Unconscious Visual Working Memory: a critical review and Bayesian meta-analysis

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Abstract

The relationship between consciousness and working memory (WM) has been recently debated both at the theoretical and methodological levels (Persuh et al., 2018; Velichkovsky, 2017). While there is behavioral and neural evidence that argues for the existence of unconscious WM, several methodological concerns have been raised, rendering this issue highly controversial. To address the robustness of the previous findings, here we adopt a meta-analytic approach to estimate the effect size and heterogeneity of the previously reported unconscious WM results, also including unpublished results. We used meta-regression to isolate relevant experimental variables, in particular, consciousness manipulation and the WM paradigm to identify the source of the heterogeneity in the reported effect size of the unconscious WM. Our meta-analysis supports the existence of the unconscious WM effect and critically reveals several experimental variables that contribute to relevant heterogeneity. Our analysis clarifies several theoretical and methodological issues. We recommend that future studies explicitly operationalize the definition of consciousness, standardize the methodology and systematically explore the role of critical variables for the unconscious WM effect.

Keywords: working memory, unconscious processing, meta-analysis, consciousness, subliminal

1 Introduction

Working memory (WM) is one of the most studied topics in psychology and neuroscience. Beyond several definitions (see Cowan, 2017 for a review), WM can be described as the retention of a limited amount of information in a heightened state of availability for goal-directed operations (Cowan, 2017; Luck, 2008). Despite the relevant theoretical and experimental development, the relationship between WM and consciousness is not completely clear.

Baddeley's multicomponent model (Baddeley, 1992, 2000) described WM as a set of memory buffers (i.e., slave systems) governed by a central executive system. The *episodic buffer* (Baddeley, 2000) was the attempt to integrate the concept of consciousness within the WM model, introducing an information integration system among slave systems and long-term memory that contributes to conscious experience. However, as reviewed by Andrade (2001), the relationship between WM and consciousness has not been completely clarified. In fact, the role of unconscious information and the possibility that subliminal stimuli can be short-term consolidated and manipulated was not considered.

Similarly, the Global Workspace Theory (GWT; Baars, 1997, 2005) of consciousness directly links the concept of WM to conscious experience with the crucial role of the attention (i.e., *spotlight of attention*). While the information within the attentional spotlight is necessarily conscious, Baars suggested the existence of WM aspects outside the focus of attention (Baars, 1997). On the other hand, recent studies of "no-report" paradigms raise the possibility that WM is more related to reporting of consciousness, rather than conscious experience *per se* (Tsuchiya et al., 2015; van Boxtel & Tsuchiya, 2015). Indeed, some studies on visual fragile short-term memory suggest that WM is not directly related to conscious perception or its metacognition (Pinto et al., 2017; Vandenbroucke et al., 2014). Again, the relationship between unconscious stimuli and WM consolidation was not clearly addressed.

Empirical evidence for information retention and manipulation outside the focus of attention comes from *state-based* models of WM (reviewed in LaRocque et al., 2014) and retro-cueing paradigms (Oberauer & Hein, 2012; see Souza & Oberauer, 2016 for an overview) suggesting that *unattended* information can be retained for a short time interval and reactivated.

Beyond the attentional manipulation, the study by Soto and colleagues (2011) was the first to demonstrate the WM consolidation and retention of subliminal stimuli. The authors briefly presented a masked Gabor patch within a WM paradigm. The main task was to keep the stimulus in memory regardless of the visibility and then perform a WM task comparing the target orientation with a fully visible stimulus. They assessed the participants' conscious experience in each trial. Surprisingly, when they selected unseen trials and analyzed WM performance in these trials, it was still above-chance. Note that they used a *change detection task* (Pashler, 1988; Rensink, 2002), in which after the encoding and the delay interval, the memorized (invisible) stimulus was compared to another similar (visible) one. To perform this task, the participant had to encode, retain, and then compare a subliminal visual stimulus.

Since the study by Soto and colleagues (2011) several behavioral and neural findings have provided evidence of unconscious WM phenomena (Persuh et al., 2018; reviewed in Velichkovsky, 2017). These findings led to theoretical proposals that incorporate unconscious WM in the models of cognition and try to explain how unconscious information can be retained for a short interval, manipulated and used for goal-directed behaviors (Jacobs & Silvanto, 2015; Soto & Silvanto, 2014, 2016).

However, the existence of the unconscious WM has not been universally accepted in the field. The above mentioned behavioral and neural findings have been criticized at the theoretical and methodological levels. The main issues relate to whether or not the tasks used genuinely involved WM (Stein et al., 2016) and how unconsciousness was evaluated

(Finkbeiner & Coltheart, 2014; Newell & Shanks, 2014; Shanks, 2017; Vadillo et al., 2016).

Thus, it is fair to state that the existence of unconscious WM is controversial at the moment.

While the majority of published results support the presence of the unconscious WM effect, this is not easy to interpret at its face value. Two recent literature reviews by Velichkovsky (2017) and Persuh and colleagues (2018) highlighted difficulty in drawing strong conclusions about the unconscious WM effect due to the relatively low number of studies and the high methodological heterogeneity.

Here, unlike these qualitative reviews, we aim to draw quantitative conclusions by adopting a Bayesian meta-analytical approach. In particular, we pay specific attention to the methodological heterogeneity in the study design. With this method, we try to identify the most relevant variables that explain estimated heterogeneity.

2 Method

2.1 Literature search and inclusion criteria

Studies were selected according to these keywords: (*"working memory" OR "short term memory" OR "short term retention" OR "short term maintenance" OR "visual working memory" OR "visual short term memory"*) AND (*unconscio* OR unawar* OR unseen OR "without consciou*" OR "non-conscio*" OR sublimin**) using Scopus, Web of Science and Psycinfo as databases. Google Scholar, ProQuest, and OpenGrey were used to search for unpublished studies. Backward and forward research was conducted on relevant papers and previously cited literature reviews (Persuh et al., 2018; Velichkovsky, 2017. Figure 1 depicts the PRISMA (Moher et al., 2009) diagram of the literature selection process.

In terms of study selection, we considered the following eligibility criteria:

- The articles had to be in English
- The experimental paradigm had to be visual WM-specific

- The to-be-remembered stimuli had to be directly manipulated in terms of visual awareness at the time of encoding (for an overview see Breitmeyer, 2015)
- Awareness for to-be-remembered stimuli had to be assessed with subjective (e.g., Perceptual Awareness Scale; Sandberg et al., 2010) or objective measures (e.g., Signal Detection Theory; Green & Swets, 1966).
- The WM behavioral performance had to be measured by a direct task (i.e., change detection) and not with indirect measures (e.g., faster reaction times)
- Participants had to be healthy adults

The literature search ended in September 2020. A new literature search from that date revealed that no new studies were published in accordance with the inclusion criteria we adopted.

