described as the log likelihood ratio LLR between target presence and absence, which they update following each sample.

$$LLR_t = \sum_{i=1}^t \log \frac{p(o_i | \bar{\theta}_{presence})}{p(o_i | \bar{\theta}_{absence})}$$

Where

$$p(o_i | \bar{\theta}) = \begin{cases} \bar{\theta} & \text{if } o_i = 1 \\ 1 - \bar{\theta} & \text{if } o_i = 0 \end{cases}$$

With $\bar{\theta}$ being the assumed value of θ in the agent's internal model of their perception (in all our simulations, $\bar{\theta} = \theta$). The probability that a target is present given the evidence so far is then:

$$p(present | O_t) = \frac{e^{LLR_t}}{1 + e^{LLR_t}}$$

With O_t being the entire stream of evidence until time point t . And, assuming that, at the time of committing to a decision, the agent decides “present” if and only if $p(present | O) > 0.5$, the probability of being correct at that time point is:

$$p(correct | DECIDE, O_t) = \max(p(present | O_t), 1 - p(present | O_t))$$

When following the optimal policy, the expected value at time point t equals the maximum of 1) the probability of being correct if decision is taken now, and 2), the expected value of waiting and collecting additional evidence, discounted by the temporal discount factor γ :

$$E(V|O_t) = \max(p(\text{correct}|O_t), p(1|O_t)\gamma E(V|[O_t, 1]) + p(0|O_t)\gamma E(V|[O_t, 0]))$$

Where $E(V|[O_t, 1])$ is the expected value at time point $t + 1$, assuming the next sample is 1, and $p(1|O_t) = p(\text{present}|O_t)\bar{\theta}_{\text{present}} + p(\text{absent}|O_t)\bar{\theta}_{\text{absent}}$ is the probability that the next sample will be 1, marginalized over target presence and absence (similar for 0). The optimal action at time t is determined by the maximizing term (deciding now or waiting).

Finally, confidence ratings are modeled as the estimated probability of being correct when committing to a decision.

Occlusion effects

We simulate stimulus occlusion as a scaling of the probability of obtaining positive evidence by a parameter $\alpha \in [0,1]$. Similar to θ_{present} and θ_{absent} , α is paralleled by a metacognitive variable, $\bar{\alpha}$, which corresponds to participants' beliefs about the effects of occlusion on stimulus visibility. This way of defining occlusion has three notable characteristics. First, the relative effect of occlusion on the probability of sampling a 1 (α) is much more pronounced than its positive effect on the probability of sampling a 0 ($\frac{1-\alpha\theta}{1-\theta}$). For example, for the case of $\theta = 0.1$ and $\alpha = 0.7$, occlusion reduces the probability of sampling a 1 by a factor of 1.43, but increases the probability of sampling a 0 by a factor of 1.03 only.

Second, the informativeness of obtaining positive evidence, quantified as the log likelihood ratio between target presence and absence following a 1, is unaffected by beliefs about the effects of occlusion on visibility, $\bar{\alpha}$:

$$LLR_{[1]} = \log \frac{p(1|present)}{p(1|absent)} = \log \frac{\bar{\alpha}\bar{\theta}_{present}}{\bar{\alpha}\bar{\theta}_{absent}} = \log \frac{\bar{\theta}_{present}}{\bar{\theta}_{absent}}$$

And third, the informativeness of obtaining negative evidence, quantified as the log likelihood ratio between target presence and absence following a 0, approaches 0 with lower values of $\bar{\alpha}$, as if the model considers the probability that evidence would have been obtained if a target was present:

$$\begin{aligned} |LLR_{[0]}| &= \left| \log \frac{p(0|present)}{p(0|absent)} \right| = \\ &= \left| \log \frac{1 - \bar{\alpha}\bar{\theta}_{present}}{1 - \bar{\alpha}\bar{\theta}_{absent}} \right| < \left| \log \frac{1 - \bar{\theta}_{present}}{1 - \bar{\theta}_{absent}} \right| \end{aligned}$$

Together, we get a double dissociation. Occlusion affects the probability of obtaining positive evidence, but beliefs about occlusion have no effect on the interpretation of such evidence once obtained. On the other hand, occlusion has little effect on the probability of obtaining negative evidence, but beliefs about the effects of occlusion affect the interpretation of such evidence once obtained. As a result, timing and confidence in decisions about absence depend much more on beliefs about the effect of occlusion than on the true effect of occlusion on visibility.

In the simulations we had two occlusion levels; one where $\alpha = 0.8$ (easy condition) and one where $\alpha = 0.0.6$ (hard condition). We present the results of two simulated agents: V_{INCORP} is an ideal observer who uses information about the expected effect of occlusion on visibility to

interpret data and make decisions ($\bar{\alpha} = \alpha$), and V_{IGNORE} is an observer who interprets perceptual evidence similarly in both levels of occlusion ($\bar{\alpha} = 0.7$ for both hard and easy conditions). For both agents, we found the optimal policy (given their beliefs) using backward induction, and simulated 4000 trials to obtain predictions.

