Title: Studying unconscious processing: Contention and consensus

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Short abstract (<100 words)

There is no widespread agreement about the extent to which perceptual and cognitive processes can occur unconsciously. This lack of consensus stems, at least in part, from the use of a diversity of sometimes suboptimal methods and measures. We, a group of international experts in unconscious processing, propose methodological recommendations and identify outstanding issues that highlight knowledge gaps and complex decisions that researchers have to face. Our recommendations and their underlying considerations can guide future research and serve as a reference point for future discussions, ultimately leading to more rigorous approaches in unconscious processing research.

Long abstract (<250 words)

The scope of unconscious processing has long been, and still remains, a hotly debated issue. This is driven in part by the current diversity of methods to manipulate and measure perceptual consciousness. Here, we provide ten recommendations and nine outstanding issues about designing experimental paradigms, analyzing data, and reporting the results of studies on unconscious processing. These were formed through dialogue among a group of researchers representing a range of theoretical backgrounds. We acknowledge that some of these recommendations naturally do not align with some existing approaches and are likely to change following theoretical and methodological development. Nevertheless, we hold that at this stage of the field they are instrumental in evoking a much-needed discussion about the norms of studying unconscious processes and helping researchers make more informed decisions when designing experiments. In the long run, we aim for this paper and future discussions around the outstanding issues to lead to a more convergent corpus of knowledge about the extent – and limits – of unconscious processing.

Keywords: Unconscious processing, perceptual awareness, awareness measures, recommendations, best practices

Introduction

The scope of **unconscious processing** (see Glossary) is one of the most hotly contested topics in cognitive science (Eriksen, 1956; Marcel, 1974; Peirce & Jastrow, 1884; Peters et al., 2017; Shanks, 2017), and, accordingly, has been extensively researched (for reviews, see Axelrod et al., 2015; Hassin, 2013; Kouider & Dehaene, 2007; Mudrik & Deouell, 2022; Newell & Shanks, 2014; Sterzer et al., 2014; Tsikandilakis et al., 2019). Unconscious processing refers to a myriad of phenomena (see "*Unconscious processing: definitions and measures*" below); in this article we specifically focus on the effect of stimuli that are not consciously perceived on perceptual and cognitive processes or on neural activity (which has arguably constituted the lion's share of research on unconscious processes thus far).

This research has often yielded contradictory results, fueling many longstanding and ongoing disagreements (Francken et al., 2022). While some argue that unconscious processing permeates even high-level cognitive functions (e.g., Bargh, 2014; Dijksterhuis & Nordgren, 2006; Hassin, 2013), such as executive control (Dienes et al., 2022; van Gaal et al., 2012), working memory (Soto et al., 2011) and information integration (for reviews, see Mudrik et al., 2014; Hirschhorn et al., 2021), others claim it is more limited in scope and only encompasses overlearned, automatic, reflex-like processes (e.g., Chater, 2018; Hesselmann & Moors, 2015; Holender, 1986; Newell & Shanks, 2023; Searle, 1998).

This state of affairs might result from a variety of factors. First, there are irreconcilable theoretical positions regarding what it takes for perceptual processing to count as unconscious (Phillips, 2018b). This has been a matter of debate since the inception of the field (Cheesman & Merikle, 1986; Greenwald et al., 1996; Merikle, 1992; for a review, see Kouider & Dehaene, 1996), mirroring ongoing debates about the definition of consciousness: while some theories of consciousness equate consciousness with cognitive access (i.e., availability of contents to a range of processes including memory, planning, report, etc.; Naccache, 2018; Naccache & Dehaene, 2001), others deny that cognitive access is a necessary correlate of consciousness and instead claim that it is independent from phenomenal consciousness itself (e.g., Lamme, 2016; cf. Block, 1995). These different theoretical approaches might accordingly inject confusion into studies of unconscious processes, as they might stem from fundamentally different conceptual frameworks and offer conflicting interpretations of whether the same data reflect unconscious processing.

A second, more practical reason for the lack of agreement in the field pertains to the way consciousness should be manipulated and measured. Researchers have employed different approaches to study unconscious processes. This variability pertains to: (a) the experimental procedures to suppress stimuli from awareness[1] (e.g., to render them consciously invisible, see Breitmeyer, 2015; Kim & Blake, 2005; or consciously inaudible, see Dykstra, Cariani, & Gutschalk, 2017); (b) the measures for assessing participants' awareness of the stimuli (i.e., determine if - and to what extent - they consciously perceived the stimulus; Reingold & Merikle, 1988; Sandberg et al., 2010); and (c) the strategies for analyzing and interpreting the data (Dienes et al., 1995; Fleming, 2017; Kristensen et al., 2020; Maniscalco & Lau 2012; Rausch et al., 2015; Sandberg et al., 2011; Schmidt & Vorberg, 2006; Shanks, 2017). This diversity, on its own, is not problematic. Diverse methods allow differentiation or, when based on the same theoretical underpinning, provide higher convergent validity (Campbell & Fiske, 1959; Skora et al., 2021). However, in the field of unconscious processing, such convergence has yet to be achieved, possibly due to the differing methodological weaknesses of some of these procedures, and their different theoretical underpinnings.

Given this disagreement around both theoretical and operational approaches, some have suggested that the field suffers from a lack of a clear definition of what is meant by 'unconscious processing' (Francken et al., 2022), as well as from the absence of guidelines for "best practices" (Rothkirch & Hesselmann, 2017). In other fields, groups of researchers have successfully built consensus about terminology (e.g., visual distraction: Liesefeld et al., 2024) and methodologies (e.g., computational modeling: Wilson & Collins, 2020; EEG: Pernet et al., 2020; Picton et al., 2000; and fMRI: Poldrack et al., 2017), which helped form a more coherent corpus of knowledge. Applied to the field of unconscious processing, providing standards for conducting experiments, analyzing data, and reporting results might help establish a more informed body of knowledge, whereby studies can be more easily compared and integrated into a cohesive account. Notably, although all guidelines were discussed in the context of behavioral studies, most of them similarly apply to neuroimaging and electrophysiological research.

[1] In this paper, we use 'awareness' and 'consciousness' interchangeably.

Here, we take a first step in this direction. We report the results of an attempt to identify where consensus and where contentions exist among a group of 32 researchers from different laboratories around the world, coming from a wide range of theoretical backgrounds and views on consciousness research (though inevitably not representing *all* views and approaches in the field). We conducted a series of three meetings employing consensus methods (Waggoner et al., 2016) and surveying views and preferences. This yielded a set of ten practical recommendations and nine outstanding questions. We provide a quantification of the state of agreement for the recommendations, which had to be approved by at least two thirds of the participants (Beatty & Moore, 2010). While being an author of this paper does not imply agreement with each and every recommendation, it does convey the commitment to the process of establishing these recommendations, and acknowledging their importance.

That is, all authors agree that despite the inevitably debatable nature of some of the recommendations, they can move the field forward: while some will be widely adopted, others will evoke discussion and criticism. Either way, they can facilitate a transition from the current state of affairs, where different labs use different methods without necessarily justifying their approach and making explicit comparisons with other available methods.

We aim to promote a collaborative effort to reach a consensus and identify areas of contention through dialogue and constructive exchange of views. The recommendations were established in a process where critical methodological issues were presented, discussed, and voted upon (see *Supplementary Materials: The process by which the recommendations were established)*. Then, a subset of recommendations were selected, refined and substantiated during extensive iterations between the participants in the writing process, until an agreement was obtained for at least two thirds of participants. Finally, while working on the article, a final round of voting took place to assess co-authors' agreement on the final version of the recommendations. Our work was inspired by existing recommendations for measuring awareness (Overgaard & Sandberg, 2012; Rausch & Zehetleitner, 2016), conducting response priming experiments (Schmidt et al., 2011), and evaluating claims of high-level unconscious processing (Moors et al., 2019). Here, we go beyond these earlier endeavors, by (a) systematically evaluating approaches encompassing the different stages of research, (b) basing it on the discussions of a larger group of researchers with diverse theoretical backgrounds, and (c) incorporating several recently raised issues with profound impact on the interpretation of study results (e.g., Meyen et al., 2022; Shanks, 2017; see further discussion below).

The challenge of studying unconscious processing is twofold; on the one hand, researchers should make sure participants are indeed unaware of the stimuli, thereby overcoming ***contamination by conscious processing*****.** On the other hand, they should present the stimuli in a way that allows them to evoke *sufficiently strong signals***,** to yield measurable effects (e.g., on behavioral measures of perception and cognition). Achieving both goals is far from trivial (Michel, 2023a; Reingold & Merikle, 1988; Seth et al., 2008). The recommendations presented here specifically target this challenge, focusing on the study of unconscious processing. Although many of our recommendations also apply to studies focusing on distilling the neural correlates of consciousness (e.g., contrasting conscious and unconscious processes to identify neural correlates of consciousness; Crick & Koch, 1990; Koch et al., 2016) they were not formed with the intention to address *all* the challenges of studying consciousness. For example, a major challenge in studying consciousness pertains to the ability to tease apart the neural correlates of consciousness from associated cognitive and decision processes, like report (Aru et al., 2012; De Graaf et al., 2012). Here, we do not address this conundrum, as it is outside the scope of this work.

Alongside the ten recommendations, the nine outstanding issues represent cases where the group did not reach a substantial level of agreement in the voting procedure (at least two thirds). We describe them and provide a range of possible solutions. These outstanding issues might be resolved following future methodological developments in the field. Such advancements may also lead to modifications of our recommendations. Yet, until this happens, the recommendations can be used as a reference point, allowing the field to strengthen the rigor of its research methods, while the outstanding issues highlight the knowledge gaps we should strive to close. Importantly though, what both the recommendations and the outstanding issues demonstrate is that there is no perfect recipe for studying unconscious processes. Each decision made by researchers has its advantages and disadvantages, and prioritizing one aspect (e.g., adding more measures of awareness to improve sensitivity) might lead to a cost in another (e.g., diluting participants' resources due to task load). Such conundrums and possible courses of actions are addressed below.

We first define some key concepts used in unconscious processing research. We then present the 10 recommendations for best practices (see also Box 1 for more general recommendations for improving the quality of experimental methods specially relevant to unconscious processing). Finally, we discuss nine outstanding issues and possible solutions.

Unconscious processing: definition and measures

The word 'unconscious' has been used in the context of a multitude of phenomena, from subliminal effects through implicit processing to social biases and stereotypes (e.g., Gawronski, Hofmann, & Wilbur, 2006; Graham & Lowery, 2004). Here, we focus on evidence for **unconscious processing** of external stimuli, which we define as the effect of stimulus features that are not consciously perceived on perceptual and cognitive processes. To that end, we operationally define **conscious processing** as having a reportable experience of the critical content of the stimulus, in the sense of being able to report having perceived it if asked to. That is, for studies that aim at inspecting unconscious processes, we deem it critical that participants are not able to report the content of interest as having been consciously perceived.

As we explained above, some theoretical approaches reject the relationship between having a conscious experience and having cognitive access to its contents (Block, 2007; Lamme, 2010), claiming that experience overflows access, such that contents that are not reportable can nevertheless be consciously experienced (for divergent views see Cohen & Dennett, 2011; for reviews, see Block, 2011; Mudrik et al., 2025; Overgaard, 2018; Phillips, 2016). Here, we refrain from making a theoretical stand about this discussion. Instead, we focus on the purpose of empirically investigating unconscious processing; in this specific context, the field generally agrees that consciousness should indeed be ruled out by evaluating cognitive access (Francken et al., 2022).

Throughout the article, we accordingly apply the term **unconscious processing** to cases where participants are unable to report the content of the stimulus that elicits the process of interest (e.g., participants being unable to report seeing a face, in an experiment that tests for face processing), despite this content eliciting some measurable effect. Some have suggested that in this case, the resulting experience should not differ from the experience evoked in trials where this content was not even presented (MacMillan, 1986; Peters et al., 2017). Note also that one implication of our definition of unconscious processing is that it does not include cases of 'implicit' processing, where the stimuli themselves are consciously perceived, but participants are not aware of their effect on behavior or processing (for reviews, see Dai et al, 2023; Giménez-Fernández et al., 2023; Moran et al., 2023; Shanks & St. John, 1994; Williams, 2020). Nevertheless, several of the issues considered here, such as the relative advantages of subjective and objective measures, can also be applied to implicit processing research.

Our operational definition of unconscious processing yields two criteria that should be fulfilled. First, *the* ***critical content*** *of interest* (which can be a whole stimulus or a stimulus feature, see Outstanding issue 2*) should not be consciously perceived*. In typical experiments on unconscious processing, this is achieved in one of two ways: the critical content is presented (1) in a 'threshold' or 'liminal' condition, where it is sometimes experienced and sometimes not, or (2) in a 'subliminal' condition, where it is presented so that it is never experienced (though in practice this is rarely the case, and conscious processing occurs in some of the trials; see Outstanding issue 5). In the latter case, researchers try to present the stimuli such that they are just under the 'perceptual threshold' at which participants report consciously experiencing the stimulus, to promote sufficient neural activation to affect behavior (for interpretations of this threshold that are consistent with signal detection theory, see MacMillan, 1986). Whether the critical content was indeed not consciously perceived is typically demonstrated using one or several ***direct measures of awareness***, examining whether participants report having no perception of the stimulus (or of some of its features), and/or are unable to perform some explicit judgment that would be straightforward if the stimulus were consciously perceived (though see Outstanding issue 7 for a discussion of the double-dissociation approach, where demonstrating unawareness is not required, as well as Outstanding issue 8).