We selected 13 papers and 2 PhD theses. Nested within the selected papers we have 38 independent effect sizes. Table 1 summarizes all included studies. Below, we provide detailed descriptions of each study.

Table 1 Details of studies included in the meta-analysis. The Published column indicates the source of the effect size (published or unpublished). The Mean Trials is the average number of included trials per participant. WM paradigm is the paradigm used in the study. Blinding is the paradigm to manipulate the conscious experience. Acc Measure is the measure to evaluate task accuracy (accuracy or d'). Target is the category of the to-be-remembered stimulus. WM Task is the relevant information about the target for the task (identity, location, or both). Hedges's g is the computed effect size and SE is the standard error.

Study	Published	Sample Size	Mean Trials	WM Paradigm ^a	Blinding ^b	Acc Measure	Target	Target Duration (ms)	WM Task	Hedges's g	Hedges's g SE
Barton (2018)	Unpublished	17	116	CDT	CFS	Accuracy	Gabor Patch	200	Identity	0.26	0.24
	Unpublished	31	31	CDT	CFS	Accuracy	Bars	500	Identity	-0.14	0.18
Bergström & Eriksson (2014)	Published	21	84	DD	AB	Accuracy	Letter	150	Identity	0.94	0.26
	Published	26	42	DD	AB	Accuracy	Letter	150	Identity	0.76	0.22
	Published	26	22	DD	AB	Accuracy	Letter	150	Identity	0.76	0.22
Bergström & Eriksson (2015)	Published	15	128	DMS	CFS	d'	Face	3,000	Identity-Location	0.54	0.26
	Published	16	194	DMS	CFS	d'	Tools	3,000	Identity-Location	0.75	0.27
Bergström & Eriksson (2018)	Published	25	174	DMS	CFS	d'	Tools	3,000	Identity-Location	0.62	0.21
	Published	26	107	DMS	CFS	d'	Tools	3,000	Identity-Location	0.10	0.19
Dutta et al. (2014)	Published	22	20	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.55	0.22
	Published	15	43	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.71	0.24
King et al. (2016)	Published	16	34	CDT	BM	Accuracy	Gabor Patch	17	Identity	0.38	0.25
Nakano & Ishihara (2020)	Published	18	33	DMS	SM	Accuracy	Small Dot	50	Location	0.41	0.24
Pan (2014)	Published	20	-	CDT	BM	Accuracy	Face	10	Identity	0.26	0.22
Soto et al. (2011)	Published	7	26	CDT	BM	Accuracy	Gabor Patch	16	Identity	1.32	0.48
	Published	7	40	CDT	BM	Accuracy	Gabor Patch	16	Identity	1.28	0.47
	Published	9	40	CDT	BM	Accuracy	Gabor Patch	16	Identity	1.01	0.38
	Published	9	53	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.89	0.37

Taglialatela Scafati (2017a)	Unpublished	12	224	DD	CFS	Accuracy	Playing Cards	500	Location	0.27	0.27
	Unpublished	14	229	DD	CFS	Accuracy	Playing Cards	500	Location	0.22	0.25
	Unpublished	16	224	DD	CFS	Accuracy	Playing Cards	500	Location	0.27	0.24
	Unpublished	19	347	DD	CFS	Accuracy	Playing Cards	500	Location	-0.19	0.22
	Unpublished	15	464	DD	CFS	Accuracy	Playing Cards	500	Location	0.22	0.25
	Unpublished	7	227	DD	CFS	Accuracy	Playing Cards	500	Location	-0.37	0.34
Taglialatela Scafati (2017b)	Unpublished	13	220	DD	BM	Accuracy	Playing Cards	24	Location	0.00	0.26
	Unpublished	10	124	DD	BM	Accuracy	Playing Cards	24	Location	-0.20	0.29
	Unpublished	16	233	DD	BM	Accuracy	Pictures	24	Identity	0.04	0.24
Taglialatela Scafati (2017c)	Unpublished	18	222	DD	BM	Accuracy	Playing Cards	24	Location	0.20	0.23
	Unpublished	18	176	DD	BM	Accuracy	Playing Cards	24	Location	0.07	0.23
Taglialatela Scafati (2017rep)	Unpublished	22	9	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.03	0.21
	Unpublished	12	13	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.23	0.27
	Unpublished	22	3	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.10	0.21
	Unpublished	22	11	CDT	BM	Accuracy	Gabor Patch	16	Identity	0.00	0.21
Trübutschek et al. (2017)	Published	17	80	DET	MM	Accuracy	Square	17	Location	1.24	0.31
	Published	19	62	DET	MM	Accuracy	Square	17	Location	1.63	0.34
Trübutschek et al. (2019a)	Published	38	174	DET	MM	Accuracy	Square	17	Location	1.70	0.22
Trübutschek et al. (2019b)	Published	23	47	DET	MM	Accuracy	Square	17	Location	0.96	0.21
	Published	30	129	DET	MM	Accuracy	Square	17	Location	1.07	0.20
	^a CDT = Change Detection Task, DD = Delayed Detection, DMS = Delayed Match-to-sample Task, DET = Delayed Estimation Task										
	^b CFS = Continuous Flash Suppression, AB = Attentional Blink, BM = Backward Masking, SM = Sandwich Masking, MM = Metacontrast Masking

2.2 Studies description

To facilitate comparisons among results, it is important to define a common set of terms to describe WM paradigms and results. Beyond methodological differences, the common structure of a visual WM task is composed of encoding, retention, and test or probe phases

(Liesefeld & Müller, 2019; Luck, 2008). The encoding phase is combined with a so-called blinding paradigm (Breitmeyer, 2015; Kim & Blake, 2005) to manipulate the stimulus visibility. Then the WM behavioral performance is evaluated assessing the impact of the subliminal information. Using subjective or objective measures (e.g., Signal Detection Theory;

Green & Swets, 1966) the visual experience of the target stimulus is assessed. Figure 2 presents a general paradigm structure that is common among all selected studies.