Model fitting

Model parameters were fitted to the behaviour of individual participants. 10 model parameters were included:

1. $\theta_{absent} = p(1|absent)$
2. $\theta_{\Delta} = p(1|present) - p(1|absent)$
3. $\bar{\theta}_{absent}$
4. $\bar{\theta}_{\Delta}$

Parameters 1-4 were allowed to vary between 0.00005 and 0.27.

5. γ : the temporal discounting parameter. Allowed to vary between 0.989 and 0.999955.
6. Minimal non-decision time. Allowed to vary between 0.2 and 1 second.
7. Maximal non-decision time minus minimal non-decision time. Allowed to vary between 0.1 and 1 second.
8. α : the effect of occlusion on visibility. For model-fitting purposes, $p(1)$ in the easy condition was set to θ/α and in the hard condition $\theta\alpha$.
9. $\bar{\alpha}$: the believed effect of occlusion on visibility. For model-fitting purposes, here $\bar{p}(1)$ in the easy condition was $\bar{\theta}/\bar{\alpha}$ and in the hard condition $\bar{\theta}\bar{\alpha}$.

Parameters 8 and 9 were allowed to vary between 0.67 and 1.

To account for noise in the decision-making process, action selection followed a softmax distribution:

$$p(a) = \frac{\exp(v_a/T)}{\sum_a \exp(v_a/T)}$$

where v_a is the value associated with taking action a and T is the softmax temperature:

10. T . Allowed to vary between 0.0015 and 1. Notably, the model fits of most participants converged to the lowest possible value for T , indicating very little decision noise.

To aid with model fitting, parameters 1-5 were fitted in logit space and then transformed via a sigmoid function and parameters 8-10 were fitted in log space and then exponentiated.

Model fitting was carried out in Matlab (version R2023a, Optimization Toolbox). We used a combination of simulated annealing (Matlab's `simannealbnd`) and the nonlinear programming solver `fmincon`. For Exp. 1,2 and 3, we ran 12 independent optimizations per participant, starting at random points in the parameter space, and used the parameters that produced the best fits in terms of log likelihood. For the long experiments 2a and 2b, we ran 48 independent optimizations per participants. The authors would like to acknowledge the use of the University of Oxford Advanced Research Computing (ARC) facility in carrying out this work.

<http://dx.doi.org/10.5281/zenodo.22558>

Acknowledgements

This work was supported by a European Research Council (ERC) consolidator grant (101001592) under the European Union's Horizon 2020 research and innovation programme, awarded to CP. MM is supported by a post doctoral research fellowship from All Souls College at the University of Oxford. We thank Daniel Yon and Mathias Sablé-Meyer for useful feedback on previous versions of this paper.

References

- Alhazen, & Smith, A. M. (2001). *Alhacen's Theory of Visual Perception: A Critical Edition, with English Translation and Commentary, of the First Three Books of Alhacen's De Aspectibus, the Medieval Latin Version of Ibn Al-Haytham's Kitab Al-Manazir*. American Philosophical Society.
- Altman, D. G., & Bland, J. M. (1995). Absence of evidence is not evidence of absence. *BMJ : British Medical Journal*, 311(7003), 485. Retrieved from <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2550545/>
- Anderson, J. R. (1990). *The adaptive character of thought*. New York: Psychology Press.
<https://doi.org/10.4324/9780203771730>
- Beltrán, D., Liu, B., & Vega, M. de. (2021). Inhibitory Mechanisms in the Processing of Negations: A Neural Reuse Hypothesis. *Journal of Psycholinguistic Research*, 50(6), 1243–1260. <https://doi.org/10.1007/s10936-021-09796-x>
- Byrne, R. M. J., & Tasso, A. (1999). Deductive reasoning with factual, possible, and counterfactual conditionals. *Memory & Cognition*, 27(4), 726–740.
<https://doi.org/10.3758/BF03211565>
- Calder-Travis, J., Charles, L., Bogacz, R., & Yeung, N. (2024). Bayesian confidence in optimal decisions. *Psychological Review*. Retrieved from <https://ora.ox.ac.uk/objects/uuid:f5696994-8d91-472e-a7a0-f1637c5c6573>

Chun, M. M., & Wolfe, J. M. (1996). Just Say No: How Are Visual Searches Terminated When There Is No Target Present? *Cognitive Psychology*, 30(1), 39–78.

<https://doi.org/10.1006/cogp.1996.0002>

Corlett, P. R., Horga, G., Fletcher, P. C., Alderson-Day, B., Schmack, K., & Powers, A. R. (2019). Hallucinations and strong priors. *Trends in Cognitive Sciences*, 23(2), 114–127.