The second criterion pertains to providing evidence *that the critical content was nevertheless processed (i.e., a* ***processing measure****).* Such evidence is usually obtained by ***indirect measures of unconscious processing,*** which show processing of the critical content at the behavioral or neural level.[2] For example, in a typical masked priming experiment (see **Figure 1** for more details), the direct measure of awareness consists of either detecting the presence of the stimulus of interest - the prime - or detecting/discriminating one of its features; and the indirect measure of unconscious processing involves measuring the impact of the critical content on a behavioral response to a consciously perceived target stimulus.

Measures of awareness can be objective or subjective. With **objective measures,** participants are typically asked to make some direct judgment on the critical content (e.g., “Was the stimulus tilted to the left or to the right”, see Parkes et al., 2001; or “was the stimulus present or absent”; Naccache & Dehaene, 2001). Objective measures are typically taken using a forced-choice question (i.e., a task with a limited number of response options and no option to opt out) so the accuracy of participants’ answers can be assessed. The critical content is deemed not consciously perceived if objective task performance is not better than chance-level performance across trials (but see again footnote 2), or by demonstrating **null sensitivity** in the task. For such demonstrations, the use of signal detection theory measures such as *d’* is considered a best practice (Green & Swets, 1966).

With **subjective measures,** participants report the presence or absence of a conscious experience triggered by the critical content (e.g., clarity, phenomenal magnitude, confidence). Therefore, these reports cannot be considered as correct or incorrect, as they reflect the content of participants’ own perceptual experiences[3]. Using subjective measures, the critical content is

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[2] Note that in some cases, direct measures can also be used to infer unconscious processing, as we explain below.

[3] Note that confidence can be compared to performance (Maniscalco & Lau, 2012; 2014) to estimate metacognitive abilities, but this is not an objective measure of consciousness. To illustrate, participants may not be confident about a correct response, yet this does not constitute a “wrong” response. Therefore, confidence ratings are often used as subjective measures, with low confidence reflecting the absence of reportable conscious processing. However, confidence ratings can also be used to code the strength of evidence, in which case they can be used as objective measures of consciousness. For further discussion on the relationship between confidence and awareness, see Koriat, 2000; Rosenthal, 2019.

considered not consciously perceived if the participant reports no experience of the relevant aspect of the stimulus (Dienes, 2007).

Both subjective and objective measures have been criticized (for further discussion, see Hesselmann, 2013; Irvine, 2012; Overgaard, 2015; Seth et al., 2008; Timmermans & Cleeremans, 2015; and the sections below). One important issue is the **criterion problem** (Eriksen, 1960; Lloyd et al., 2013; see also Francken et al., 2022, Figure 2)[4]. Given the same borderline (threshold) perceptual experience, one participant may report perceiving the stimulus while a more conservative participant would report not perceiving it (Schmidt, 2015; for a recent study demonstrating how this could affect the results, see Fahrenfort et al. 2024). Subjective measures may accordingly overestimate or underestimate unconscious processing, depending on how conservative or liberal, respectively, the participant's criterion is. Both conservative and liberal criteria for answers on subjective awareness measures are problematic. On the one hand, conservative criteria could lead to unwarranted conclusions regarding the scope of unconscious processing, in case participants say they did not perceive the critical content, when in fact they did have some faint perception of it. Conversely, liberal criteria could lead to a reduced number of trials that are considered unconscious. Given the criterion problem, some researchers hold that the subjective awareness measure should be accompanied by chance-level performance in an objective measure (Andersen et al., 2019; Holender, 1986; Hesselmann et al., 2011; Izatt et al., 2014; Merikle & Reingold, 1990). Others also suggest to examine the relationship between the objective and subjective responses (see Recommendation 4 below), or to use a task that more directly targets the content of participants' perception, such as asking them to reproduce the experience of the stimulus (Sánchez-Fuenzalida et al., 2023).

Objective measures suffer from different limitations. They may be too strict, in several ways: First, since it is not easy to obtain true chance performance (with a narrow confidence interval or

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[4] This is true for most subjective measures, and for objective detection tasks; unless signal detection measures are used to measure sensitivity independently from criterion-placement (bias). This can be done for example for detection tasks if catch trials, in which the critical content is not shown, are incorporated in the design (Recommendation 5), and it can also be done using a 2-Interval Forced Choice question as the subjective measure (Elosegi, Mei & Soto, 2024; Amerio et al., 2024; Rajananda et al., 2020; Peters et al., 2017, Peters & Lau, 2015; although this approach has been criticized; Phillips, 2021).

strong Bayesian support for the null) in the objective measure, researchers may choose to suppress the critical content so strongly that it produces signals too weak to elicit measurable (unconscious) effects. As a result, such studies might underestimate the extent of unconscious processing (e.g., Michel, 2023b; Reingold & Merikle, 1988), as weaker suppression might have allowed for an effect to be found while preserving chance-level performance on the objective measure. Although this is not a limitation of the measure itself, but of the strategies used by researchers, it is nevertheless important to acknowledge this concern, as it might affect the resulting conclusions. Second, above-chance discrimination performance in the objective measure might also be driven by unconscious processing (Merikle, 1984; Merikle et al., 2001; Michel, 2023b; Schmidt & Vorberg, 2006; Micher et al., 2024). In fact, some researchers have considered above-chance objective performance as a marker of *unconscious* processing, as long as participants subjectively deny consciously perceiving the stimulus (e.g., Lau & Passingham, 2006; Soto et al., 2011; Trübutschek et al., 2017; 2019a; 2019b; see further discussion in the *Conclusion* section). This again reflects differences in theoretical approaches, which in turn yield different definitions of conscious vs. unconscious processing.

On the other hand, objective measures might also lead to an *over*estimation of unconscious processing, depending on when they are taken, and on the nature of the task. Suppose that objective measures are taken at the end of the experiment. In that case, participants might disengage due to a lack of motivation or due to fatigue, underestimating their level of awareness earlier in the experiment (Pratte & Rouder, 2009). If objective measures are interleaved during the experiment, carryover effects may occur from trials in which the stimulus was consciously perceived (e.g., Lin & Murray, 2014). Another potential issue might occur if the objective measure probes a feature that is harder to detect than the feature of interest driving the unconscious effect. In that case, having the objective measure focus on that feature could again overestimate unconscious processing, as the objective task might be too difficult (e.g., determine if a scene includes an incongruent object, or whether a face is male or female). Then, performance might be at chance-level even when participants consciously experienced some relevant aspects of the stimulus (e.g., saw some parts of the scene, or the eyes of the face), including the critical content. Participants will be classified as unaware, although they were conscious of its critical content, at least to some extent. This partial conscious experience might explain an effect of interest obtained with the processing measure (e.g., Gelbard-Sagiv et al., 2016). Although rare, the opposite situation is also possible, in case the objective measure targets a feature that is more easy to detect than the feature of interest. Thus, objective measures come with their own challenges, but provide complementary information to subjective measures.

Though the above definitions and approaches are still being discussed and shaped in the field (Michel, 2023b; Rothkirch & Hesselmann, 2017), we adopt them here as a basis for our discussion of the following recommendations and outstanding issues.

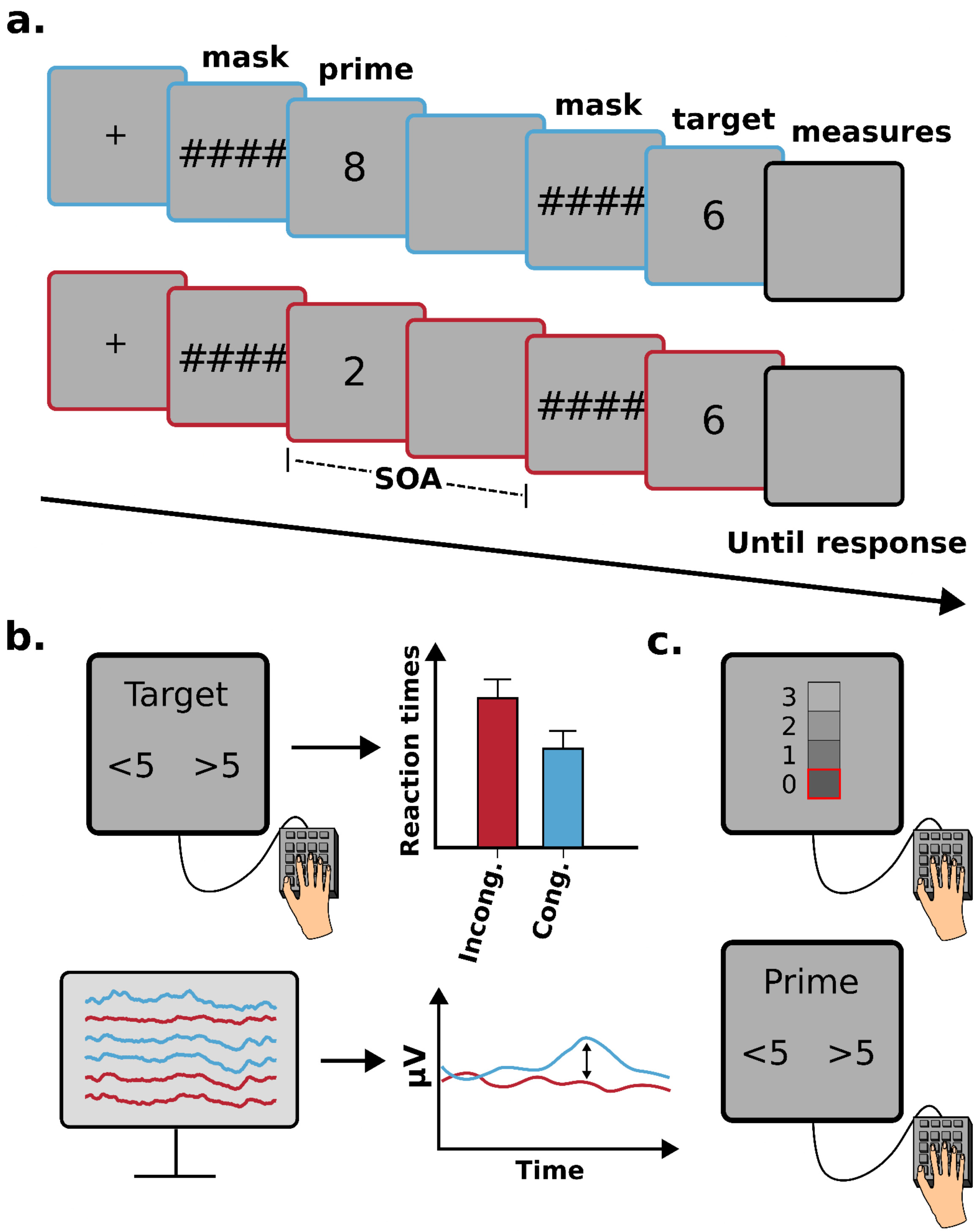

Figure 1. A. Example of a visual response priming task (Naccache & Dehaene, 2001). The task is to press one button if the target is larger than 5 and another if it is smaller than 5. The prime frame is preceded and followed by mask frames, and the stimulus onset asynchrony (SOA) determines the extent

to which the prime is perceived. In congruent trials (blue), the prime and target both correspond to the same response as both are larger than 5, while they correspond to opposite responses in incongruent trials (red). *B.* Processing measures can be either behavioral (top panels, showing slower reaction time on incongruent trials) or neural (bottom panels, schematically showing differential responses based on congruency), and consist of averaging responses or activity across trials in the congruent and incongruent conditions (right panels). *C.* Awareness measures can be subjective (for example, the Perceptual Awareness Scale in the top panel; see Recommendation 3 below) or objective (alternative forced choice on the prime - or the critical content - in the bottom panel; here, determining whether the prime is smaller or larger than 5).

Recommendations

A: DESIGNING THE EXPERIMENT

1. Use both subjective and objective measures

A majority of participating researchers agreed that both subjective and objective measures should be collected to assess awareness. Currently, various approaches are used in the field, with some studies using only a subjective awareness measure (e.g., Avneon & Lamy, 2019; Bahrami et al., 2010; Sperandio et al., 2018; Scott et al., 2018; Yang et al., 2017), some using only an objective one (e.g., Bruno et al., 2020; Filippova & Kostina, 2020; Haase & Fisk, 2020; Pesciarelli et al., 2019; Stein et al., 2020; Tu et al., 2019), some using both (e.g., Biderman & Mudrik, 2018; Biderman et al., 2020; Faivre et al. 2016; Gayet et al., 2020; Hesselman et al., 2011; Izatt et al., 2014; Lamy et al., 2009; Sand & Nilsson, 2017), and others including only post-experiment debriefing (e.g., Chng et al., 2019; Fadardi & Bazzaz, 2011; Park & Donaldson, 2016; Wang & Forster, 2015). Such heterogeneity of awareness measures across studies makes it challenging to compare and interpret results (Rothkirch & Hesselmann, 2017). To improve the comparability of future studies, we recommend collecting both types of awareness measures wherever feasible.