In a series of 4 experiments, Soto et. al (2011) used a backward pattern¹ masking procedure and participants had to retain a masked Gabor patch orientation for a delay of 1 to 5 seconds. After the delay, a visible Gabor had to be compared with the retained one even if participants reported no conscious experience of the target. After selecting only unseen trials (i.e., lower Perceptual Awareness Scale score), performance was above-chance in all experiments. The performance was unaffected by distractors presented during the retention interval (exp. 2-3) and the presence of two to-be-remembered targets (exp. 4). Using the same paradigm, Dutta and colleagues (2014) replicated this result together with a combined

Figure 2

¹ While the original articles may not be explicit, we were able to distinguish “pattern” and “metacontrast” masking in all papers. In the former type, the mask and the target spatially overlap, while in the latter they do not. We decided to distinguish them because the presence or absence of the spatial overlap likely engages different neural mechanisms in the visibility manipulation (Breitmeyer, 2007; Breitmeyer, 2015).

transcranial direct current stimulation (TDCs)/fMRI registration. Along the same line, King and colleagues (2016) demonstrated an above-chance behavioral accuracy on orientation discrimination for a backward masked Gabor patch.

Manipulating different WM task parameters (number of to-be-remembered stimuli, presence of distractors and retention interval length and task difficulty) and recording magnetoencephalographic (MEG) activity, Trübtschek and colleagues (2017; 2019; 2019) used a metacontrast masking approach together with a delayed location estimation task and demonstrated an above-chance behavioral accuracy for unconscious trials. In all experiments and across experimental manipulations they found a consistent pattern of above-chance accuracy in guessing the to-be-remembered target location. Using a different behavioral approach and a fMRI registration, Bergstrom and Eriksson (2015) demonstrated that both the unseen stimulus identity (i.e., faces) and position can be retained for up to 15 seconds using a continuous flash suppression (CFS) paradigm (Tsuchiya & Koch, 2005). These results have been partially replicated by Bergstrom and Eriksson (2018) in a combined behavioral/fMRI experiment where the same effect was found only before the fMRI session, but not after. Using attentional blink (AB) together with a WM paradigm, Bergström and Eriksson (2014) demonstrated that unseen digits are retained and then identified after a delay interval.

Pan et al. (2014) performed a series of experiments about the relationship between consciousness and WM. According to our selection criteria we included only experiment 2 where the target stimulus was subliminal. Firstly, they presented a masked to-be-remembered face. Then, another face that could either match the to-be-remembered face or not was presented under the breaking CFS paradigm (bCFS; Stein et al. 2011). As soon as the CFS masked face emerged from suppression, participants were asked to indicate the side in which the stimulus was presented. Finally, another face was displayed, and participants were required to perform a change-detection task according to the first to-be-remembered face. They found

that RTs were faster when there was a match between the to-be-remembered face and the CFS-masked face, suggesting that unseen visual stimuli can influence WM operations. Critically, the change-detection accuracy (i.e., the result included in the meta-analysis) was at chance.

Finally, using a sandwich pattern masking approach with different stimulus visibility levels, Nakano and Ishihara (2020) reported an above-chance WM performance for unseen stimuli in a location-based change-detection task. Crucially, after selecting unseen trials only, the above-chance accuracy was no longer statistically significant.

With regard to the unpublished literature, Taglialatela-Scafati (2019 PhD Thesis) performed 9 experiments with a novel experimental paradigm where the location of a cued playing card needed to be guessed after a variable delay. Crucially, compared to other experiments, the author did not explicitly ask participants to be engaged in the WM task. Taglialatela-Scafati (2019 PhD Thesis) also performed 2 conceptual replications and 4 direct replications of Soto and colleagues (2011). The first 9 experiments used CFS (exp. 1–6) and backward masking (exp. 7–9). Overall, there was no evidence for an above-chance behavioral effect. To note, a crucial aspect of experiments 1–9 is that authors selected subjects, rather than trials. They included all trials from the subjects for whom the consciousness invisibility was statistically supported (i.e., chance detection performance on a separate session). This is quite unlike other studies which included all participants but selected a subset of trials performing the post-hoc trial selection. Note that we are explicitly agnostic about the advantages and disadvantages of these methods, which have been discussed elsewhere (e.g., Shanks, 2017). We considered the work by Taglialatela-Scafati (2019 PhD Thesis) as 3 separate papers based on the three methods they employed (exp. 1–6 CFS; exp. 7–9 backward masking; exp. 10-15 - conceptual and direct replications).

Barton (2018 PhD thesis) performed two EEG experiments using a change-detection approach and CFS to study the influence of unseen VWM information on the visible

counterpart. They also tested the behavioral change-detection accuracy for unseen information without finding above-chance performance.

2.3 Meta-regression moderators

A lot of research has been conducted to understand how the sensory inputs that do not give rise to consciousness can still impact behavior and brain activity. Traditionally, it has been regarded that there are severe limits in such unconscious information processing (Merikle et al., 2001; Reingold & Merikle, 1988). There is an ongoing debate about which high-level cognitive functions can be deployed without consciousness (see Hassin, 2013; Hesselmann & Moors, 2015; Goldstein & Hassin, 2017, Shanks, 2017). More agreement regarding unconscious processing of low-level visual stimuli exists (for a review about unconscious effects under CFS see Tsuchiya & Koch, 2005; Stein, 2019; Stein & Peelen, 2021). Notably, however, the unconscious processing of low-level sensory information has also been criticized (Peters et al., 2017; Peters & Lau, 2015). We remain agnostic about this debate. While the meta-analytical approach allows us to combine different research results and assess the impact of contrasting methodological choices, it cannot directly address such underlying theoretically related issues.

An important aspect of unconscious processing literature is the experimental manipulation and the assessment of conscious experience. In this paper, we distinguish 1) *attention manipulation*, which includes attentional blink (e.g., Shapiro et al., 1997), 2) *sensory manipulation*, which includes masking techniques (B. Breitmeyer, 2007; B. G. Breitmeyer & Ogmen, 2000) and 3) *interocular suppression* approaches, which includes CFS (Tsuchiya & Koch, 2005). As it is likely that these three classes of methods achieve unconscious processing with different mechanisms (Breitmeyer, 2015; Breitmeyer et al., 2015; Kim & Blake, 2005), we directly considered the adopted *blinding technique* as an explanatory variable in the meta-analytic model.

As mentioned above, in most unconscious WM papers, a subset of trials is selected according to the subjective experience assessed with discrete or continuous scales (Sandberg et al., 2010; Sandberg & Overgaard, 2015; Wierzchoń et al., 2014). While this method brings a clear advantage of having a trial-wise measure of consciousness, it comes with the cost of reducing the statistical power due to fewer trials. Furthermore, post-hoc trial selection could bias the data analysis due to the regression to the mean (Shanks, 2017). Thus, we decided to include the average number of unseen trials per subject for each experimental condition as an explanatory variable to estimate its impact.