<https://doi.org/10.1016/j.tics.2018.12.001>

Firestone, C., & Scholl, B. J. (2016). Cognition does not affect perception: Evaluating the evidence for “top-down” effects. *Behavioral and Brain Sciences*, 39, e229.

<https://doi.org/10.1017/S0140525X15000965>

Friston, K. (2010). The free-energy principle: a unified brain theory? *Nature Reviews Neuroscience*, 11(2), 127–138. <https://doi.org/10.1038/nrn2787>

Gershman, S. J., Vul, E., & Tenenbaum, J. B. (2012). Multistability and perceptual inference. *Neural Computation*, 24(1), 1–24. https://doi.org/10.1162/NECO_a_00226

Gold, J., & Shadlen, M. N. (2001). Neural computations that underlie decisions about sensory stimuli. *Trends in Cognitive Sciences*, 5(1), 10–16. [https://doi.org/10.1016/S1364-6613\(00\)01567-9](https://doi.org/10.1016/S1364-6613(00)01567-9)

Goldberg, S. (2011). If that were true i would have heard about it by now. *Social Epistemology: Essential Readings*, 92–108.

Gorea, A., & Sagi, D. (2000). Failure to handle more than one internal representation in visual detection tasks. *Proceedings of the National Academy of Sciences*, 97(22), 12380–12384.

- Haarsma, J., Deveci, N., Corbin, N., Callaghan, M. F., & Kok, P. (2023). Expectation Cues and False Percepts Generate Stimulus-Specific Activity in Distinct Layers of the Early Visual Cortex. *Journal of Neuroscience*, 43(47), 7946–7957.
<https://doi.org/10.1523/JNEUROSCI.0998-23.2023>
- Hellmann, S., Zehetleitner, M., & Rausch, M. (2023). Simultaneous modeling of choice, confidence, and response time in visual perception. *Psychological Review*, 130(6), 1521–1543. <https://doi.org/10.1037/rev0000411>
- Helmholtz, H. von. (1866). Concerning the perceptions in general. *Treatise on Physiological Optics*,.
- Hsu, A. S., Horng, A., Griffiths, T. L., & Chater, N. (2017). When Absence of Evidence Is Evidence of Absence: Rational Inferences From Absent Data. *Cognitive Science*, 41(S5), 1155–1167. <https://doi.org/10.1111/cogs.12356>
- Kellij, S., Fahrenfort, J., Lau, H., Peters, M. A. K., & Odegaard, B. (2021). An investigation of how relative precision of target encoding influences metacognitive performance. *Attention, Perception, & Psychophysics*, 83(1), 512–524. <https://doi.org/10.3758/s13414-020-02190-0>
- Kok, P., Brouwer, G. J., Gerven, M. A. J. van, & de Lange, F. P. (2013). Prior Expectations Bias Sensory Representations in Visual Cortex. *Journal of Neuroscience*, 33(41), 16275–16284. <https://doi.org/10.1523/JNEUROSCI.0742-13.2013>
- Littman, M. L. (2009). A tutorial on partially observable markov decision processes. *Journal of Mathematical Psychology*, 53(3), 119–125. <https://doi.org/10.1016/j.jmp.2009.01.005>

- Locke, J. (1690). *An essay concerning human understanding*. <https://doi.org/10.1037/11304-008>
- Mazor, M. (2021). *Inference about absence as a window into the mental self-model*.
- Mazor, M., Friston, K. J., & Fleming, S. M. (2020). Distinct neural contributions to metacognition for detecting, but not discriminating visual stimuli. *Elife*, 9, e53900.
- Mazor, M., Maimon-Mor, R. O., Charles, L., & Fleming, S. M. (2023). Paradoxical evidence weighting in confidence judgments for detection and discrimination. *Attention, Perception, & Psychophysics*, 1–30.
- Mazor, M., Mazor, N., & Mukamel, R. (2019). A novel tool for time-locking study plans to results. *European Journal of Neuroscience*, 49(9), 1149–1156.
- Mazor, M., Moran, R., & Fleming, S. M. (2021). Stage 2 registered report: Metacognitive asymmetries in visual perception. *Neuroscience of Consciousness*, 2021(1), niab025.
- Meuwese, J. D. I., Loon, A. M. van, Lamme, V. A. F., & Fahrenfort, J. J. (2014). The subjective experience of object recognition: comparing metacognition for object detection and object categorization. *Attention, Perception, & Psychophysics*. <https://doi.org/10.3758/s13414-014-0643-1>
- Moran, R. (2015). Optimal decision making in heterogeneous and biased environments. *Psychonomic Bulletin & Review*, 22(1), 38–53. <https://doi.org/10.3758/s13423-014-0669-3>
- Oaksford, M., & Hahn, U. (2004). A bayesian approach to the argument from ignorance. *Canadian Journal of Experimental Psychology/Revue Canadienne de Psychologie Expérimentale*, 58(2), 75.