Collecting both types of awareness measures also makes it possible to better understand how awareness measures relate to each other (e.g., Stein et al., 2021). Importantly, as we acknowledge above, the two awareness measures might "disagree"; for example, one might find above-chance performance in the objective measure, although participants subjectively report not perceiving the stimulus. We do not think that this situation necessarily implies that participants were, in fact, aware of the critical content (and simply used a too conservative criterion for the subjective measure). This is only one out of two possible interpretations. The other is that performance in the objective measure was driven by unconscious processing, and currently there is no definitive way to determine which of the two is correct (which is not just a methodological matter, but a theoretical one). Still, this is an important piece of information that puts the results in a different perspective compared to a similar study where the same effect of interest was observed, but the two awareness measures did "agree". Thus, we think this is relevant information that should be reported. Accordingly, we recommend that in such a case, researchers are clear about the theoretical basis of their interpretations.

Some experimental tasks do not lend themselves to collecting both types of awareness measures (for example, in multi-trial inattentional blindness (IB) paradigms; Pitts et al., 2012; although see recent single-trial IB studies that did collect such measures; Hirschhorn et al., 2024; Nartker et al., 2025). Others, looking for double dissociations between awareness and processing (Outstanding issue 7), might not require collecting both measures. And in some experimental settings, where subjective and objective measures have been shown to converge (Kiefer, 2023; Kiefer & Kammer, 2024), researchers might find it beneficial to focus on one measure only. In these cases, it is advisable to label the processing as "subjectively invisible" (this could also be applied in the case above, where objective performance is above chance level) or "objectively indiscriminable" (see Stein et al., 2021, for such an approach), to clearly specify on what type of measure the claim is substantiated[5].

2. **Objective measure: Use a forced choice discrimination task on the feature of interest**

[5] Note that Stein and colleagues refer to the latter as "objectively invisible". The use of the phrase "subjectively invisible" again assumes that subjective reports capture the contents of a conscious percept, which, as we discussed above, is not accepted by those who hold that perception overflows report (Block, 2007; Lamme, 2010). To alleviate that concern, researchers may consider using "subjectively unreported" instead.

Different studies have used various types of objective measures, including identification of the stimulus (i.e., naming the stimulus; e.g., Bengson & Hutchison, 2007; Brunellière & Frauenfelder, 2014), and performing a discrimination task that pertains to one of its features (e.g., larger or smaller than five as in Figure 1; Filippova & Kostina, 2020; Kouider & Dupoux, 2001; Lourenco et al., 2016; Naccache & Dehaene, 2001; Ortells et al., 2016). We recommend having participants perform a forced-choice discrimination task. Such tasks rely on simple instructions, thereby minimizing confusion for the participants who often perform dual, or even triple tasks on each trial (i.e., the processing measure, the subjective and the objective measures).

When analyzing performance in the objective measures, signal detection theory provides very useful tools. For a task in which there are trials where the stimulus or feature of interest is absent (see Recommendation 5), *d'* can be computed to determine whether the stimulus or feature is detected above chance-level (this also applies to 2-Interval Forced Choice tasks, where participants have to report in which one of two intervals a stimulus is presented). For discrimination tasks, assuming there are two exclusive responses, one can still calculate *d'* by defining one of the options as the signal and the other as noise. This allows researchers to estimate participants' sensitivity; additional signal detection theory measures can be used to evaluate their criterion and bias (Hautus et al., 2021). If participants' responses are manipulated parametrically (e.g, by manipulating response bias, etc.), Receiver Operating Characteristic (ROC) curves can be computed, allowing researchers to check if the results are robust to changes in participants' report threshold.

Importantly, the objective task can be focused on several aspects of the stimulus display, including on the presence or absence of the stimulus of interest, or on one of its features (e.g., is a grating tilted left or right). We recommend focusing the objective measure on the feature of interest. That is, the feature that is taken to influence the processing measure (Dulany, 1963; Michel, 2023b; Schmidt & Biafora, 2024; to avoid underestimation, which might occur when the objective measure probes a lower-level feature of the stimulus, or overestimation of unconscious processing, which might occur when the measure probes a higher-level feature of the stimulus; see *Unconscious processing: definition and measures*). This approach is advantageous because it ensures a high level of task similarity between the two measures

(Reingold & Merikle, 1988; Schmidt et al., 2011). On the one hand, it is preferential over using a detection task on the entire stimulus, because the latter might underestimate unconscious processing by posing a too-stringent test (Michel, 2023b). On the other hand, querying for more specific features may be too liberal. As an example, consider a priming task probing number processing (represented in Figure 1), where digits are presented as primes and targets, and participants are asked to determine whether the target digit is larger or smaller than 5. There, the unconsciously processed feature of interest is whether the prime digit is larger or smaller than 5, and so the objective measure should pose that exact question (rather than, for example, reporting the specific digit). As another example, if one looks for differential neural activity between emotional and non-emotional faces that are not consciously perceived, the objective measure should involve a discrimination task between an emotional and a neutral face[6].

A downside of probing the feature of interest that applies only to priming studies is that the response about the target is likely to overshadow the response about the stimulus of interest (here, the prime; Peremen & Lamy, 2014). In the above example, if the prime was the digit 2 but the target was the digit 6, participants might have had some conscious experience of the prime, but confused it with the target, and erroneously reported that the prime was larger than 5. Such contamination by processing of the clearly visible target renders the objective measure less sensitive and more prone to errors. This issue can be mitigated, to some extent, in several ways. First, by counterbalancing the order of the objective measure and the processing measure (in experiments that use priming to probe unconscious processing; Stein & Peelen, 2021). Second, by adding trials within the main experiment where the target is not presented (Faivre & Kouider, 2011). However, if the target contributes to the invisibility of the critical content, this might not reflect participants' processing of this stimulus in target-present trials. This too can be overcome by presenting a neutral target, rather than no target (e.g., in the example above, presenting a symbol instead of a digit), so that the target could still contribute to masking the critical content, yet without evoking a competing representation. This too, unfortunately, evokes complications, as participants are required to task-switch between trials, such that on some trials they perform a task about the target and on other trials, they perform a

[6] Note that it is sometimes more complicated. Consider, for example, a priming task probing global versus local amodal completion, where an occluded figure is presented as a prime and its amodal completion (global or local) as targets. In this case, the objective measure cannot involve the stimulus of interest (in this case the occluded prime), but rather, a discrimination between global and local completions of the prime (Kimchi et al., 2023).

task about the critical content. This task-switching might require additional resources that would tamper with the unconscious process of interest. Thus, this requires some piloting, or a practice session, to make sure participants can indeed perform this task.

3. Subjective awareness measure: a preference towards the Perceptual Awareness Scale (PAS)

Over the years, different subjective measures have been developed, ranging from dichotomous aware/unaware judgments (e.g., Ro et al., 2009), through numerical scales (either visibility scales or confidence scales, e.g., Dienes et al., 1995; Kuhn & Dienes, 2006; Rothkirch et al., 2012) to wagering tasks (Persaud et al., 2007). Some even present the subjective measures as a forced choice task, by asking participants to indicate, for example, in which of two intervals they felt more confident that a stimulus appeared (Peters & Lau, 2015), allowing them to conduct a sensitivity analysis on subjective responses (Elosegi, Mei & Soto, 2024; Amerio et al., 2024; Rajananda et al., 2020; see also De Gardelle & Mamassian, 2014). Though all these measures can be used to gauge conscious perception, a majority of researchers in this project (78%, see Figure 2) preferred the PAS (Ramsøy & Overgaard, 2004), echoing preferences in the field at large (Francken et al., 2022).

The PAS is an ordinal scale that is held to capture situations of null awareness, by providing nuanced intermediate levels that can be used whenever participants think they might have perceived something despite having far from a clear conscious percept (namely 1 = “No experience,” 2 = “Brief glimpse,” 3 = “Almost clear image/experience,” and 4 = “Clear image/experience”; see Ramsøy & Overgaard, 2004, Sandberg et al., 2010; Wierzchoń et al., 2014; 2019; though see Tunney & Shanks, 2003, for work implying that binary ratings might be more sensitive). Another advantage of this scale is its qualitative descriptions, which are based on people’s responses when asked to put their experience into words (Ramsøy & Overgaard, 2004). Note that it is not always clear whether these qualitative descriptions generalize across all experimental settings (Christensen et al., 2006). Accordingly, it has been suggested that in some cases, the labels for the scale should be tailored to the specific needs of the research protocol using the same method as in the original PAS paper (for further discussion, see Sandberg & Overgaard, 2015). Arguments have also been made that the PAS constitutes a more appropriate awareness measure than subjective measures that target other aspects of experience (Sandberg et al., 2010), such as confidence (though see Dienes & Seth, 2010; Michel, 2023b; Szczepanowski et al., 2013). The PAS (and other subjective measures) can be applied either on the entire stimulus or on the feature of interest, as we discuss below (Outstanding issue 2).

The use of PAS has been questioned in recent years (Sandberg & Overgaard, 2015). Some, including several authors of this contribution, feel quite strongly that the PAS should not be the measure of choice, because it does not eliminate the criterion problem (Fahrenfort et al., 2024; see Peters & Lau, 2015 for an alternative that sidesteps the problem, although doubts have been raised about whether or not this actually constitutes a subjective measure; Phillips, 2021) and because of its ambiguity concerning the characterization of trials classified as “brief glimpses” (Dienes & Seth, 2010; Michel, 2023b, see also Recommendation 7). Others have questioned its validity as a measure of consciousness. According to Michel (2019), the PAS does not measure what it is meant to measure (i.e., it may capture the intensity and precision of perceptual contents rather than consciousness of those contents, though see Skora et al., 2021; see also Schmidt & Biafora, 2024). For these reasons, some researchers assess awareness relying on other subjective measures such as confidence, which allows deriving specific metrics based on signal detection theory (Maniscalco & Lau, 2012; 2014). This includes the calibration of confidence reports to one’s performance in a task (e.g., in detecting a stimulus), an ability that has been termed ‘metacognitive efficiency’. In the context of unconscious processing, metacognitive reports on the critical content can be used as a subjective measure, under the rationale that conscious perception is needed for efficient metacognition. However, one should note that when using the subliminal paradigm (Outstanding Issue 6), metacognitive efficiency is expected to be low due to extensive signal degradation. More crucially, while related, confidence and visibility are not the same construct, and it is unclear whether measures of confidence target conscious experience itself. For these reasons, and the advantages described in the previous paragraph, at this stage most of us still recommend using the PAS as a subjective measure of awareness.

4. Collect all measures on a trial-by-trial basis (when possible)

Objective and subjective awareness measures can either be administered on a trial-by-trial basis, that is, on the same trials on which the processing measure is obtained (e.g., Biderman & Mudrik, 2018; Sand & Nilsson, 2017), or separately, in an additional session (e.g., Bruno et al., 2020; Stein et al, 2020; Tu et al., 2019). We recommend the former approach, when possible, for three main reasons. First, measuring consciousness in a separate session might yield an inaccurate estimate due to differences between the main experimental session and the separate awareness measurement session. These differences might stem from order effects, like learning on the one hand, and fatigue on the other hand, though such differences can possibly be mitigated by using an ABAB design (see Outstanding issue 3). Perhaps more critically, the main experimental session and the separate awareness session also commonly involve differences between the tasks (Lin & Murray, 2014). These differences might affect findings in different ways. Participants might be less motivated and less engaged in a separate session, where the task focuses on awareness of the critical content, which is typically difficult to consciously perceive. Unmotivated participants may underperform on this task, leading to an underestimation of their conscious perception of the critical content in the main session (Pratte & Rouder, 2009; Note that this motivational problem is less relevant for experiments using liminal stimulation, see Outstanding issue 6). Conversely, participants might direct more attention to the critical content in the separate session, whereas in the main session, they had to provide a report on the target. This change in attention allocation might lead to an overestimation of the level of conscious processing in the main session.

The second reason why testing for awareness on a trial-by-trial basis is preferred is that the level of awareness most likely fluctuates from trial to trial (Mathewson et al., 2009), and this cannot be captured when testing awareness in a separate session. Thus, even if the separate awareness session indicates no awareness, it does not guarantee that there were no trials with conscious perception in the main session. Probing for awareness on a trial-by-trial basis allows the researcher to identify trials in which participants did not consciously perceive the stimulus (although such post-hoc selection poses the risk of regression to the mean; see Recommendation 8 below).