The experimental paradigm used to measure WM capacity/accuracy is another crucial aspect that varies significantly across the studies, which might explain the heterogeneity in the unconscious WM effect size. As said before, Soto and colleagues (2011) used a change detection task (CDT) where a to-be-remembered stimulus is retained for a given period of time (from 1 up to 5 seconds) and then the subject performs a task where the retained information is task-relevant. We distinguish three more WM memory paradigms: the *delayed estimation task* (DET) that employs a continuous probe display instead of a dichotomous approach (Bays et al., 2009; Zhang & Luck, 2008), the *delayed match-to-sample* (DMS; e.g., Daniel et al., 2016) which is another popular WM paradigm, and the *delayed detection* (DD), in which subjects perform a forced-choice detection task about the to-be-remembered stimulus after a temporal delay.

Another important parameter in WM paradigms is the target presentation time that is directly related to the WM encoding process (e.g., Vogel et al., 2006). We included it as the explanatory variable.

2.4 Statistical model and effect size computation

In terms of the dependent variable, most of the studies report the percentage of correct responses on the WM tasks and a one-sample t-test against the chance level as evidence for the

behavioral effect. For this reason, we compute the Cohen's d_z as an effect size index that is considered the most appropriate and flexible choice from available data (Cohen, 1988; Lakens, 2013). When directly available, effect size measures were extracted (or computed) from papers using available information. If not available, we extracted critical information (e.g., mean and variability of the accuracy) from plots using Webplotdigitizer (Rohatgi, 2017). Given the relatively small size of selected studies, we calculated the Hedges' g as effect size measures applying a small-sample correction factor to Cohen's d_z measures (Hedges, 1981).

To combine the results of the selected papers, we adopted a random-effect meta-analysis model. Similar to a fixed-effect model, the random-effect meta-analysis estimates an average effect. However, the latter assumes and estimates also the between-paper heterogeneity (τ ; *tau*), which estimates the variability of the phenomenon due to methodological differences employed in different studies (Borenstein et al., 2009).

A single paper in cognitive neuroscience often contains more than one experiment. Further, any given experiment may contain multiple results due to manipulations in experimental conditions. This nested structure makes the effect sizes not independent, which can undermine the model parameters estimations (Van den Noortgate et al., 2013). To address the effect size dependencies, we used the three-level meta-analytic approach without collapsing or excluding information (Assink & Wibbelink, 2016; Cheung, 2014, 2019; Van den Noortgate et al., 2013). The three-levels meta-analysis extends the standard random-effect meta-analysis adding the estimation of the within paper variability along the between-papers variability. Level 1 refers to the participants' level that is summarized with the effect size and the sampling variability calculated from summary statistics. Level 2 refers to the variability among effect sizes nested within papers. Level 3 refers to the variability between different papers. We computed the *intraclass correlation coefficient* (ICC) as suggested by Cheung (2014)

separately for level 2 and 3 in order to estimate the percentage of observed heterogeneity at each level.

For multiple effect sizes within the same experiments, we estimate an overall effect and variance using the approach suggested by Borenstein and colleagues (2009, p. 225) assuming a correlation of 0.5.

We computed the three-level model under a Bayesian framework using R (R Core Team, 2020) and the *brms* package (Bürkner, 2017) based on STAN probabilistic programming language (Carpenter et al., 2017; Stan Development Team, 2021). We performed the publication bias assessment using the *metafor* package (Viechtbauer, 2010).

We employed the Bayesian approach for three reasons. First, given the relatively low number of selected studies, traditional methods could underestimate the real heterogeneity (see Williams et al., 2018 for simulations). Second, within a Bayesian framework, we can interpret posterior distributions in probabilistic terms as a range of plausible values combining observed data (i.e., the likelihood) and prior knowledge (i.e., prior distributions). Third, we consider a dichotomous *p-value* approach as inadequate (McShane et al., 2019; Williams et al., 2018). Following Williams and colleagues (2018) and Molto and colleagues (2020) notations, we formalized the overall three-level random effect model in Equation 1.

$$\hat{\mu} = \alpha + \alpha_{paper[j]} + \alpha_{study[ij]} + \varepsilon_{ij}$$

$$\varepsilon_{ij} \sim Normal(0, \sigma_{ij})$$

$$\begin{aligned}\alpha_{paper[j]} &\sim Normal(0, \tau_{paper}) \\ \alpha_{study[ij]} &\sim Normal(0, \tau_{study}) \\ \alpha &\sim Normal(0, 2) \\ \tau_{paper, study} &\sim HalfCauchy(0, 1)\end{aligned}$$

Equation 1: Formulation of the three-level meta-analysis model

Each effect size is a linear combination of a general intercept (i.e., the overall average effect) and each paper/study deviation plus the sampling error of each effect that is computed together with the effect size measure. Bayesian modeling needs prior distributions on each estimated parameter. We selected a normal distribution ($M = 0$; $SD = 2$) as regularizing prior (see Gelman, 2006) to limit the impact on posterior distribution estimation. We chose this prior because it can exclude very implausible effects size values (i.e., roughly 90% of plausible effect size values range between -3 and 3 in terms of Cohen's d). We chose the prior on between-papers variability (level 3) and within-papers variability (level 2) based on Williams and colleagues' (2018) simulations and Van Erp and colleagues' (2017) meta-analysis. In particular, the latter estimated an empirical distribution of τ from psychological literature.

Beyond the overall effect, the estimated heterogeneity can be explained within a meta-regression framework using theoretically relevant variables extracted from the selected studies.

We consider the following moderators:

- Type of blinding technique
- The type of WM task
- The target presentation time
- The average number of trials per participant

To evaluate the meta-regression models with categorical variables, we used the cell means parameterization (i.e., removing the intercept). This allowed us to compute desired contrast from posterior distributions (Schad et al., 2020) and to evaluate the average effect size in a specific condition. For both the overall meta-analysis (i.e., intercept-only model) and the meta-regression analyses the models were computed using both published and unpublished studies. Given that meta-regression parameters represent the estimated marginal mean of the specific moderator level (categorical moderators), we used the same priors as the overall model.