Olawole-Scott, H., & Yon, D. (2023). Expectations about precision bias metacognition and awareness. *Journal of experimental psychology General*, 152(8), 2177–2189.

<https://doi.org/10.1037/xge0001371>

Powers, A. R., Mathys, C., & Corlett, P. R. (2017). Pavlovian conditioning induced hallucinations result from overweighting of perceptual priors. *Science*, 357(6351), 596–600.

<https://doi.org/10.1126/science.aan3458>

Press, C., Kok, P., & Yon, D. (2020). The Perceptual Prediction Paradox. *Trends in Cognitive Sciences*, 24(1), 13–24. <https://doi.org/10.1016/j.tics.2019.11.003>

Pylyshyn, Z. (1999). Is vision continuous with cognition?: The case for cognitive impenetrability of visual perception. *Behavioral and Brain Sciences*, 22(3), 341–365.

<https://doi.org/10.1017/S0140525X99002022>

Ratcliff, R., & McKoon, G. (2008). The diffusion decision model: Theory and data for two-choice decision tasks. *Neural Computation*, 20(4), 873–922.

<https://doi.org/10.1162/neco.2008.12-06-420>

Ratcliff, R., Smith, P. L., Brown, S. D., & McKoon, G. (2016). Diffusion Decision Model: Current Issues and History. *Trends in Cognitive Sciences*, 20(4), 260–281.

<https://doi.org/10.1016/j.tics.2016.01.007>

Rausch, M., Hellmann, S., & Zehetleitner, M. (2018). Confidence in masked orientation judgments is informed by both evidence and visibility. *Attention, Perception, & Psychophysics*, 80(1), 134–154. <https://doi.org/10.3758/s13414-017-1431-5>

- Solovey, G., Graney, G. G., & Lau, H. (2015). A decisional account of subjective inflation of visual perception at the periphery. *Attention, Perception, & Psychophysics*, 77(1), 258–271. <https://doi.org/10.3758/s13414-014-0769-1>
- Stuke, H., Weilhhammer, V. A., Sterzer, P., & Schmack, K. (2019). Delusion proneness is linked to a reduced usage of prior beliefs in perceptual decisions. *Schizophrenia Bulletin*, 45(1), 80–86. <https://doi.org/10.1093/schbul/sbx189>
- Summerfield, C., & Egner, T. (2009). Expectation (and attention) in visual cognition. *Trends in Cognitive Sciences*, 13(9), 403–409. <https://doi.org/10.1016/j.tics.2009.06.003>
- Treisman, M., & Williams, T. C. (1984). *A Theory of Criterion Setting With an Application to Sequential Dependencies*. 44.
- Walton, D. (1992). Nonfallacious arguments from ignorance. *American Philosophical Quarterly*, 29(4), 381–387.
- Walton, D. (2010). *Arguments from ignorance*. Penn State Press.
- Weilhhammer, V. A., Stuke, H., Sterzer, P., & Schmack, K. (2018). The Neural Correlates of Hierarchical Predictions for Perceptual Decisions. *Journal of Neuroscience*, 38(21), 5008–5021. <https://doi.org/10.1523/JNEUROSCI.2901-17.2018>
- Yaron, I., Faivre, N., Mudrik, L., & Mazor, M. (2023). *Individual differences do not mask effects of unconscious processing*.
- Yon, D., Thomas, E. R., Gilbert, S. J., Lange, F. P. de, Kok, P., & Press, C. (2023). Stubborn predictions in primary visual cortex. *Journal of Cognitive Neuroscience*, 35(7), 1133–1143. https://doi.org/10.1162/jocn_a_01997

Yon, D., Zainzinger, V., de Lange, F. P., Eimer, M., & Press, C. (2021). Action biases perceptual decisions toward expected outcomes. *Journal of Experimental Psychology: General*, 150(6), 1225.

Zang, A., Vega, M. de, Fu, Y., Wang, H., & Beltrán, D. (2022). Language switching may facilitate the processing of negative responses. *Frontiers in Psychology*, 13.

<https://doi.org/10.3389/fpsyg.2022.906154>

Zylberberg, A., Barttfeld, P., & Sigman, M. (2012). The construction of confidence in a perceptual decision. *Frontiers in Integrative Neuroscience*, 6. Retrieved from

<https://www.frontiersin.org/articles/10.3389/fnint.2012.00079>

Appendix

Pre-registered analysis

Probability correct was 0.81 (SD=0.04) in Exp. 1, 0.81 (SD=0.03), and 0.82 (SD=0.10) in Exp. 3.

Hypothesis 1 (PRESENCE/ABSENCE RESPONSE TIME).

Exp. 1.

A paired t-test on the median individual level-response times revealed a significant difference between target-present and target-absent response times ($M = -454.16$, 95% CI $[-505.58, -402.75]$, $t(250) = -17.40$, $p < .001$).

Exp. 2.