Third, trial-by-trial measures allow researchers to examine the relationship between participants' performance in the objective measure and their responses on the subjective measure. The level of agreement between the two has been used as an indication of the sensitivity of the subjective measure at the group level (see Sandberg et al., 2010; Wierzchoń et al., 2019) and to measure metacognitive performance at the individual level (similar to the "zero correlation criterion" between confidence ratings and objective performance, Dienes & Berry, 1997; Dienes & Perner, 2004). It may also be useful to examine performance in the objective measure specifically among trials in which the subjective measure indicates no conscious perception. If at chance, objective performance corroborates claims of unawareness on these trials (the "guessing criterion"; Dienes et al., 1995). If not, it might inform us about the level of unconscious processing driving the performance on this measure (for example, in studies investigating "unconscious perception", Peters et al., 2017), or indicate that participants' criterion in the subjective measure was too conservative (see again the discussion in Recommendation 1). Looking at both measures also allows sensitivity and bias to be distinguished. For example, participants' objective performance across PAS levels might indicate a conservative criterion in reporting that they consciously perceived the stimulus if performance on PAS-0 trials is high (but as noted above, such performance might also reflect unconscious processing), or a liberal criterion if performance on higher levels of PAS is low (see also Michel, 2023b; note, though, that calculating sensitivity separately for each PAS level has been criticized due to sub-sampling; Schmidt et al., 2024; Stein et al., 2016).

Although it is advantageous overall, the inclusion of trial-by-trial awareness measures also comes with disadvantages. The first was already discussed in Recommendation 2, with respect to priming studies, where the objective measure might be contaminated by the clearly visible target. A second disadvantage of including trial-by-trial awareness measures is that they might decrease the effects captured with processing measures due to shared resource allocation (i.e., dual task costs, see Kiefer et al., 2023; Fischer et al., 2011; Wentura, Rohr & Kiefer, 2024). At the very least, participants will be presented with two tasks, or three, in experiments where the processing measure involves an additional task (e.g., a priming experiment where a clearly visible target is presented). For some research questions, adding many tasks after each stimulus may not be the optimal solution (see Jiang et al., 2015). This concern can in some cases be mitigated by a more thorough practice session at the beginning of the experiment.

Notably, in some cases it is impossible to administer the objective measure during the task (e.g., in no-report paradigms, Frässle et al., 2014; Pitts et al., 2014). In these cases, we recommend administering an objective measure in a separate session. The order in which the measures should be administered within a trial also remains controversial. These issues are discussed further in the Outstanding issues section.

5. Add catch trials in which no stimulus (or feature of interest) is presented

When using subjective measures, it is often helpful to add catch trials in which no stimulus is presented (e.g., Avneon & Lamy, 2019; Micher & Lamy, 2023) to better assess the validity of participants' subjective reports. Specifically, catch trials allow researchers to estimate the tendency to report awareness when no stimulus was presented and make it possible to calculate signal detection metrics like *d*' based on participants' visibility reports in non-catch vs. catch trials. In case the subjective measure addresses awareness of a feature, rather than the presence of the entire stimulus (see Outstanding issue 2), catch trials should lack this feature (when possible). This could entail, for example, presenting a two-headed arrow or a horizontal line as a catch trial in a study that examines orientation processing of right/left arrows.

Catch trials may indicate that some participants report consciously perceiving stimuli even when none were presented, as is expected on any signal detection model. One option is to exclude participants with many such false alarms, under the assumption that they are not using the scale properly (keeping in mind, however, that reports of brief glimpses – as opposed to reports of clearly seeing the stimulus - are common even when no stimulus is presented; Sandberg et al., 2014). Because such an exclusion is driven by participants' general use of the scale and is orthogonal to the effect of interest, it should not in theory evoke regression to the mean (see Recommendation 8). However, such effects are indeed possible as this might lead to excluding participants with a liberal criterion, such that the remaining sample is biased towards a conservative criterion (thereby risking conscious contamination). Researchers should accordingly carefully consider these issues and explain how their results might be affected by participants' exclusion. Another option is simply to report the number of such participants, and compare the results with and without them (taking into account that in some cases, participants might have false perceptions, but these are not directly relevant to the study of unconscious processing).

The addition of catch trials also allows computing participants' sensitivity (e.g., *d'* in signal detection theory) and criterion, based on their subjective visibility reports in trials with and without stimuli. Criteria estimates can be useful to better interpret above-chance performance in the objective measure. Moreover, when the authors intend for the suppressed stimuli to never be perceived by participants, they can have an indication about whether this is the case by testing if sensitivity is different from zero (although many trials are required for precise estimation; MacMillan, 1986).

6. Ensure adequate precision of the processing and awareness measures

To demonstrate unconscious processing, the sensitivity of both the measure of processing and the measure(s) of awareness must be sufficiently high to support robust inferences. Accordingly, researchers should allocate sufficient trials to both measures (Bates et al., 1992; Dufek et al., 1995). This is especially important for the objective measure, where low sensitivity might lead to a false negative conclusion, namely failing to find evidence for above-chance performance despite residual conscious processing. This could lead to an erroneous conclusion that the effect found on the processing measure can indeed be ascribed to unconscious processing (Leganes-Fonteneau et al., 2021; Meyen et al., 2022; Shanks, 2017; see further discussion in Recommendation 8). Yet, many studies in the field assign more trials to the processing measure than to the objective measure, especially if the latter is administered in a later session (Vadillo et al., 2016). For example, in a recent reanalysis of data from 12 studies that included an objective measure, the average number of trials in this measure was 82.09 (*SD* = 30.28, median = 76.91, IQR = 16.97), while the average number of trials in the processing measure was 334.79 (*SD* = 284.27, median = 212.77, IQR = 552.84) (Yaron et al, 2023).

How many trials are enough? This will inevitably depend on the specific combination of stimulation and task parameters, but we recommend that researchers provide a clear rationale for their choice in the methods section. The rationale can rely on simulations that take into account the estimated level of measurement noise, which differs across experimental situations (e.g., Yaron et al., 2023) and across objective and subjective awareness measures. Notably, in some experimental contexts, there might be an upper bound on the number of trials that can be presented, either because of the nature of the effect under investigation (e.g., inattentional blindness; Mack & Rock, 1998), or because of technical or experimental constraints (e.g., fatigue, training effects, time constraints in a neuroimaging facility or elsewhere). In these cases, researchers may consider running complementary control tests on the same participants (e.g., by asking participants in a neuroimaging experiment to perform a behavioral test in a mock scanner). When such solutions are not possible, we encourage researchers to acknowledge the limited sensitivity or precision of the awareness measure in the publication and be explicit about the level of evidence that they can provide for lack of conscious processing.

Importantly, some researchers use subjective measures to exclude trials where participants reported perceiving the stimuli. Such exclusion inevitably affects the number of trials in the processing measure and in the objective measures. This again raises the issue of sensitivity, as well as potentially leading to a further complication, as some participants might have too few trials in one or several analyses (see Outstanding issue 5). Alongside these issues, post-hoc selection of trials evokes additional problems, which we discuss below (see Recommendation 8).

B. Analyzing the data

7. **When using the PAS, trials rated as “brief glimpse” should not be classified as “unaware”**

When using the PAS as a subjective awareness measure, a majority thought it best not to consider trials rated with the second lowest level of invisibility (“brief glimpse” trials) as trials lacking conscious perception. However, some of us thought otherwise, and the agreement scores were second lowest amongst the recommendations we established (85%, see Figure 2). The rationale of the majority view was that if participants are instructed appropriately, this second lowest rating on the PAS should indicate that they consciously perceived some aspect of the stimulus to some extent (even if briefly). Under this view, it is advisable to only focus on trials where participants clearly stated not having perceived the stimulus (“no awareness” trials). Moreover, while typically “brief glimpse” ratings are associated with higher accuracy, some of us noted that, based on their experience, these ratings are sometimes used as a “default rating” when participants do not pay attention to the stimulus, close their eyes, etc. (though this has not been systematically demonstrated), so that including these trials in the analysis might contaminate the effect of interest with noise and lead to underestimating unconscious processing (e.g., Avneon & Lamy, 2019; Kimchi et al., 2018; Ophir et al., 2020; Micher & Lamy, 2023). In contrast, the rationale of the minority group for considering such trials as “unaware” is that when saying that they only got a brief glimpse of the stimulus, participants report not consciously experiencing the feature of interest, which is more aligned with our recommendation to focus the objective measure on that feature. Thus, not including these trials might lead to an underestimation of unconscious processing (Dienes & Seth, 2010; Michel, 2023b). We agree with this point, but as a group decided to take the more cautionary approach here, especially given the above claim that participants might sometimes be misusing this specific rating.

The disagreement may arise because of the different definitions given to “brief glimpse” in different studies: Defining it as “I saw something but have no idea what it was” is importantly different from “I vaguely saw something”. When deciding which PAS level is relevant to claims of subliminality, this decision should be justified according to what content is relevant to the task and how the PAS levels are defined to participants. In cases where there are sufficient trials, a possible approach is to provide results of analyses for each PAS level (see Outstanding issue 9, and also Ophir et al., 2020, for an analysis that better characterizes “brief glimpse” trials).

8. Examine the possible effect of misclassification due to measurement error

A common question in studies on unconscious processing is whether to perform any type of post-hoc selection on the data. Two such selections are commonly used. First, excluding participants based on above-chance performance in the objective measure (determined, for example, using a predefined threshold, a binomial test, or a Bayesian test; Rouder et al., 2007). Second, excluding trials based on the subjective awareness measure, keeping only trials where participants reported not consciously experiencing the critical content.

The motivation for excluding participants who show above-chance performance, or trials where the stimulus was consciously perceived, is to ensure that the evidence for processing is not contaminated by such participants or trials, and genuinely reflects unconscious processing. However, *post-hoc* selection of data causes regression to the mean, which can in turn lead to false positive results (Schmidt, 2015; Schmidt et al., 2024; Shanks, 2017). To explain, measurement error in the awareness measure(s) (which is probably inevitable) means that some aware participants or trials are likely to be misclassified as unaware and vice versa. If awareness and processing performance are positively correlated, then for these misclassified participants or trials, their processing performance will regress towards the group mean and evidence of unconscious processing will wrongly be inferred. Indeed, evidence suggests that the reliability of many awareness measures is modest at best (Rothkirch et al., 2022; Stein et al., 2024; Yaron et al., 2023), making such misclassification unavoidable. We deem it essential that studies of unconscious processing provide a convincing explanation of why their results are not contaminated by misclassification due to measurement error.

One might strive to avoid excluding participants/trials altogether, by designing the experiment using strong enough suppression methods or by applying individual calibration, so that each participant's objective performance is at chance-level and that all trials are rated as unaware. This might not be a realistic expectation, however: even if calibration succeeds, practice effects occurring over the course of the experiment might lower perceptual thresholds, and stimulus visibility might vary unpredictably on a trial-by-trial basis even given identical stimulation on each trial. In addition, using a suppression method that is too strong might excessively degrade the critical content to the point that it cannot be processed at all. A more feasible solution is therefore to exclude participants or trials and then account for post-hoc data selection at the analysis stage. Several methods have been suggested for testing whether the obtained results might indeed be driven by regression to the mean (e.g., Dienes, 2024; Goldstein et al., 2022; Leganes-Fonteneau et al, 2018; Yaron et al., 2023; Rothkirch et al., 2022). These methods are still new and under development, so we refrain from recommending a specific one (see the *Supplementary Material: How to account for regression to the mean at the analysis stage?* for details). We suggest instead that researchers choose the method that seems most suitable to their experimental context (see also Outstanding issue 7, for a discussion of dissociation approaches that do not rely on demonstrating null sensitivity).

In addition, to determine whether participant exclusion affected the overall results, we recommend that researchers also analyze the data of participants who perform above chance-level in the objective measures (given that there are enough such participants), and report them (see further discussion in the reporting section below).

C. Reporting the results and writing the manuscript

9. Precisely define what is meant by ‘unconscious processing’

The term ‘unconscious processing’ is often used with multiple interpretations, for instance in studies exploring the processing of suppressed stimuli (for review, see Mudrik & Deouell, 2022), experiments focusing on implicit processes affecting behavior (Greenwald & Banaji, 1995; Maresch et al., 2021), or studies of cognitive and social biases that affect people’s decisions without them being aware of those biases (e.g., Gawronski, Hofmann, & Wilbur, 2006; Graham & Lowery, 2004). We accordingly recommend that researchers adopt a cautious approach and be precise about the meaning that they are ascribing to key terms (see Glossary for our definitions of key terms).

More specifically, the following points should be made clear in any report of studies on unconscious processing: (a) what exactly participants were (presumed to be) unaware of (the entire stimulus, a specific feature of the stimulus, etc.); (b) how did the choice of the suppression method potentially affect the type of evoked unconscious processing (Breitmeyer, 2015; Dubois & Faivre, 2014; Peremen & Lamy, 2014); (c) how was awareness measured, i.e., via an objective measure, a subjective awareness measure, or both; and (d) what were the instructions given to participants regarding the measures.

10. Justify the chosen measures and provide conditional interpretations

Given the great heterogeneity of methods in consciousness studies, and the potential effect of methodological choices on the results (Yaron et al., 2022), we encourage researchers to justify their choices explicitly, and in particular the choice of awareness measures. This entails explaining what kind of results are held to demonstrate lack of awareness of the stimulus of interest or stimulus feature (this seems especially important given the lack of consensus around definitions). It is also important to directly address criticisms of the chosen measures (discussed above). Hence, we further recommend that researchers explicitly state in the discussion section exactly how they expect their results to be affected by potential criticisms of their awareness measure(s). For example, this would involve stating how interpretations would change if there was indeed a bias in the subjective awareness measure or if the results were affected by poor reliability of these measures (e.g., misclassification of awareness due to measurement error). This approach ensures that claims are appropriately conditioned on the extent to which the critical stimuli were indeed successfully suppressed.