To test the hypothesis in the Bayesian framework, we adopt the Region of Practical Equivalence (ROPE; Kruschke & Liddell, 2018) approach. In ROPE, the parameter posterior distribution is compared to a range of values that are assumed equivalent to the null value. ROPE can be considered as the Bayesian version of the equivalence test (Lakens, 2017).

Following the suggestion by Kruschke (2018), we compare the 89% Highest Posterior Density Interval (HPDI; 89% of most plausible parameters values) for each relevant model parameter with the defined ROPE. If the HPDI falls completely outside the ROPE we reject the hypothesis that the parameter estimated value is equivalent to the null. If the HPDI is completely within the ROPE we can support the absence of effect. In other cases, we cannot clearly support one or the other scenario, but we can quantify how much posterior distribution is outside the ROPE without taking an inferential decision.

We set the upper limit of the ROPE as 0.1 in terms of standardized effect size (i.e., negligible effect; Cohen, 1988). The lower bound is not specified given that a one-sample effect size on proportions is not symmetric on the null value (i.e., chance performance is 0 in terms of effect size).

To estimate the overall moderator effect, we compared models using the leave-one-out cross-validation criterion (LOO-CV; Vehtari et al., 2017). With LOO-CV, we assign weights to individuate the model with better prediction accuracy. To assess the explained heterogeneity

for each clustering level, we computed an R^2 value for level 2 and 3 as suggested by Cheung (2014). We computed the R^2 using the median as a point estimate of the heterogeneity posterior distributions.

An important step in meta-analysis is the publication bias assessment (Rothstein et al., 2006). To mitigate this problem, we took two measures. First, we searched and included unpublished literature. Second, we adopted a descriptive approach using a funnel plot (Sterne et al., 2005). Funnel plots depict observed effects as a function of standard errors, centered on the estimated meta-analytic effect. Because of this, for low precision studies, the shape of the funnel becomes broad around the estimated effect, unlike those for high precision studies. Funnel plot asymmetry indicates possible missing (unpublished) studies. To quantify the asymmetry, we use the Egger regression (Egger et al., 1997) where we regress effect sizes with standard errors. The regression slope should be estimated around zero if no funnel plot asymmetry is present. In general, with a low number of studies and high heterogeneity, publication bias assessment should be treated with caution (Marks-Anglin et al., 2020).

All data and analysis scripts to reproduce the results are available on the OSF repository (<https://osf.io/4bfnm/>).

3 Results

Firstly, we fitted an intercept-only regression model to estimate the overall effect size and the paper/study heterogeneity. All diagnostic values are below the critical point (i.e., 1.1; Gelman & Rubin, 1992).

The overall effect size is 0.54 (SE = 0.14; 89% HPDI [0.32, 0.77]), the between-papers variability is 0.50 (SE = 0.12; 89% HPDI [0.33, 0.68]) and the within-papers variability is 0.06 (SE = 0.05, 89% HPDI [0, 0.14]). The overall effect size is considered medium (Cohen, 1988) and the posterior HPDI is completely outside the ROPE. Between-paper heterogeneity is

relatively high compared to the meta-analytic estimation of Van Erp and colleagues (2017) where plausible *tau* values range from 0 to 0.4.

To estimate the impact of prior choices on the estimated effect size and heterogeneity, we performed a sensitivity analysis using a range of priors with different standard deviations ranging from 0.1 to 10. Beyond very informative prior distributions ($SD < 0.5$), the impact of prior specification is negligible (see the supplementary materials for details).

In Figure 3 we represent the forest plot for the overall model (published and unpublished literature) with posterior distributions for the estimated average effect along with each paper-level effect.

The estimated heterogeneity is mainly distributed at level 3 (variability between papers) with an intra-class correlation of 0.99 compared to the level 2 (variability between studies nested within papers) with 0.01. This means that the heterogeneity is driven by between-papers differences and there is low variability between experiments of the same paper.

To detect any individual paper or study that influenced the overall effect size and the estimated heterogeneity, we performed a leave-one-out sensitivity analysis. Removing one paper at a time, we obtained the variability in leave-one-out as 0.03 (min = 0.47; max = 0.58) for the overall effect size, 0.02 (min = 0.47; max = 0.51) for level 3 and 0.004 (min = 0.05, max = 0.07) for the level 2. Removing one study at a time, we obtained the overall effect size variability as 0.01 (min = 0.47; max = 0.56), 0.02 for level 3 (min = 0.41; max = 0.52) and 0.003 for level 2. (min = 0.05, 0.07). Given that effect size and heterogeneity estimation are stable, we conclude that there is no highly influential individual paper or study.

Next, to assess publication bias, we used the funnel plot (Figure 4; Sterne et al., 2005).

Figure 4

There is a subtle asymmetry on the left side, with low precision studies (SE of near 0.5) on the right side. To quantify this visual impression of asymmetry, we used a regression model using the *regtest* function from *metafor* (Viechtbauer, 2010) with the effect size standard errors as predictor (Marks-Anglin et al., 2020). We found evidence for funnel plot asymmetry ($z = 2.35, p = 0.0185$).

As we mentioned above, the majority of included studies perform a post-hoc trials selection according to the reported visibility level. We reasoned that the number of trials available per subject could influence the unconscious WM effect. While the mean number of available trials *per* subject is 119, it ranges widely from Min = 3 to Max = 464, with a large standard deviation of 106. In terms of the percentage of discarded trials, the mean is 47%, but it ranged from Min = 1% to Max = 95%, with a large standard deviation of 27%. This reduces the precision of the subject-level estimation and the overall statistical power. In terms of the blinding techniques, the CFS is the most efficient with a mean percentage of discarded trials of 19% compared to attentional blink (70%), backward pattern masking (56%), and backward metacontrast masking (64%).

Finally, we computed the prediction interval related to the estimated effect size. Prediction intervals are an important metric that estimates the distribution of plausible effect sizes for a hypothetical new study considering all variability sources (Borenstein et al., 2009, pp. 127–133; IntHout et al., 2016; Spineli & Pandis, 2020). The 89% prediction interval is calculated with the formula provided by Riley and colleagues (2011) using the median of the posterior distribution of *tau* values as point estimates. For the overall model, the lower and upper bound of the 89% prediction interval are 0.09 and 0.99. Clearly, the range of plausible values for a new hypothetical study is broad under both models suggesting that despite being confident about the average effect, the high heterogeneity leaves us uncertain in terms of the prediction for future studies.