A paired t-test on the median individual level-response times revealed a significant difference between target-present and target-absent response times ($M = -384.93$, 95% CI $[-432.62, -337.23]$, $t(233) = -15.90$, $p < .001$).

Exp. 3. A paired t-test on the median individual level-response times revealed a significant difference between target-present and target-absent response times ($M = -581.37$, 95% CI $[-659.58, -503.16]$, $t(248) = -14.64$, $p < .001$).

Hypothesis 2 (OCCLUSION EFFECT IN PRESENCE).

Exp. 1.

A paired t-test on the median individual level-response times in hit trials revealed a significant effect of occlusion on RT ($M = -65.95$, 95% CI $[-102.95, -28.95]$, $t(250) = -3.51$, $p < .001$).

Exp. 2. A paired t-test on the median individual level-response times in hit trials revealed a significant effect of occlusion on RT ($M = -134.34$, 95% CI $[-185.95, -82.73]$, $t(233) = -5.13$, $p < .001$).

Exp. 3. A paired t-test on the median individual level-response times in hit trials revealed a significant effect of occlusion on RT ($M = -126.41$, 95% CI $[-174.69, -78.12]$, $t(248) = -5.16$, $p < .001$).

Hypothesis 3 (OCCLUSION EFFECT IN ABSENCE).

Exp. 1.

A paired t-test on the median individual level-response times in correct rejection trials revealed no significant effect of occlusion on RT ($M = 5.88$, 95% CI $[-32.81, 44.56]$, $t(250) = 0.30$, $p = .765$).

Exp. 2. A paired t-test on the median individual level-response times in correct rejection trials revealed no significant effect of occlusion on RT ($M = 15.03$, 95% CI $[-22.34, 52.40]$, $t(233) = 0.79$, $p = .429$).

Exp. 3. A paired t-test on the median individual level-response times in correct

rejection trials revealed no significant effect of occlusion on RT ($M = -15.33$, 95% CI $[-87.84, 57.18]$, $t(248) = -0.42$, $p = .677$).

Hypothesis 4 (OCCLUSION RESPONSE INTERACTION).

Exp. 1.

We find a significant interaction between occlusion level and response on reaction times ($M = -71.82$, 95% CI $[-127.16, -16.48]$, $t(250) = -2.56$, $p = .011$).

Exp. 2.

We find a significant interaction between occlusion level and response on reaction times ($M = -149.37$, 95% CI $[-212.21, -86.53]$, $t(233) = -4.68$, $p < .001$).

Exp. 3.

We find a significant interaction between occlusion level and response on reaction times ($M = -111.08$, 95% CI $[-201.18, -20.97]$, $t(248) = -2.43$, $p = .016$).

Hypothesis 5 (SENSITIVITY).

Exp. 1.

We find a significant drop in perceptual sensitivity (d') as a function of occlusion ($M = 0.37$, 95% CI $[0.27, 0.46]$, $t(250) = 7.64$, $p < .001$).

Exp. 2.

We find a significant drop in perceptual sensitivity (d') as a function of occlusion ($M = 0.51$, 95% CI $[0.42, 0.61]$, $t(233) = 10.46$, $p < .001$).

Exp. 3.

We find a significant drop in perceptual sensitivity (d') as a function of occlusion ($M = 0.58$, 95% CI $[0.50, 0.67]$, $t(248) = 13.64$, $p < .001$).

Hypothesis 5 (CRITERION).

Exp. 1.

We find a significant increase in the signal detection criterion (c) as a function of occlusion ($M = -0.14$, 95% CI $[-0.19, -0.09]$, $t(250) = -5.46$, $p < .001$).

Exp. 2.

We find a significant increase in the signal detection criterion (c) as a

function of occlusion ($M = -0.15$, 95% CI $[-0.21, -0.10]$, $t(233) = -5.85$, $p < .001$).

Exp. 3. We find a significant increase in the signal detection criterion (c) as a function of occlusion ($M = -0.10$, 95% CI $[-0.15, -0.05]$, $t(248) = -4.02$, $p < .001$).

Hypothesis 7 (PRESENCE/ABSENCE CONFIDENCE).

Exp. 2.

A paired t-test on the mean individual-level confidence ratings from correct responses only revealed a significant effect of target presence on confidence ($M = 0.01$, 95% CI $[0.00, 0.03]$, $t(233) = 2.92$, $p = .004$).

Hypothesis 8 (OCCLUSION CONFIDENCE EFFECT IN PRESENCE).

Exp. 2.

A paired t-test on the mean individual-level confidence ratings in correct trials only revealed a significant effect of occlusion on hit reaction times ($M = 0.04$, 95% CI $[0.03, 0.05]$, $t(233) = 9.87$, $p < .001$).

Hypothesis 9 (OCCLUSION CONFIDENCE EFFECT IN ABSENCE).

Exp. 2.