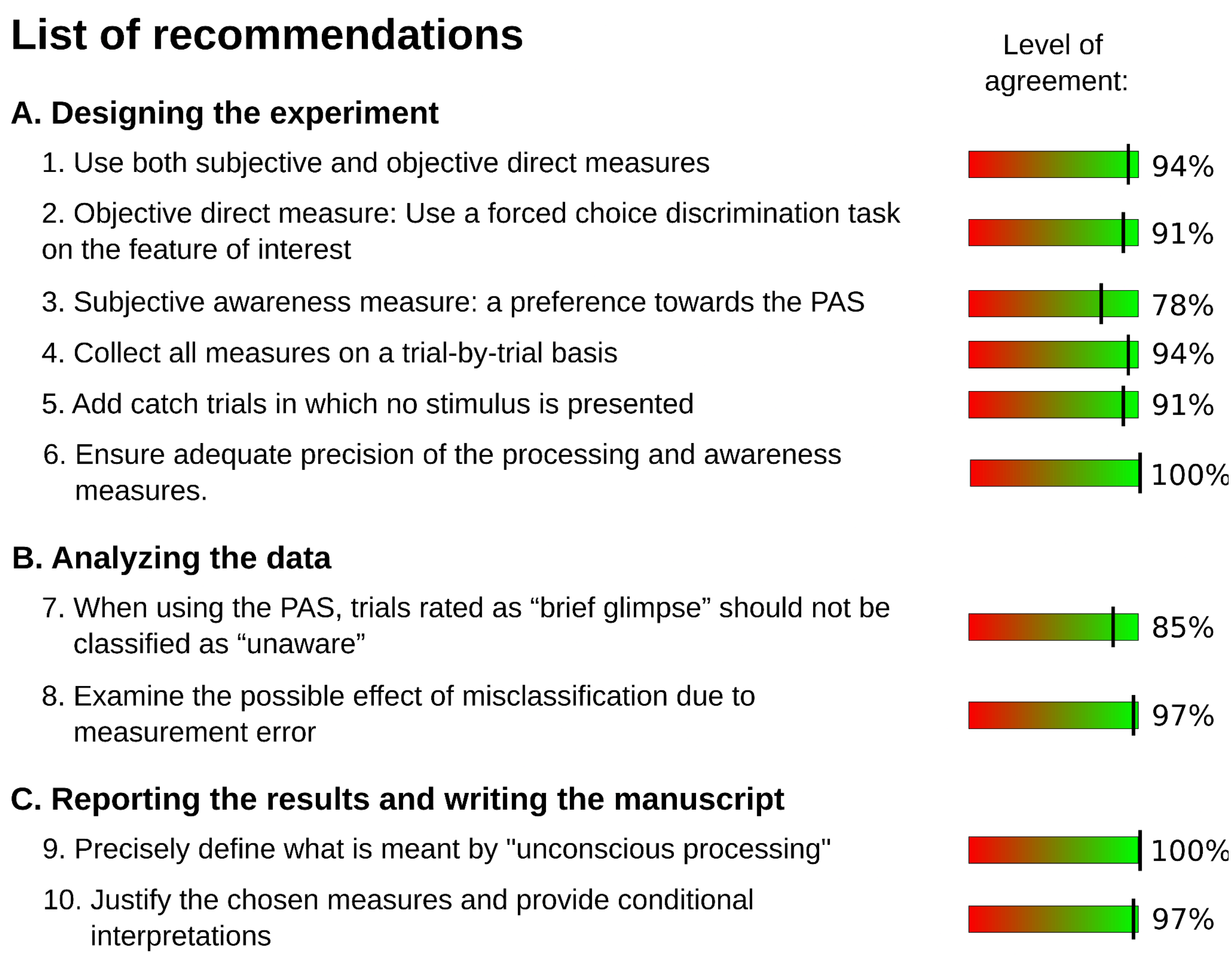

Figure 2. Ten recommendations for unconscious processing research. The level of agreement across all co-authors for each recommendation is shown on the right. For more information about the survey pipeline that led to these recommendations see *Supplementary Material: The original list of recommendations that were voted on during the first two meetings.*

Outstanding issues

Several points raised in our discussions did not yield a consensus, highlighting current controversies and ongoing challenges in unconscious processing research. Below, we elaborate on the main unresolved issues in the hope of accelerating attempts to resolve them. See a summary of these issues, possible solutions and their respective pros and cons, in Figures 3 (issues 1-4) and 4 (issues 5-8).

1. The order in which awareness measures are administered within a trial

In recommendations 2 and 5, we suggested administering three measures on each trial: the processing measure, and the subjective and objective measures. When a separate task is needed for the processing measure, it is typically administered first, since many of these tasks require a speeded response. There is relatively little data on the effect of task ordering of the two awareness measures (e.g., Wierzchon et al., 2014). Thus, we chose not to recommend a specific order.

Possible options include having the objective measure precede the subjective one, or vice versa, as well as alternating between the two (though this might evoke confusion). Another alternative is to probe for both awareness measures with a single question, thereby requiring a single response (e.g., Stein et al., 2021; the subjective-objective measure of awareness, Gelbard-Sagiv et al., 2016; or the prime assessment grid, Sand & Nilsson, 2017). Besides shortening trial duration and reducing the chance of response priming between the processing and awareness measures (as a different motor response is required), one advantage of simultaneous probing is that it has been shown to improve measures reliability in related research (Guggenmos, 2021; Vadillo et al., 2022; 2024). Also, it might reduce the response load, as participants only have two responses to provide on each trial, rather than three. On the other hand, it is also possible that participants find it more confusing to incorporate two responses within one task, and thus it may be more error-prone, in addition to being slower/less meaningful in the reaction time measure. Overall, more data should be acquired to better appreciate the advantages and disadvantages of each approach.

2. The feature to which subjective awareness measures refer

Subjective measures can be used to probe the conscious perception of the presence of an entire stimulus, or of a specific feature of interest (e.g., its color, orientation, etc.). Although the former yields a stricter test, which might eliminate partial awareness of the critical content and contamination of the reported effects by conscious processing (Kouider & Dupoux, 2004; Kouider et al., 2010; Gelbard-Sagiv et al., 2016), the latter tests what is relevant to the subliminal perception enabling task performance (Michel, 2023b). Also, applying the subjective measures on the feature of interest is advantageous as it allows researchers to compare its results with that of the objective measure, which, according to our recommendation, should refer to the feature of interest. For example, in a study investigating unconscious processing of facial expressions, participants might remain unaware of the expression even if they manage to get a glimpse of the hairline, or if they notice that something appeared without having any experience of its content, as typically occurs in visual crowding (Faivre & Kouider, 2011; Schmidt & Biafora, 2022). Under that approach, it seems advisable to use subjective measures to assess participants' awareness of the expression, rather than of the entire stimulus. Note that when using the PAS as a subjective measure, such usage might require modifying its verbal labels to the relevant context, which might require some verification process (cf., Sandberg & Overgaard, 2015). Importantly, the scale should be formulated so that the lowest level clearly means having no awareness whatsoever of either the feature of interest or the stimulus, depending on one's choice (Ramsøy & Overgaard, 2004; Peremen & Lamy, 2014).

3. Block ordering when the objective measure is administered separately

Traditionally, when separate awareness measurement sessions are administered, they precede or follow the processing measure sessions but are not interleaved. This minimizes effects of training during the processing measure session. Conducting the awareness measurement session post hoc also prevents participants from putting attention to the critical content during the processing measure task. On the flipside, however, this may evoke effects of task order, training and fatigue (see

Recommendation 4). In turn, this can be mitigated by collecting awareness measurement data both at the beginning and at the end of the experiment, or by using an ABAB design, where the main experiment (A) and the awareness test block (B) are administered interchangeably. Note that these approaches are not possible in studies wherein administering the objective measure first could change the outcome of the processing measure (e.g., inattentional blindness). Also, this design does not address all the disadvantages of separating the different measures, which is why we generally recommend taking the objective measure on a trial-by-trial basis during the main session (see Recommendation 4).

4. Including clearly-visible stimuli

Some studies include trials in which highly salient stimuli are intended to be clearly consciously perceived by participants, for example by presenting them at a higher contrast or not masking them. Arguably, these trials can be used to assess and improve the reliability of the objective measure (Lin & Murray, 2014; Pratte & Rouder, 2009): when participants fail to perceive the critical content on (almost) every trial, they may become demotivated and adopt a constant, meaningless response strategy. Including highly salient stimuli might alleviate this problem by ensuring that participants are more motivated, less bored and more alert (Pratte & Rouder, 2009). However, such trials can also lead to longer experiments, adding to the effects of fatigue and training that might influence the overall results. There is also evidence that including trials with clearly visible stimuli increases the visibility of stimuli in other trials, which would have not been consciously perceived otherwise (Lin & Murray, 2014), and that consciously exposing participants to the stimuli facilitates unconscious processing (to the point that it might not be present without such an exposure; Faivre et al., 2014; Gayet et al., 2014). Another potential concern is that including such trials will raise participants' criterion, making them report not seeing stimuli that have been faintly experienced.

By and large, these possible positive and negative effects of including trials with highly salient stimuli have not been tested empirically in a systematic manner. For example, the test-retest reliability of the objective measure with or without the inclusion of these stimuli has not been compared. Reliability is expected to be low in case these stimuli are not included, as all participants would perform at chance-level, with no variability in performance as a function of stimulus strength.; Yaron et al., 2022). Including them might help assess the general reliability of the measure, which can be highly informative. We accordingly encourage researchers to perform such tests and report their results. Further, including a condition where the stimulus is consciously perceived can be essential for modeling what performance would be expected for a putative unconscious condition (Dienes, 2015).

It is also important to investigate other methods for keeping motivation high while responding to the objective measures. One avenue involves incentivizing participants to maximize their discrimination performance (e.g., via monetary payoffs). Participants may perform better when paid, compared to when they are not, with evidence that token payment is as effective as monetary payment (Johnson & Bickel, 2002). It will be relevant to test whether such effects can also be demonstrated in the context of objective measures.

	1. The order in which awareness measures are administered within a trial	
	Sequentially	Simultaneously
Issues to consider	Longer time, order effects	Potentially confusing, response load

	2. The feature to which the subjective measures refers	
	The entire stimulus	The feature of interest
Issues to consider	Risk of underestimating unconscious processing, might create a mismatch with the indirect and objective direct measures, which typically refer to the feature of interest	Less strict, possible effects of partial awareness

	3. Block ordering when the objective awareness measure is administered separately		
	Post-test	Pre-test and post-test	Alternating
Issues to consider	Participants may be fatigued leading to underestimation of awareness	Lengthens experiments, training effects, participants might be more attentive to the stimulus in the main task	Awareness measures may yield inconsistent results between pre- and post-group

	4. Clearly-visible stimuli	
	Including	Not including
Issues to consider	Lengthens experiments, might evoke a criterion shift or increase the visibility of other stimuli	Unable to assess reliability of direct measures, participants' motivation may decline

Figure 3. A summary of outstanding issues 1-4.

5. Dealing with participants having too few trials due to trial exclusion

In many experiments targeting unconscious processing that use subjective measures, trials in which participants subjectively reported perceiving the stimuli are removed from analysis, which leads to some participants having too few trials in one or several analyses. There are at least two options to deal with this issue, and a combination of them may be used, depending on the experimental situation. The first is to either exclude participants with a low number of trials in at least one of the conditions of interest (this number should be decided on in advance). Because such an exclusion is driven by participants' general use of the scale and is orthogonal to the effect of interest (unless there is a low number of trials because participants reports are extreme on a relevant variable such as criterion), it should not evoke regression to the mean (see Recommendation 8). Another option is to report the number of such participants, and compare the results with and without them. Alternatively, one can compensate for the low number of trials using appropriate statistical methods (e.g., Generalized Linear Mixed Models; see Brysbaert & Stevens, 2018, for further discussion), or to minimize trial exclusions by designing an experiment in which stimulus intensity is calibrated for each participant at the beginning or during the experiment to be below the detection threshold. Notably, though, such a design is hard to achieve and might lead to excessive suppression of the stimuli, potentially underestimating unconscious processing. Also, it may not apply to all experimental contexts (e.g., the liminal paradigm; Lamy et al., 2019; or inattentional blindness Mack & Rock, 1998). One may also use Bayesian hierarchical modeling or vague individual-level priors biased towards Ho; for example, this is in effect what is done when d' is calculated by adding 0.5 trials to each cell (e.g. Barrett et al., 2013, footnote 6 and associated text).

6. Using the liminal or subliminal approach

Many studies in the field have taken the subliminal approach, aiming to present all critical stimuli under the detection threshold (e.g., Van den Bussche et al., 2009). To compare unconscious processing with conscious processing, some have compared the processing of such subliminal stimuli to that of supraliminal, and sometimes highly salient, ones (e.g., Kiefer & Spitzer, 2000). However, this approach introduces a confound whereby the conditions also differ in the physical properties of the stimuli.