3.1 Meta-regression

Meta-regression allows us to explain between-paper variability using some paper-level variables. Unfortunately, due to a small number of the selected studies, some study level variables, such as the type of paradigm, the blinding technique or the type of stimulus cannot be tested in the same model. Another difficulty of moderator analysis is that when multiple experiments are carried out by the same research group using a unique methodology, a single moderator can produce spurious effects. Given that we could not combine all categorical moderators in a single model, all meta-regression analyses need to be treated with caution due to possible confounding effects.

Thus, we consider all our moderator analyses exploratory. Table 2 contains all model estimates, standard errors, HPDIs and percentages of ROPE/HPDI overlapping for each model parameter. Figures 5 and 6 depict posterior distributions for all meta-regression analyses.

From Figure 5 together with our level 3 analysis described in the previous section, the effect size is dependent on the research groups (see Table 2 for how we grouped them). For this reason, we fitted a meta-regression model using *first authors* as predictor to explore its impact on the effect size estimation. Overall, the authors model has a level-2 R^2 of 0 and a level-3 R^2 of 0.91. As shown in Figure 5, this suggests that there is a strong effect of the author groups, which we come back to in the Discussion.

Figure 5

In order to assess the impact of the sample size and the mean number of trials per subject, we fitted a model with the mean-centered predictors. This allows us to estimate the overall effect size for an average sample size and number of trials together with a systematic effect size variation. The effects of both trial number and sample size are estimated at almost 0, and no relevant portion of heterogeneity is explained by this model, as indexed by an R^2 of 0 at both levels of analysis.

Table 2 Meta-regression table. For each model parameter is reported the estimated value with the standard error and the 89% HPDI. For relevant parameters is also reported the proportion of HPDI within /outside the ROPE. For heterogeneity estimation (tau) the ROPE is not reported. The ROPE is not reported for the implicit vs explicit instructions model because it is considered completely exploratory.

Model	Parameter	Effect	SE	89% HPDI		ROPE	
				Low	High	Within	Outside
~ Blinding Paradigm	AB	0.78	0.35	0.23	1.33	0	1
	BM	0.34	0.13	0.13	0.55	0.01	0.99
	CFS	0.25	0.18	-0.03	0.54	0.17	0.83
	MM	1.33	0.22	0.98	1.68	0	1
	Tau Paper	0.30	0.10	0.16	0.46	-	-
	Tau Study	0.06	0.05	0.00	0.14	-	-
~ Target Duration	150-500ms	0.30	0.32	-0.22	0.81	0.25	0.75
	16-50ms	0.63	0.18	0.35	0.91	0	1
	3000ms	0.46	0.40	-0.16	1.11	0.16	0.84
	Tau Paper	0.52	0.13	0.34	0.72	-	-
	Tau Study	0.06	0.05	0.00	0.14	-	-
~ Trials + Sample Size	Average	0.57	0.16	0.32	0.82	0	1
	Trials	0.00	0.00	-0.00	0.00	1	0
	Sample Size	-0.00	0.01	-0.01	0.01	1	0
	Tau Paper	0.53	0.13	0.35	0.74	-	-
	Tau Study	0.06	0.06	0.00	0.14	-	-
~ WM Paradigm	CDT	0.38	0.17	0.11	0.64	0.02	0.98
	DD	0.25	0.20	-0.05	0.56	0.19	0.81

	DET	1.34	0.24	0.95	1.71	0	1
	DMS	0.45	0.24	0.07	0.82	0.04	0.96
	Tau Paper	0.34	0.10	0.20	0.49	-	-
	Tau Study	0.06	0.05	0.00	0.14	-	-
~ Author	Barton	0.01	0.25	-0.36	0.41	0.66	0.34
	Bergstrom	0.60	0.15	0.37	0.82	0	1
	Dutta	0.61	0.26	0.20	1.01	0	1
	King	0.37	0.32	-0.13	0.90	0.18	0.82
	Nakano	0.40	0.31	-0.09	0.89	0.14	0.86
	Pan	0.25	0.30	-0.23	0.73	0.29	0.71
	Soto	1.07	0.29	0.61	1.51	0	1
	TaglialatelaScafati	0.07	0.12	-0.11	0.25	0.63	0.37
	Trubutschek	1.30	0.16	1.06	1.57	0	1
	Tau Paper	0.15	0.11	0.00	0.29	-	-
	Tau Study	0.07	0.05	0.00	0.15	-	-
~ Instructions	Implicit - Explicit	-0.57	0.38	-1.16	0.04	-	-
	Explicit	0.62	0.14	0.39	0.84	-	-
	Tau Paper	0.47	0.12	0.30	0.65	-	-
	Tau Study	0.06	0.06	0.00	0.15	-	-
Overall Model	Average	0.54	0.14	0.32	0.77	0	1
	Tau Paper	0.50	0.12	0.33	0.68	-	-
	Tau Study	0.06	0.05	0.00	0.14	-	-

Figure 6 summarizes all other 4 moderator analyses. Figure 6A shows the effect of the blinding paradigm. This factor has a level-2 R^2 of 0.13 and a level-3 R^2 of 0.63. For convenience we considered the sandwich masking approach by Nakano and Ishihara (2020) together with the backward masking. The metacontrast masking, backward masking and attentional blink HPDIs are almost outside the ROPE area. The continuous flash suppression is associated with the lower effect size ($\beta = 0.25$, $SE = 0.18$, 89% HPDI = $[-0.03, 0.54]$) and 17% of the HPDI is contained within the ROPE.

Figure 6

Figure 6B shows the effects of instructions. Soto and Silvanto (2014) and Pan and colleagues (2014) noticed the role of instructions in modulating the unconscious WM effect. In particular, being explicit about the needs for a WM retention during the trial seems an important factor even for unseen information. In our considered studies, Taglialatela-Scafati (experiments 1-9; 2019) used a more implicit paradigm compared to other papers, without giving explicit instructions about the to-be-remembered stimulus. For this reason, we fit another model contrasting Taglialatela-Scafati's (2019) experiments with other "explicit"

experiments. We dummy coded the instruction factor in order to estimate also the average effect for the explicit-only condition. The average effect for the explicit-only condition is 0.62 (SE = 0.14, 89% HPDI = [0.39, 0.84]) and the difference between implicit and explicit condition is -0.57 (SE = 0.38, 89% HPDI = [-1.16, 0.04]). The unbalanced number of studies reduces the estimation precision (i.e., the HPDI coverage is large and contains 0) but the type of instructions could have an impact on the unconscious WM effect. (See Discussion).