A paired t-test on the mean individual-level confidence ratings in correct trials only revealed a significant effect of occlusion on correct-rejection reaction times ($M = 0.04$, 95% CI $[0.03, 0.05]$, $t(233) = 10.54$, $p < .001$).

Hypothesis 10 (OCCLUSION RESPONSE INTERACTION ON CONFIDENCE).

Exp. 2.

We find no significant interaction effect between occlusion and target presence on confidence ($M = 0.00$, 95% CI $[-0.01, 0.01]$, $t(233) = 0.01$, $p = .992$).

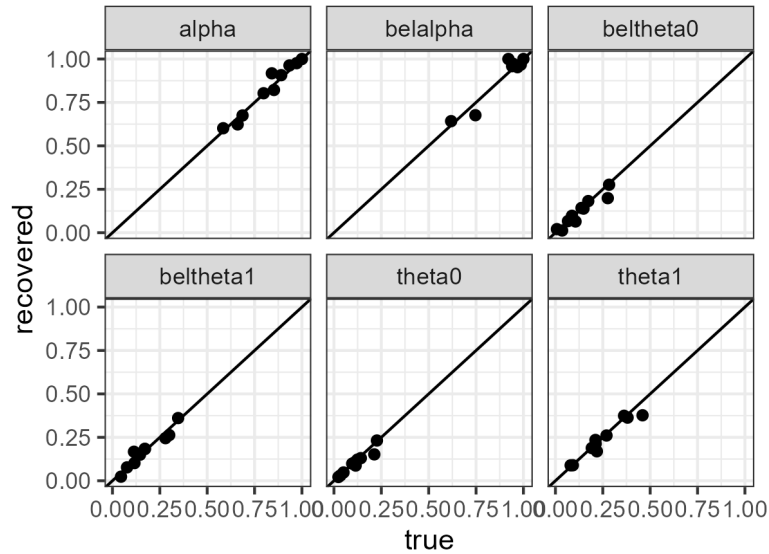
Parameter recovery

Figure A1: Parameter recovery results, Exp. 2 (long version).

To verify that model-parameters are in principle recoverable, we used the participant-specific parameters fitted to participants in the long version of Exp. 1 and simulated 896 trials per participant. We then repeated the same model-fitting procedure on these recovered parameters. The correlations between fitted and recovered parameters were generally very high for the parameters of interest (see Fig. 7). Most importantly for our purpose, the difference between $\bar{\alpha}$ and α , indicating whether participants overestimated, underestimated, or accurately estimated the effect of occlusion on target visibility, was highly recoverable ($r = .95$, 95% CI [.80, .99], $t(8) = 8.69$, $p < .001$). Furthermore, the Interecpt term linear regression model fitted to predict the recovered $\bar{\alpha} - \alpha$ from the true $\bar{\alpha} - \alpha$ was not statistically different from 0 ($t(8) = 0.41$, $p = .695$), suggesting that this term was recovered not only in high precision, but also with very little bias.

We repeated the same exercise, this time using parameters from the short version of Exp. 2 and stimulating 72 trials per participant. Due to the lower number of simulated trials, the correlations here were lower. Still, $\bar{\alpha} - \alpha$ was recoverable well above-chance, even with this very low number of trials ($r = .62$, 95% CI [.54, .69], $t(232) = 12.08$, $p < .001$). Again, the model intercept was not significantly different from 0 ($t(232) = 1.64$, $p = .102$), suggesting that the relationship between α and $\bar{\alpha}$ can be recovered without introducing a bias.