Accordingly, some researchers have opted for presenting stimuli around the threshold, so that the same stimuli evoke conscious or unconscious processing on different trials (Andersen et al., 2016; Avneon & Lamy, 2018; Kimchi et al., 2018; Micher & Lamy, 2023; Micher et al., 2024; Peremen & Lamy, 2014; Van den Bussche et al., 2013). This liminal-presentation approach allows researchers to assess the extent to which different processes exclusively vary with conscious perception (Lamy et al., 2019). Furthermore, they might also help avoid underestimating unconscious processing as the stimuli are less degraded and participants are more motivated (as also indicated in Outstanding issue 4). These approaches are based on post-hoc trial sorting, however, and therefore suffer from the shortcomings discussed above. First, since aware and unaware trials are classified based on the subjective awareness measure, the criterion problem should be addressed (Eriksen, 1960; Fahrenfort et al., 2024; Lloyd et al., 2013). A possible solution can be to also administer an objective measure (see Recommendation 1), to validate participants' subjective ratings (Lamy et al., 2015). Second, trial selection might influence the results due to effects of misclassification induced by measurement error (Fahrenfort et al., 2024; Shanks, 2017; see Recommendation 8). Future studies should focus on addressing the shortcomings associated with taking either the liminal or the subliminal approach.

7. Using the single or the double dissociation approach

As outlined above, when trying to demonstrate unconscious processing, researchers typically look for a simple dissociation between the direct awareness measures (held to indicate null awareness) and the indirect processing measure (which should show evidence of some degree of processing; see *Unconscious processing: definition and measures*; Dienes et al., 1995; Merikle & Reingold, 1988). This single dissociation approach requires that participants are completely unaware of the stimulus (or of the feature of interest), which might require excessive stimulus degradation. More importantly, if the chosen measures fail to properly capture conscious processing, the results might be contaminated by conscious processing or partial awareness (Kouider & Dehaene, 2004; see Schmidt & Vorberg, 2006, for a discussion of this issue; for another approach, see Reingold, 2004).

Hence, some researchers prefer a double dissociation approach whereby an experimental factor is manipulated to produce opposite effects on the indirect processing measure and the direct awareness measure (Biafora & Schmidt, 2022; Schmidt, 2015; Schmidt et al., 2011; Vorberg et al., 2003; see also Merikle & Joordens, 1997; cf. Dunn & Kirsner, 1988). For example, in meta-contrast masking experiments, SOA can be increased so that priming effects become stronger while prime visibility decreases. The underlying rationale is that such double dissociations cannot be explained by a single, conscious factor that drives behavior, necessitating the existence of an unconscious factor that contributes to it. Accordingly, unconscious processing can be demonstrated using stimuli that are consciously experienced on some trials, as long as a double dissociation is demonstrated (Schmidt & Vorberg, 2006).

The advantage of the double dissociation approach is that it does not require a demonstration of participants' complete unawareness of the stimuli. Thus, potential problems of the single dissociation approach are circumvented: There is no need to measure awareness on a trial-by-trial basis (Recommendation 4) or to exclude trials or participants based on above-chance performance in an objective measure (Recommendation 8; for a recent paper suggesting this approach, see Schmidt et al., 2024). The double dissociation approach also relies on less strict assumptions; for example, it does not require that the awareness measures are exhaustive (Reingold & Merikle, 1988). However, it also comes with its own limitations. Double dissociations can be accounted for by single process models (see Dunn & Kirsner, 2003; Juola & Plunkett, 1998), such that finding a double dissociation between the objective and processing measures might not necessarily mean that the latter are driven by unconscious processes (but see Schmidt & Vorberg, 2006, for a reply). Additionally, looking for double dissociations often requires many participants to achieve satisfying levels of statistical power, such that underpowered designs might lead to unfounded conclusions, reflecting noise patterns (Crawford & Garthwaite, 2006). Moreover, so far it seems that this approach is limited to the investigation of low-level types of processing and relatively simple stimuli (Biafora & Schmidt, 2020; but see also Schmidt et al., 2010). It thus remains to be seen whether it can be applied to higher-level cognitive functions and to more complex stimuli. Dunn & Kirsner (1988) argued for reversed associations as counting against single process models: Two double dissociations in opposite directions.

8. **Directly contrasting the processing and objective measures**

According to some (Reingold & Eyal, 2004; Reingold & Merikle, 1988; Meyen et al., 2022), claiming the existence of unconscious processing cannot rest solely on (1) demonstrating a null effect in the objective measure together with (2) an above-null effect in the processing measure, especially given the challenge of establishing a true null effect in (1). According to this view, one should also provide evidence for *a* *quantitative difference between these two effects*: a larger effect in the processing measure than in the awareness measure. To be able to compare the two measures, however, one needs to place them on the same measurement scale (e.g., since the objective measure is typically binary, one would need to also binarize the processing measures, by, for example, using classification on the reaction time data).

Several studies that followed this logic and directly compared the processing and objective measures have failed to find a difference between the two (Fisk & Haase, 2022; 2023; Meyen et al., 2023, 2024; Schnepf et al., 2022, Zerweck et al., 2021; another study found inconclusive results; Huang et al., 2023). This failure to find a larger effect with the processing measure than with the objective measure may be interpreted in one of two ways. One is that the processing effects are not genuinely driven by unconscious processing, but instead are subserved by conscious processing. Alternatively, it might be that the data transformation needed for comparing the two measures reduces the sensitivity of the processing measure, making it harder to find an effect even if it exists. Further research is needed to arbitrate between these two interpretations. Most promising would be to design an objective measure that is as sensitive as the processing measure, as generally speaking, more sensitive awareness measures are needed for a better estimation of conscious processing. Such a design is yet to be formulated; one promising avenue is to go beyond binary measures. For example, one could use confidence reports for a more stratified response coding, but this still requires some development and further thought. Another possible solution is to vary the degree of conscious perception and empirically determine the relation between priming and conscious perception (which will be mostly but not always linear; Skora et al., 2023); this relationship can be used to predict the level of conscious perception needed to explain an amount of putatively unconscious priming, hence empirically calibrating the two to be on the same scale (Dienes, 2015).

9. **Performing multiverse analyses**

In multiverse analyses, instead of taking a single analytic path for a given dataset, researchers perform and report different analyses for their data and explain how conclusions are affected by the analytical choices (Moors & Hesselmann, 2019). A recent study on unconscious processing used simulations and empirical testing to show that such choices - for example, participant exclusion thresholds and the statistical parameters - can indeed heavily affect the results (Stein et al., 2024). Applying multiverse analysis can help mitigate this problem. Taking the example of participant exclusion, this could involve comparing the results under different scenarios. For example, (a) analyzing the data when including all participants; (b) when excluding participants based on above chance-level performance in the objective measure; (c) when excluding only aware or both insensitive and aware participants using the Bayesian awareness categorization technique (Leganes-Fonteneau et al, 2018); or (d) when excluding participants who report being conscious on many trials in the subjective awareness measure.

Although multiverse analyzes have recently become more frequent (e.g., Steegen et al., 2016; Vadillo et al., 2024), they are still not the norm in the field of cognitive science, including unconscious processing (but see Moors & Hesselman, 2019). The main disadvantage of the multiverse approach is the large number of analyses; this requires considerably more effort, and increases the size of the results section of a single paper.

	5. Dealing with participants having too few trials due to trial exclusion	
	Minimizing trial exclusions at the experimental design stage	Exclude many trials and use appropriate statistical methods to address the exclusion
Issues to consider	Hard to achieve, might introduce greater physical differences with clearly visible stimuli	Risks of excluding trials (see Fahrenford et al., 2024; Schmidt, 2015), including regression to the mean on the trial level

	6. Using the liminal or subliminal approach	
	Liminal	Subliminal
Issues to consider	Trial selection is necessary, possible priming of the visibility of suppressed stimuli	Cannot readily compare unconscious and conscious processing, possible underestimation of the unconscious effect as the stimuli are more degraded

	7. Dissociation approach	
	Simple dissociation	Double dissociation
Issues to consider	Requires demonstrating complete unawareness, makes strong assumptions on awareness measures (e.g., exhaustiveness)	Requires more data to achieve satisfying statistical power, can also be explained by single-process models, has not been used to test high-level unconscious processes

	8. Comparing processing and objective measures	
	Direct contrast on same metric	Different metric
Issues to consider	Requires having both measures on the same scale, which typically entails a loss of statistical power for the processing measure.	Requires demonstrating complete unawareness

	9. Analysis scope	
	Single analysis	Multiverse analysis
Issues to consider	Robustness of effects across different definitions of unawareness cannot be assessed	Requires correction for multiple comparisons

Figure 4. A summary of outstanding issues 5-9.

Table 1. Glossary.

Unconscious processing	Processing of a stimulus or a stimulus feature that is not associated with a conscious experience of that stimulus or feature. Claims of unconscious processing typically rely on establishing stimulus processing using a *processing measure* (i.e., behavioral or neural), while providing evidence that the stimulus was not consciously perceived using one or several *awareness measures* (*see below*).

Critical content	The content of a stimulus that is held to evoke the studied unconscious processing. Depending on the research question, it can either be an entire stimulus (e.g., a masked cue predicting the upcoming appearance of a clearly visible target stimulus) or a feature of that stimulus (e.g., whether the prime is smaller or larger than 5).
Processing measure	A measure of performance or neural activity that serves as evidence for the processing of the critical content.
Direct measure	A measure obtained when participants are explicitly instructed to perform a task on the critical content (Reingold & Merikle, 1988). Direct measures can be further subdivided into either subjective or objective (Cheesman & Merikle, 1984; Seth et al., 2008). Direct and indirect measures (see below) were originally defined in the context of priming experiments, where an unconsciously processed prime preceded a consciously processed target. In that context, direct measures refer to tasks that pertain to subjectively perceiving the critical content or being able to perform some judgment on it.
Indirect measure	A processing measure (see above) wherein participants are asked to perform a task on a stimulus other than the critical one (Reingold & Merikle, 1988). Performance on that task is used to infer the processing of the critical content. In the context of masked priming experiments, the indirect measure is focused on the behavioral response to a consciously processed target that is either congruent or incongruent with a preceding prime.
Subjective awareness measure	An introspective report of one's perception of the critical content (Ramsøy & Overgaard, 2004; Zehetleitner & Rausch, 2013). Claims of unconscious processing generally rely on participants' reports of not consciously perceiving the stimulus.
Objective measure	A response about the critical content, pertaining either to its existence (i.e., detection) or to its features (i.e., discrimination). When used as an awareness measure, claims of unconscious processing generally rely on providing evidence of *null sensitivity* in this measure (*see below*), alongside another evidence that the stimulus has been processed. The critical difference between subjective and objective measures is that while the former pertain to reports of one's experience (and can therefore not be externally verified), the latter pertain to the critical content itself, and can accordingly be deemed correct or incorrect.
Null sensitivity	Demonstration that objective performance is at chance-level. Null sensitivity has often been assessed using statistical tests on individual data to select participants for further analyses (but see the *misclassification* issue below) or at the group level by performing a statistical test on the average performance of each participant.
Contamination by conscious processing	A confound that occurs when the results of the processing measure can be explained by residual awareness of the critical content, which was not captured by both the objective and subjective measures (e.g., due to insufficient sensitivity).
Individual calibration	A procedure whereby the presentation parameters (e.g., intensity or duration of the stimulus or mask) are adapted according to each participant's individual detection threshold in order to maximize the signal intensity (e.g., Lamy, Salti, &

	Bar-Haim, 2009; Michel, 2023a), while maintaining unawareness.
Subjective criterion problem	A participant's criterion for reporting subjective perception might lead to an overestimation or an underestimation of the content of their perception. This might stem from differences in instructions, arousal, motivation, etc. Such a criterion problem can overestimate unconscious processing, if participants report faint perceptions as unconscious.
Misclassification of awareness	A statistical phenomenon where participants or trials are misclassified as "unaware" of the critical content due to measurement error in the awareness measure. If awareness of the critical content is positively correlated with the processing measure, then the accidental inclusion of participants who are truly aware but misclassified as unaware could result in (spurious) evidence of unconscious processing (e.g., due to regression to the mean; Shanks, 2017).

BOX 1 - General Recommendations

The recommendations below apply to many different fields of scientific research, yet we hold them especially important for studies of unconscious processing, as we explain below.

I. Ensure the sample size is sufficient to assure adequate power or evidence to test your hypotheses

Having an adequate sample size is crucial for empirical testing (Fraley & Vazire, 2014; MacCallum et al., 1999) and is especially important when it comes to unconscious processing research, for two reasons. First, as we explain above, strong conclusions are often based on null results (e.g., participants' performance being at chance-level in the objective measure). Thus, it is crucial to make sure the sample is sufficiently large to detect an effect, if it exists. Second, unconscious effects are sometimes small (Gambarota et al., 2022; Hedger et al., 2016; Van den Bussche et al., 2009), and not always replicable (Biderman & Mudrik, 2018; Moors et al., 2016; Moors & Hesselmann, 2018; Yu, 2023). Thus, studies using frequentist statistics should be powered for the smallest effect one does not want to miss out on (Dienes, 2021a). Sample size can be pre-determined in several ways (Lakens, 2022), relying on power analyses. An alternative is to adopt open-ended sequential designs (Schönbrodt et al., 2017), where data collection is stopped once there is enough information to reach a conclusion either for or against unconscious processing, using Bayesian statistics with informed priors (Dienes, 2015).