Figure 6C shows the effect of the WM paradigms. This factor has a level-2 R^2 of 0.09 and level-3 R^2 of 0.54. Only the delayed-estimation task (DET) adopted by three studies (Trübutschek et al., 2017; Trübutschek, Marti, & Dehaene, 2019; Trübutschek, Marti, Ueberschär, et al., 2019) and colleagues is completely outside the ROPE ($\beta = 1.34$, SE = 0.24, 89% HPDI = [0.95, 1.71]). For the other three WM paradigms (CDT, DD and DMT), we have more uncertainty given the partial overlap. Figure 6 depicts the posterior distributions for each model parameter.

Figure 6D shows the effect of the target duration (16-50ms, 150-500ms and 3000ms), where only targets presented up to 50ms are associated with an effect size posterior distribution completely outside the ROPE. This is strongly related to the effects of the blinding paradigm, as, critically, the CFS is the only paradigm that can achieve invisibility over employed only with target durations above 50ms. The papers by Trübutschek and colleagues (Trübutschek et al., 2017; Trübutschek, Marti, & Dehaene, 2019; Trübutschek, Marti, Ueberschär, et al., 2019), whose effect size is large in Figure 6, used a target with a short duration (~17ms) which was metacontrast masked (MM in Figure 6a). Due to the strong correlations among authors, durations and blinding paradigms, it is not easy to isolate the specific contribution of each factor (See Discussion). Overall, the target duration does not explain a relevant part of heterogeneity (level-2 $R^2 = 0.04$, level-3 $R^2 = 0$).

In order to compare the contribution of each moderator, we adopted a model comparison approach using the leave-one-out cross validation. In Table 3, we reported, for each model, the leave-one-out information criterion (LOOIC) with relative standard error and the model weights (Yao et al., 2018). LOOIC could be interpreted as the probability that the model will make the best prediction in new data conditional upon the set of considered models (McElreath, 2020). The blinding paradigm and the WM paradigm models are associated with the highest weight with similar R^2 and LOOIC values.

Table 3 LOOIC and R^2 values of meta-regression models. The weight column is the probability that a given model is the best according to the LOOIC criterion.

Model	Estimate	SE	Weight	$R^2_{\text{level-2}}$	$R^2_{\text{level-3}}$
~ Author	-7.66	3.56	0.03	0.00	0.91
~ Blinding Paradigm	-4.93	2.65	0.45	0.13	0.63
~ WM Paradigm	-4.92	2.60	0.46	0.09	0.54
~ Target Duration	-8.18	3.58	0.02	0.04	0.00
~ Trials + Sample Size	-8.10	3.54	0.02	0.00	0.00
Overall Model	-8.11	3.97	0.02	0.00	0.00

4 Discussion

Overall, the meta-analysis supports evidence for a medium effect (Cohen, 1988) together with high between-papers heterogeneity. Meta-regression analysis showed that beyond the highly heterogeneous methodology among authors, the psychophysical consciousness manipulation and the WM paradigm are crucial factors.

Our meta-analysis confirms the presence of an unconscious behavioral WM effect (Figure 3). At the same time, the estimated heterogeneity among studies is considerable compared to the empirically estimated heterogeneity distribution among psychological meta-analyses (van Erp et al., 2017). Most of the estimated heterogeneity is explained by the authors' model (Figure 6), suggesting that different authors employed a different set of experimental approaches undermining the capacity of explaining heterogeneity accurately.

Beyond the authors' model, the blinding paradigm and the WM paradigm are the most important moderators (Figure 6).

The blinding paradigm is a crucial component of visual consciousness studies, which can affect the available information and the level of stimulus processing. However, the paradigm choice is often not appropriately justified (Izatt et al., 2014). Here we show evidence that the type of blinding paradigm can affect the unconscious WM effect. CFS is associated with the lowest effect size, which is consistent with the claim that the CFS is a strong blinding technique compared to the others (but see Breitmeyer, 2015; Tsuchiya et al., 2006).

Another factor that emerged from our analysis is the effect of the WM paradigms (Figure 6c). In particular, the DET (Trübutschek et al., 2017; Trübutschek, Marti, & Dehaene, 2019; Trübutschek, Marti, Ueberschär, et al., 2019) is associated with the highest effect size. This may appear surprising when considering that the DET is a challenging task that requires participants to memorize a target and then choose the correct response from a continuous array

(Trübutschek et al., 2017; Trübutschek, Marti, & Dehaene, 2019; Trübutschek, Marti, Ueberschär, et al., 2019; see Zhang & Luck, 2008 for an example with colors) thus requiring a fine-grained response unlike classical dichotomous forced-choice paradigms. However, the DET's highest effect size is probably confounded by several factors. In Trübutschek and colleagues' experiments, the to-be-remembered stimulus was a simple white square, and the task-relevant information was the location. Using more complex stimuli and more challenging task-relevant information (e.g., stimulus identity) could reduce the estimated effect size as observed in Bergström & Eriksson (2015). For example, Song and Yao (2016) reported that complex stimuli like faces and houses are unconsciously processed but with lower accuracy compared to simple stimuli like Gabor patches. At the same time, in visual WM literature the stimulus complexity is a crucial factor that affects WM accuracy (e.g., Eng et al., 2005). We suggest combining the DET with different stimuli and task-relevant information in order to understand the real impact of the WM task.

The number of trials did not emerge as a relevant predictor for the effect size. At the same time, the statistical power is clearly determined by the number of available trials both considering the participant and the group level (Baker et al., 2020). Related to this, the number of analyzed trials in unconscious WM studies are generally low. This is due to a high proportion of discarded trials during the post-hoc selection of unseen trials. This is an often-neglected problem in the literature and we expected it would explain a large variance of the effect sizes across the unconscious WM papers, which we did not find. We suspect that the reason for this is a possible interaction of the number of rejected trials and the employed blinding paradigms. While CFS can achieve the highest number of available trials, this comes at the cost of an expected small effect size. In fact, if the suppression is very strong, then we expect a lower number of visible trials but also a reduction of neural activity (Tsuchiya et al., 2006). Such a small (if any) unconscious WM effect is consolidated by a huge number of available trials,

resulting in a smaller effect size with CFS. On the other hand, other weaker suppression techniques, such as backward metacontrast, pattern masking and attentional blink, require many trials to be rejected post hoc due to a high proportion of trials where participants consciously see the target. While this reduces the number of available trials for the analysis, it can boost the effect size as the suppression may be incomplete and the neural activity related to the unseen stimulus can be substantial. The problem of this approach is whether the invisibility established by these paradigms is reliable as pointed out by the others (Stein et al., 2021; Stein & Peelen, 2021). At the moment, there are no published unconscious WM papers that achieved highly reliable suppression as in CFS with as little post hoc trial selection and established a very strong unconscious WM task. A systematic and large-scale study (e.g., Stein & Peelen, 2021) is necessary for future research. While this may sound a difficult aim to achieve, our further analyses hint at various factors that can be optimized to establish a large effect size of unconscious WM with reliable invisibility.