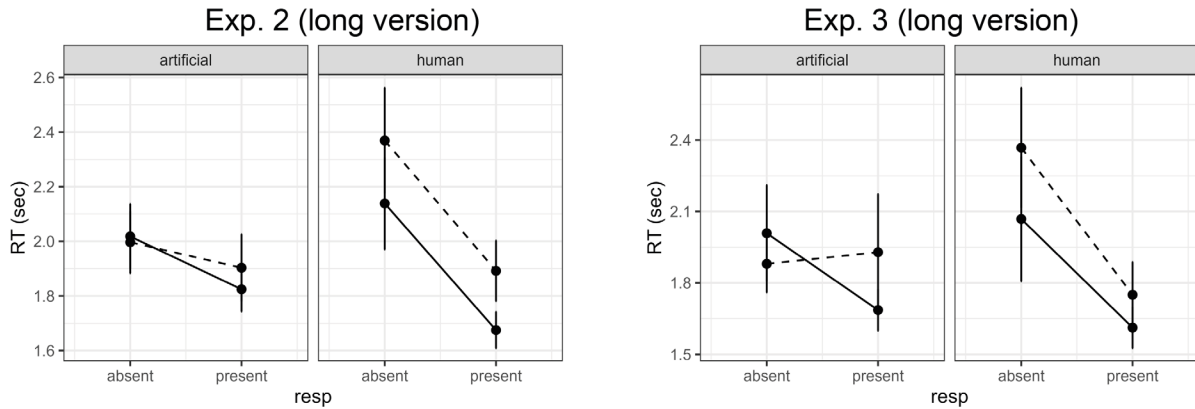
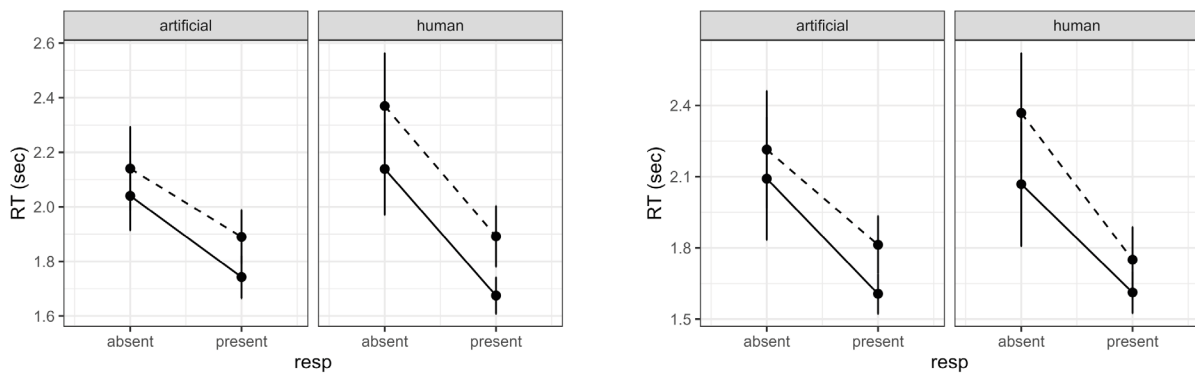
Within-condition variability**A basic model****B trial variability**

Figure A2: Association between decision and decision accuracy in human and artificial data. Solid lines represent correct responses, dashed lines are incorrect responses. Panel A: the standard model, as presented in the main text. Panel B: the extended model, with inter-trial variability.

In our experiments, incorrect responses were generally slower than correct responses. This was true both for “target present” responses (difference in seconds between false alarm and hit trials; Exp. 1: $M = 0.33$, 95% CI [0.26,0.39], Exp. 2: $M = 0.31$, 95% CI [0.23,0.38], Exp. 3: $M = 0.39$, 95% CI [0.32,0.47]), and for “target absent” responses (difference in seconds between miss and correct rejection trials; Exp. 1: $M = 0.31$, 95% CI [0.24,0.37], Exp. 2: $M = 0.37$, 95% CI [0.31,0.43], Exp. 3: $M = 0.27$, 95% CI [0.20,0.35]).

As presented in the main text, the model does not consistently account for these effects, sometimes even predicting that correct responses should be slower rather than faster than incorrect responses (“target present” responses in Exp. 1: $M = 0.03$, 95% CI [0.01,0.05], Exp. 2: $M = -0.05$, 95% CI [-0.08, -0.03], Exp. 3: $M = 0.07$, 95% CI [0.05,0.08]; “target absent” responses: $M = -0.01$, 95% CI [-0.03,0.01], Exp. 2: $M = 0.06$, 95% CI [0.04,0.08], Exp. 3: $M = -0.07$, 95% CI [-0.09, -0.04]).

Of note, the presented model assumes that all trials within a condition are of the same difficulty (that is, the visibility of the stimulus, and the believed visibility of the stimulus, are unchanged across trials). In the context of drift diffusion modelling, assuming that evidence accumulation varies between trials can account for slower error trials (Calder-Travis, Charles, Bogacz, & Yeung, 2024; Ratcliff & McKoon, 2008). Therefore, to incorporate inter-trial variability into our model, we extended the model by assigning a random “difficulty” value x to each trial, sampled uniformly from $x \in [-2, -1, 0, 1, 2]$. Target visibility on a given trial was then defined as $\alpha\eta^x\theta$, and beliefs about visibility on a given trial are $\bar{\alpha}\bar{\eta}^x\bar{\theta}$, with both η and $\bar{\eta}$ in the range of (0,1]. This way, lower values of θ give rise to more pronounced variability in the true visibility of stimuli (with $\theta = 1$ corresponding to no variability at all), and lower values of $\bar{\theta}$ produce higher variability in beliefs about the visibility of stimuli. In this specification of the model, visibility and believed visibility are perfectly correlated across trials, but a third parameter can be introduced to control the alignment between the two.