II. Test your experimental setup systematically

Testing one's setup is very important when studying unconscious processing because the typical paradigms usually require highly accurate presentation timings, given the often very short presentation times (e.g., 10 or 20 ms) or stimulus onset asynchronies. We accordingly suggest that researchers conduct timing tests, for example using an oscilloscope or diode measurement, to test the true presentation duration and the potential on-screen aftereffect of a stimulus. These results should ideally be reported (e.g., in experimental materials) to ensure that discrepant results are not simply explained by unplanned variations in stimulus presentation (Lepauvre et al., 2024).

III. Instruct participants clearly and consistently, and document the instructions

Specifically, for the awareness measures to be interpreted accurately, researchers should take great care in the way that they instruct participants about the experiment. For the subjective awareness measure, this means that it should be clearly defined what stimulus the task refers to (i.e., should awareness be probed for the entire stimulus or a feature of interest?) and that participants should understand the labels that go with each level of the scale (and remember them, in case they are not presented in full on each trial). It should be clear to them that the lowest level means having no conscious perception of the stimulus at all, in case the PAS (or any other visibility rating) is used, or having no confidence at all in one's answer (e.g., "I could have just flipped a coin"), in case a confidence level is used. In the objective measure, it is important to ensure that participants understand that they are being shown a stimulus which is very hard to detect, and in some cases might not even appear, and that it is expected that they do not always consciously perceive it. In addition, the experimenter can explain the importance of trying to answer the question as accurately as possible, even when they did not perceive the stimulus (e.g., by relying on their gut feeling).

IV. Pre-register analyses and share data publicly

Since unconscious processing is generally characterized by small effects (Gambarota et al., 2022; Hedger et al., 2016; Van den Bussche et al., 2009), and replication failures abound, it is particularly important to reduce both false positives and false negatives by protecting against post-hoc decisions that might affect the results (Murayama et al., 2014). A more specific problem to the field of unconscious processing is its inherent asymmetry, as studies typically do not distinguish between competing hypotheses, but between a null hypothesis (i.e., no unconscious processing) and the alternative (i.e., unconscious processing). Consequently, such problems as publication bias and selective reporting of successful analyses, over time, lead to the accumulation of false positives. This spuriously increases the corpus of evidence for the scope (rather than limits) of unconscious processing.

For this reason, we recommend that researchers pre-register their analysis plans to reduce degrees of freedom, and take into account the issue of multiple comparisons. When running complementary analyses, researchers should be mindful of the statistical consequences of performing multiple comparisons. Statistical corrections should be performed to make sure false discovery rates are kept below 5% (for guidance, see Benjamini & Hochberg, 1995; Chen et al., 2017). We also recommend authors to share their data publicly in open repositories (or opt for a registered report, where the journal accepts the paper based on the pre-registered methods and analysis plan; Chambers & Tzavella, 2022). Open data - including trial-by-trial information, and not only aggregated data - will also facilitate the pooling of results across studies to precisely quantify the degree of unconscious processing on larger datasets. This recommendation is not meant to discourage exploratory studies; we fully acknowledge the importance of exploration and discovery in science (Coffman & Niederle, 2015). However, we recommend that exploratory experiments be followed by a preregistered replication, or, if this is not a viable option due to some practical constraints, that the results of exploratory analyses are flagged as such and interpreted accordingly.

V. Test for the absence of an effect

As opposed to most empirical tests, where researchers are interested in finding a positive result, the goal of the objective measure is to demonstrate that participants do *not* perform above chance-level (i.e., to confirm the null hypothesis). Classical null hypothesis significance tests are less appropriate in this case because a non-significant result affords two different explanations: (1) the null hypothesis is true, and participants indeed do not consciously perceive the stimuli; or (2) the null hypothesis is false but there is insufficient evidence to reject it. Instead, we recommend that researchers directly test for evidence in favor of the null hypothesis, using either Bayes factors (Dienes, 2016; 2021; Fisk & Haase, 2023) or equivalence tests (Kruschke, 2010; Lakens, 2017). Equivalence tests or Bayesian inference by intervals (Kruschke, 2018) require researchers to explicitly state what is the maximum effect size that they consider to be still small enough to not be scientifically relevant; Bayes factors require indicating the size of effect expected. Note that Bayesian statistics might also be helpful to assess the effect of the processing measure (both in the case of a negative and a positive result).

Some further considerations should be taken into account. First, choosing priors for Bayes factors should be done with care (for guidance, see Dienes, 2021b; Rouder et al., 2012; Rouder & Morey, 2012). Second, when choosing the bounds for equivalence tests, it is important to keep in mind that when bounds are restrictive, equivalence tests require many trials, but in the end, the bounds should be determined by the smallest effect that would count as scientifically relevant (Lakens et al., 2018).

Discussion

Striking a fine balance between avoiding awareness and allowing processing, and obtaining reliable evidence for both, presents a perennial challenge to the field. This paper proposes guidelines for meeting this challenge, relating to designing experiments, analyzing data, and reporting the results of unconscious processing studies. These guidelines reflect the collective views of the authors of this paper, obtained through a comprehensive process of discussions and consensus-establishing practices (Waggoner et al., 2016). While the authors' theoretical backgrounds are diverse, the guidelines proposed here might not have a full coverage of all opinions in the field; along the same lines, the results of the polls reported here should not be taken as an indication of the level of consensus in the field at large, but a measure of the level of agreement amongst the authors of this article. Yet we hope that the present attempt to identify a majority opinion contributes to dialogue and critical discussions, which will lead to a better understanding of the issues at hand and the development of novel approaches.

By clarifying the areas of consensus and contention in the field, and presenting the considerations that underlie them, this manuscript highlights some difficulties that are inherent to the study of unconscious processing. It is evident from this project, and the discussions we held, that current methods are somewhat limited in their ability to fully and accurately assess the extent of conscious vs. unconscious processing, and to manipulate consciousness in a way that both prevents conscious perception of the critical content and preserves a signal strong enough to elicit measurable effects.

Thus, our recommendations should not be taken as a signal that there is already an established consensus in the field, given the fundamental differences between current approaches. However, our contribution highlights research practices that should be avoided and provides a reference point for discussing experimental choices made when studying unconscious processing.

Another important insight that emerged during our discussions is the role of theory in designing experiments and interpreting results. If coming from different theoretical standpoints, researchers can interpret the same result in radically different ways: as seen in previous sections, above chance-level performance in the objective measure is taken by some researchers as a measure of *conscious* processing, but others deem it a marker of *unconscious* processing as long as participants report no experience of the stimulus. While our recommendations mostly consider the former approach (see especially Recommendation 8), the latter approach is also used widely. The controversy about the interpretation of results from objective measures dovetails with an ongoing philosophical debate which asks if perception is always conscious (Phillips, 2018a, 2021), or can also take place unconsciously (Block, 2007, 2011, 2023; Burge, 2010; Peters, 2017; Phillips & Block, 2017). This philosophical discussion is outside the scope of this article, but it provides a wider framework for the controversy around objective measures, if one differentiates between conscious perception, unconscious perception and unconscious processing that does not count as perception (e.g., retinal processing; Berger & Mylopoulos, 2019; Burge, 2010).

Evidently, these theoretical positions affect the operational definitions and consequently the interpretation of results (for a demonstration of this issue with respect to theories of consciousness, see Yaron et al., 2022). Disagreements about interpretation can occur even around a relatively agreed-upon phenomenon such as blindsight (compare Mazzi et al., 2016; Overgaard et al., 2008, to Michel, 2023b). Given such fundamental disagreements, some have expressed scepticism about the field being able to move forward (Irvine, 2012; Phillips, 2018a). However, we think that there are reasons for optimism. Clearly stating the theoretical framework guiding a study allows for more transparent interpretation of results and facilitates their integration within the broader scientific discourse. Shared use and understanding of methods may lead to more refined conclusions (e.g., claims such as "If one believes that consciousness is reflected by x, this result demonstrates unconscious processing; but if one believes that consciousness is reflected by y, it does not" unravel the underlying assumptions behind researchers' choice of methods and measures). This will be a substantial improvement compared with the current situation, where theoretical assumptions are often left unspecified. Importantly, researchers with theoretical disagreements can also benefit from each other's data, especially if it includes information that would allow diverging interpretations (e.g., if it includes both objective and subjective measures, researchers from different frameworks can interpret it differently, while making their theoretical commitments explicit). Our recommendations are intended to foster such collaborative and community-facing approaches that break existing theoretical silos.

We believe that collecting evidence for or against specific theories without consideration for other positions is not sufficient at this stage; a common methodological language could enable comparing and adversarially interpreting the results of studies on unconscious processing. While engagement with our recommendations and outstanding issues will most likely not finally resolve fundamental theoretical disagreements, it will lead to progress by providing a shared basis against which empirical results can be assessed.

Alongside a better understanding of existing methods and measures, innovation is also needed; new approaches could potentially break the ongoing cycle of publishing findings that apparently push the limits of unconscious processing, only for them to be pushed back by methodological criticism (e.g., Schmidt, 2015; Shanks, 2017; Stein, 2019). We envision three directions which these novel approaches could follow. First, we should develop new methods to suppress stimuli from awareness while preserving strong signals. Second, there is a need for new measures of awareness or further refinement of existing measures. For example, within subjective awareness measures, it might be beneficial to combine subjective measures with other second-order meta-cognitive measures of performance in some experimental contexts (Jachs et al., 2015; Peters & Lau, 2015; Zehetleitner & Rausch, 2013). Similarly, for the objective measure, it might prove helpful to explore new, non-binary measures, to equate the sensitivity of the objective measure with that of continuous processing measures like reaction times or eye movements (Meyen et al., 2022). Third, information-based approaches that combine machine learning in neuroimaging studies and computational models might prove more sensitive in unraveling unconscious processing than traditional methods (Mei et al., 2022; Mei & Soto, 2024; Sandberg et al., 2022). Developing novel imaging-based processing measures may require substantial effort, yet, if successful, they might overcome some of the above-mentioned limitations (e.g., multi-task costs, confusion between multiple consecutive responses, or order effects).

Until such innovations are introduced, it seems that open questions on the best way to investigate unconscious processing are inevitable. Nevertheless, we believe that explicit engagement with the recommendations we proposed and the outstanding issues we highlight – and refining them in light of future empirical and theoretical work – will improve the reliability and replicability of research on unconscious processing.

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Supplementary Materials

The supplementary materials include two sections. In the first we describe the process by which the recommendations were established; in the second we discuss current methods for addressing the potential threat of misclassification of stimulus visibility due to measurement error.

1. **The process by which the recommendations were established**

The process of assembling the initial list of recommendations consisted of four polls and three online meetings. These were complemented by extensive online discussions while iterating over the manuscript, where the participants provided comments and suggested changes, some of which were extensive. The first two polls were sent via email and publicized on social media, and were run on Qualtrics.

1.1. First poll: online distribution via social media and personal emails (N=47)

The purpose of the first poll was to collect researchers' own methodological preferences, what they hold to be the preferred methods in the field, and to understand which issues were most important for us to discuss in this project (See **Figure S1** for the full results). It received 47 responses from researchers in the field from various stages of their career (25 tenured, 12 post-doc, 9 graduate students); most of them came from the disciplines of psychology (30) or neuroscience (15). With regard to the methods, in this initial poll the researchers mostly preferred using both objective and subjective awareness measures as the best method to measure awareness (24; 51%), and using a two alternative forced choice task as the objective measure (21; 45%), specifically targeting the feature of interest (29; 62%); PAS was the most voted on option for the subjective awareness measure (20; 43%). When asked to identify outstanding issues in the way we investigate unconscious processing, they indicated issues pertaining to the measures (36% of the issues indicated; e.g., partial awareness; how to make sure participants were indeed unaware of the stimulus or feature, etc.), methods (32%; e.g., how ecologically valid experiments in the field are; subliminal vs. liminal presentation of the prime, etc.), theory (16%; e.g., inconsistent definitions of the phenomena being studied, etc.) and analysis (16%; e.g., having a measure of evidence of no effect, reducing researcher degrees of freedom, etc.). Researchers were also asked how much they thought recent criticisms of the field (e.g., Meyen et al., 2022; Shanks, 2017) should be addressed in the recommendations, and those considered by the group as important were included in the manuscript.

1.2 Second poll: online distribution via social media and personal emails (N=37)

For the second poll, we compiled a list of methodological issues based on the results of the first poll. For each, we either suggested several alternatives, which the researchers had to choose from, or simply made a recommendation and asked them whether they agreed with it. Each option or recommendation was accompanied by a rationale, and a list of advantages and disadvantages. The second poll received 37 responses (14 tenured, 10 postdocs, 13 graduate students; See **Figure S2** for the full results). Some recommendations emerged, like measuring awareness both subjectively and objectively, using an alternative forced choice discrimination for the objective measure, or focusing the objective measure on the feature of interest. They also highlighted many topics that turned out to be too complicated to make clear-cut decisions about at the time. For instance, when asked whether visible trials should be included in the objective test, 8 voted for the option of including such trials, 7 voted for not including them, and the others refrained from answering; The question of whether or not to exclude participants in different circumstances was also deemed controversial.