We fitted the extended model to participants’ behaviour in the long version of Exp. 2 and 3 (given the higher number of free parameters, using the long version ensured we had a reasonable number of data points per free parameter). As can be seen in Fig. 8, the extended model successfully accounts for slower error trials both in “target present” and in “target absent”

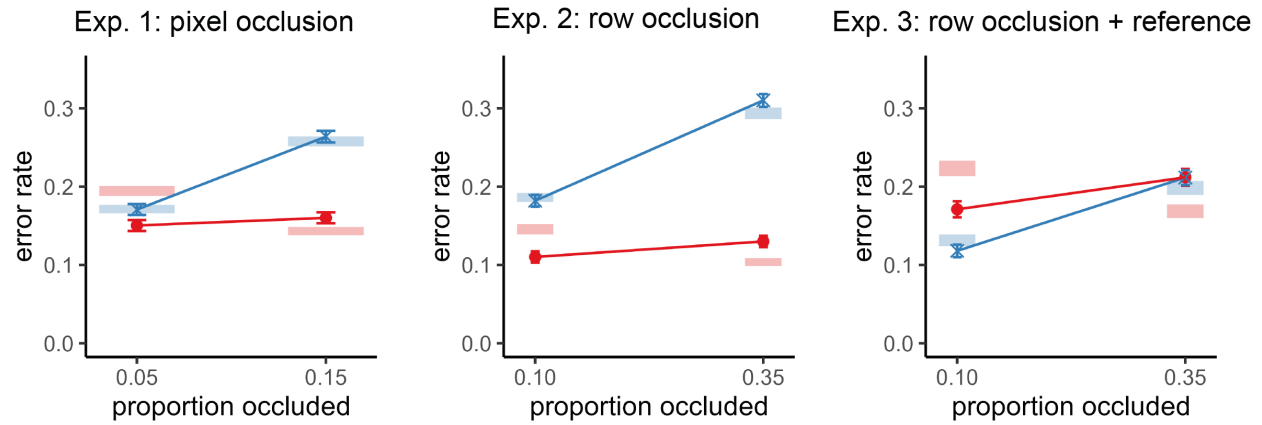
responses. Furthermore, inspection of the fitted model parameters suggests that participants underestimated the true variability in stimulus visibility across trials in Exp. 2 ($\eta - \bar{\eta} \ t(9) = -3.39, p = .008$), but not in Exp. 3, where the expected visibility of stimuli could be directly perceived in the reference stimuli ($t(9) = -0.31, p = .764$).

Constrained model fit

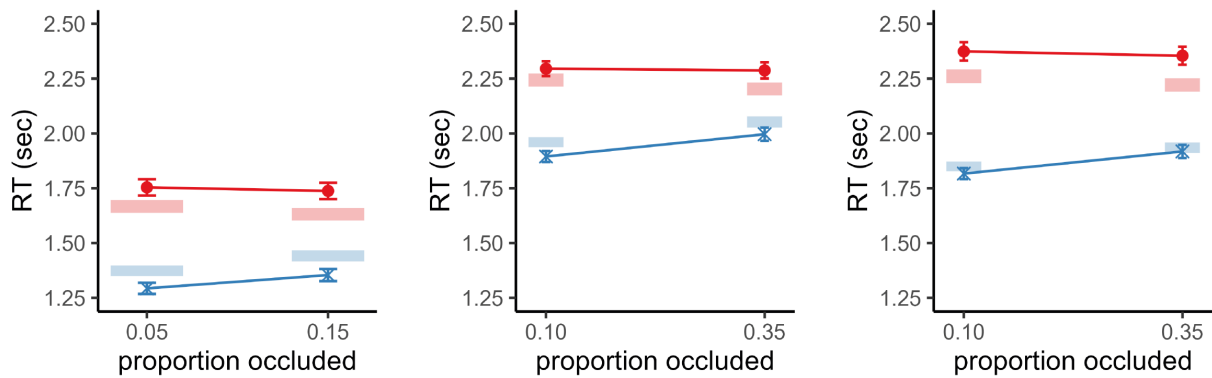
We fitted a variant of the full model, but where $\bar{\alpha}$ is constrained to be 1, effectively forcing a V_{IGNORE} architecture. This model predicted a decrease in the false alarm rate and in target-absent RTs as a function of occlusion, and no effect of occlusion on confidence.

Model fits: IGNORE variant

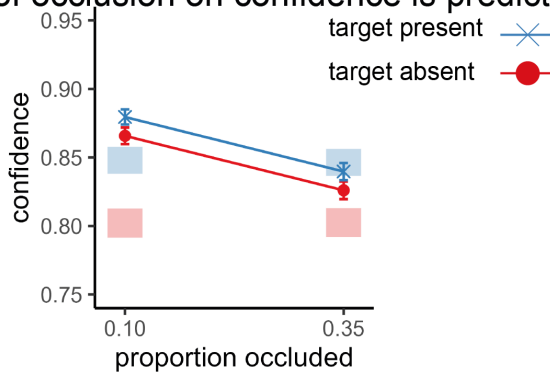
A error rates: false alarms predicted to go down in high occlusion



B reaction times: RT predicted to go down in target-absent RTs



C confidence: no effect of occlusion on confidence is predicted (out of sample prediction)

Figure A3: Empirical data and model predictions for variant V_{IGNORE} . Same conventions as Fig. 5.