1.3 Meetings 1 and 2 (N=32 researchers who attended at least one meeting)

Based on these insights, we formed a new list of recommendations (see below). We then conducted two Zoom meetings with the researchers (N=29 for the first meeting, N=27 for the second meeting) who responded to at least one of the polls to discuss each recommendation in detail and subsequently vote on them. Researchers were sent the list of recommendations in advance of the meeting in order to prepare. During these meetings, each recommendation was presented with its rationale, then a discussion was held, and then it was voted on via Socrative (https://www.socrative.com/).

1.3.1 The original list of recommendations that were voted on during the first two meetings

1. Awareness Measures - Use both a subjective and an objective measure
2. When should the objective test be administered?
   Option 1: take the objective test during the task.
   Option 2: take the objective test in a post-test.
3. Simultaneous objective and subjective measures?
   Option 1: The objective and the subjective measures are obtained via two separate questions.
   Option 2: Both measures are obtained using the subjective-objective measure of awareness (SOMA; Gelbard-Sagiv et al., 2016)
   Option 3: More empirical data should be collected to decide between 1 and 2
4. Subjective measure - PAS, administered on a trial-by-trial basis.
5. Objective measure - An alternative forced choice discrimination (rather than detection), focused on the main feature of interest.
6. Trial exclusion - Should PAS level-2 trials ("brief glimpse") be included as 'unconscious', and under what conditions?
7. Should the test-retest reliability with and without visible primes be tested experimentally?
8. Including visible trials in the objective test
9. Number of trials in objective test - 200 included trials as a lower bound
10. Number of trials in the main task - at least 30 included trials per experimental cell
11. What to do if it is not possible to have enough trials for all subjects?
12. Should sample size be pre-specified (e.g., based on a meaningful effect size or with sequential Bayesian analysis)?
13. How should researchers deal with the problem of excluding subjects?
14. Analysis of all subjects who show above chance performance in the awareness test should also be reported.
15. Is it required to demonstrate an effect over more than one method?
16. Demonstrating the absence of an effect - When assessing the results of the objective measure (or the effect of interest), researchers should not use classical null hypothesis significance tests. Instead, we recommend that they report direct evidence in favor of the null hypothesis.
17. How can researchers reduce degrees of freedom in analyses? - Researchers should pre-register their analyses and share data publicly.

1.4 Third poll - online distribution between co-authors, and Third meeting (N=25)

Following discussions during the two meetings and feedback we received thereafter, we sent a third poll (using Google Forms) asking researchers to raise additional topics they thought should be discussed in the final publication. A third meeting was held to discuss these topics, which were presented by the researchers who raised them.

1.5 Manuscript redaction

After the third meeting we wrote the first draft of the manuscript. It consisted of the recommendations that reached consensus and outstanding issues that the group had not reached an agreement upon. Recommendations, outstanding issues and their rationale were refined one last time to reflect remaining concerns and objections from the researchers.

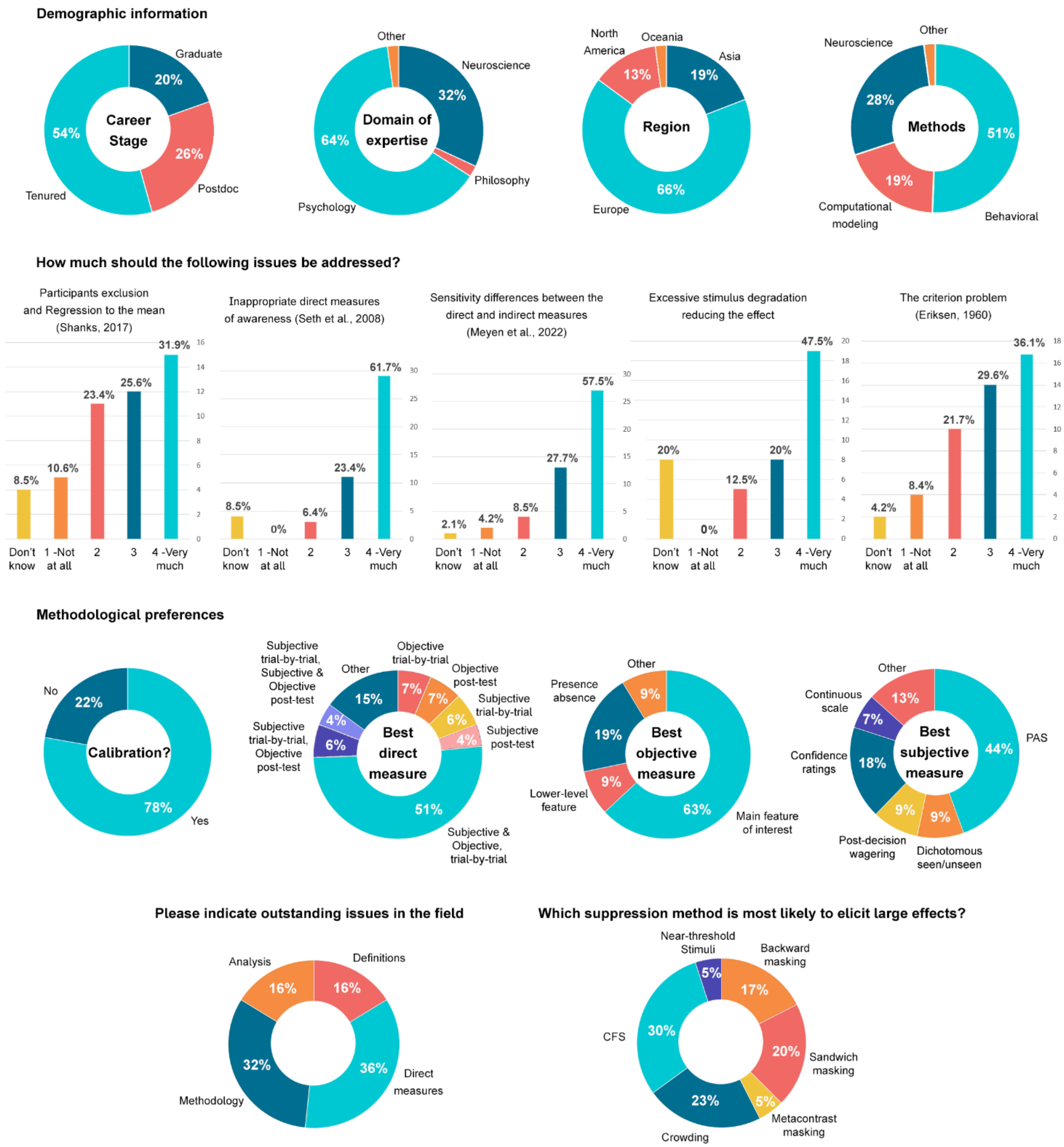
1.6 Fourth poll - online distribution between co-authors (*N*=32)

Following revisions of the manuscript, a final poll (also using Google Forms) was sent to and filled in by all co-authors. This poll included the final wording of the 10 recommendations. The results reported in the article are those of this last poll.

2. How to account for regression to the mean at the analysis stage?

Participant exclusion can bias the conclusions of unconscious processing studies. Specifically, when measurement noise causes the misclassification of participants or trials as “unaware” (and thereby their post-hoc exclusion), this can lead to regression to the mean in the processing measure of the included data. Several solutions have been suggested to address this problem. Given that these solutions are fairly new, we still lack information to arbitrate between them. Instead, we suggest a non-exhaustive, non-mutually exclusive list of options for researchers to explore. These approaches can be divided in two groups, depending on whether they rely on improving participant (or trial) classification, or whether they are aimed at showing that conclusions of unconscious processing cannot be fully explained by regression to the mean. In the former approach, researchers can identify “unconscious participants” using the Bayesian awareness categorization technique (Leganes-Fonteneau et al, 2018), in which a Bayesian procedure is used to classify individual participants as aware, unaware, or insensitive. Drawbacks of this option are that the chosen prior can substantially impact the results, and that even when a large number of trials (e.g., 200) are collected per participant, many will be classified as insensitive. Consequently, this approach might be underpowered (Yaron et al., 2023). In the second group of approaches, researchers do not try to improve the categorization of individual participants as conscious or not, but instead test if the observed effect size is larger than a theoretical effect driven by regression to the mean alone. Here, the following methods have been suggested:

1. Apply a statistical correction for regression to the mean, for example based on Kelley's formula (Rothkirch et al., 2022) or a Bayesian shrinkage approach (Malejka et al., 2021).
2. The Generative Bayesian framework (Goldstein et al., 2022), which involves modeling the data and adjusting the correlation between performances in the processing measure and objective measure based on the estimated measurement error.
3. A nonparametric bootstrapping approach (Yaron et al., 2023), in which surrogate datasets are simulated to obtain a distribution of effects driven by regression to the mean, allowing researchers to test the hypothesis that the observed effect falls within that distribution.
4. An estimation of the measurement error in the subjective awareness measure using modeling, and accounting for that in the analysis (Skora et al., 2023; see also Jurchis & Dienes, 2023, where this was applied for implicit learning).



Demographic information
Career Stage
Graduate
20%
26%
Postdoc
54%
Tenured
Domain of expertise
Other
Neuroscience
32%
Philosophy
64%
Psychology
Region
North America
Oceania
Asia
13%
19%
66%
Europe
Methods
Neuroscience
Other
28%
51%
19%
Computational modeling
Behavioral
How much should the following issues be addressed?
Participants exclusion and Regression to the mean (Shanks, 2017)
8.5%
10.6%
23.4%
25.6%
31.9%
Inappropriate direct measures of awareness (Seth et al., 2008)
8.5%
0%
6.4%
23.4%
61.7%
Sensitivity differences between the direct and indirect measures (Meyen et al., 2022)
2.1%
4.2%
8.5%
27.7%
57.5%
Excessive stimulus degradation reducing the effect
20%
0%
12.5%
20%
47.5%
The criterion problem (Eriksen, 1960)
4.2%
8.4%
21.7%
29.6%
36.1%
Don't know
1 -Not at all
2
3
4 -Very much
Methodological preferences
Calibration?
No
22%
78%
Yes
Best direct measure
Subjective trial-by-trial, Subjective & Objective post-test
Other
Objective trial-by-trial
Objective post-test
Subjective trial-by-trial
Subjective post-test
Subjective trial-by-trial, Objective post-test
Subjective & Objective, trial-by-trial
15%
7%
7%
6%
4%
4%
6%
51%
Best objective measure
Other
Presence absence
Lower-level feature
Main feature of interest
9%
19%
9%
63%
Best subjective measure
Other
Continuous scale
Confidence ratings
Post-decision wagering
Dichotomous seen/unseen
PAS
13%
7%
18%
9%
9%
44%
Please indicate outstanding issues in the field
Analysis
Definitions
Methodology
Direct measures
16%
16%
32%
36%
Which suppression method is most likely to elicit large effects?
Near-threshold Stimuli
Backward masking
CFS
Sandwich masking
Metacontrast masking
Crowding
5%
17%
30%
20%
5%
23%

Figure S1. Results from the first online poll (N=47). This poll assessed the methodological preferences of researchers in the field, and its results were the basis for the recommendations that were presented in the second poll.

Figure S2. Results from the second online poll (N=37). Researchers were presented with a preliminary set of recommendations, based on the results of the first poll. The results here were used to form the final set of recommendations that were discussed and voted on during the meetings, as presented in the main manuscript.

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Figure Captions

Figure 1. A. Example of a visual response priming task (Naccache & Dehaene, 2001). The task is to press one button if the target is larger than 5 and another if it is smaller than 5. The prime frame is preceded and followed by mask frames, and the stimulus onset asynchrony (SOA) determines the extent to which the prime is perceived. In congruent trials (blue), the prime and target both correspond to the same response as both are larger than 5, while they correspond to opposite responses in incongruent trials (red). B. Processing measures can be either behavioral (top panels, showing slower reaction time on incongruent trials) or neural (bottom panels, schematically showing differential responses based on congruency), and consist of averaging responses or activity across trials in the congruent and incongruent conditions (right panels). C. Awareness measures can be subjective (for example, the Perceptual Awareness Scale in the top panel; see Recommendation 3 below) or objective (alternative forced choice on the prime - or the critical content - in the bottom panel; here, determining whether the prime is smaller or larger than 5).

Figure 2. Ten recommendations for unconscious processing research. The level of agreement across all co-authors for each recommendation is shown on the right. For more information about the survey pipeline that led to these recommendations see Supplementary Material: The original list of recommendations that were voted on during the first two meetings.

Figure 3. A summary of outstanding issues 1-4.

Figure 4. A summary of outstanding issues 5-9.

Figure S1. Results from the first online poll (N=47). This poll assessed the methodological preferences of researchers in the field, and its results were the basis for the recommendations that were presented in the second poll.

Figure S2. Results from the second online poll (N=37). Researchers were presented with a preliminary set of recommendations, based on the results of the first poll. The results here were used to form the final set of recommendations that were discussed and voted on during the meetings, as presented in the main manuscript.