Another important aspect is the degree of participants' involvement in the unconscious WM task. For example, Soto (2011), differently from Taglialatela-Scafati (2019), explicitly asked participants to maintain in memory the unseen target stimulus. Also Pan and colleagues (2014) performed another experiment (Exp. 3a; not included in the meta-analysis) where participants were not required to hold the stimulus in memory. They failed to replicate the same indirect RTs effect of Exp. 2, highlighting the importance of being actively engaged in the WM task (Soto & Silvanto, 2014). Crucially, the change-detection accuracy (tested only in the Exp. 2) was at-chance even asking participants to actively remember the unseen stimulus. In order to clarify this important point, we suggest manipulating instructions about being actively engaged in the unconscious WM task within the same experiment.

The active involvement in the WM task can also influence the role of participants' attention. In fact, several unconscious perception effects disappear when the attention is not

engaged in the task, regardless of the stimuli visibility (Cohen et al., 2012; Dehaene et al., 2006; Koch & Tsuchiya, 2007; Tsuchiya & Koch, 2016). We suggest directly assessing the engagement in the WM task and the attentional involvement manipulating task instructions and including a condition where the top-down attention is diverted. This can clarify if the unconscious WM effect requires explicit involvement in the task or can be explained by a passive storage of information regardless of the visibility.

There are other critical aspects related to the unconscious WM demonstration that we cannot directly address with the meta-analytical approach. The first aspect to consider is the consciousness operationalization. Most of the reported papers use a subjective approach (e.g., PAS) selecting trials based on participants' reports. For example, Peters and Lau (2015) criticize this approach proposing another method to demonstrate perception without consciousness using both perceptual and metacognitive judgments. Also, Stein and Peelen (2021) reported that the type and restrictiveness of consciousness measurement can strongly influence observed results. There is no gold standard to formalize consciousness from the behavioral point of view, especially comparing subjective vs objective measures (see Seth et al., 2008; Sandberg et al., 2010). We suggest that direct comparison of subjective and objective approaches for unconscious WM is necessary to examine the effects of the operational definition of consciousness. In addition, from a signal detection point of view, the response criterion is a crucial factor in consciousness studies. Reporting a perceptual experience without controlling or measuring the response criterion could overestimate the unconscious processing (i.e., conservative response criterion; Finkbeiner & Coltheart, 2014) because under limited visibility conditions there could be a tendency to report the absence of an experience. Future studies should employ and report signal detection measures of criterion or bias (Stanislaw & Todorov, 1999) and the relationship with task accuracy (e.g., Railo et al., 2021) in order to understand a possible moderator or confound of the unconscious effect. Notably, even a strict

signal-detection approach for consciousness assessment has been criticized (Lau, 2008; Persuh, 2017; Merikle et al. 2001). Another way to evaluate the authenticity of the consciousness manipulation has been proposed by Trübutschek and colleagues (2017; 2019) where the neural activity for unseen and seen trials was directly contrasted demonstrating the difference between unseen and seen trials despite the above-chance performance.

In addition to previous methodological criticisms, a WM specific theoretical concern has been raised by Stein and colleagues (2016) where the unconscious WM effect can be explained by the participant that guessed the response (e.g., target location) when the unconscious target is presented and then retain the conscious guess, without the need of an unconscious WM consolidation. This problem is hard to solve from a behavioral-only point of view because we need evidence that the unseen information is genuinely retained and not related to other conscious processes that nevertheless influence the WM performance. Trübutschek and colleagues (2017; 2019) tried to directly face the *conscious guess maintenance* criticisms applying multivariate pattern analysis (MVPA) during the delay period. The neural prediction of the conscious guess maintenance is that the conscious guess should be detectable during the delay period for unseen stimuli and be similar to the brain activity for seen stimuli. Authors did not find evidence for this pattern reporting that for unseen trials only during the test phase the target information was decodable.

Finally, a theoretical problem related to the previous point relates to the distinction between short-term memory and WM (see Cowan, 2008 for an overview). Some authors interpreted the results included in the meta-analysis as evidence of passive short-term retention and not as a genuine form of active information manipulation (i.e., working memory; Persuh et al., 2018; Trübutschek et al., 2019). In fact, only Trübutschek and colleagues (2019) used a paradigm where, together with short-term retention, also an active manipulation (i.e., mental rotation) was required. Despite finding above-chance accuracy during unconscious trials, the

mental rotation was associated with a neural pattern similar to conscious trials. This suggests a different neural mechanism between passive short-term retention and active information manipulation (Lewis-Peacock et al., 2012; Rose et al., 2016).

5 Limitations and future directions

As introduced in the first section, the concept of WM is broad and other important WM-related phenomena, such as implicit WM (Hassin et al., 2009), priming-like behavioral effects (Pan et al., 2012, 2014) or the impact of unconscious distractors on visible stimuli (e.g., Bona et al., 2013) are not discussed here. This is clearly a limit from the theoretical point of view, but we decided to select highly comparable studies in order to reduce the number of dimensions along which the effect could vary. Furthermore, considering the estimated heterogeneity and the effect size uncertainty, including a broader range of related phenomena could drastically undermine the meta-analytic approach. Despite the random-effect meta-analysis can handle a certain amount of heterogeneity, estimating an average effect in the presence of high heterogeneity without the ability to reliably explain observed variability is problematic. The meta-regression analysis is necessary in the presence of high heterogeneity but at the same time the unbalance between the number of studies within each moderator and the possibility of confounding effects clearly undermine the possibility to draw strong conclusions. At the same time, we highlighted relevant moderators that, if appropriately manipulated, will shed light on the unconscious WM effect.

For example, from the theoretical and methodological point of view, the consciousness assessment is critical and not always managed appropriately. In fact, most of the methodological criticisms of unconscious processing need to be solved in primary studies, limiting the conclusions that can be driven from our meta-analytical approach. In order to better demonstrate the WM effect, we suggest a more homogeneous approach for future research, contrasting relevant variables such as the consciousness manipulation, the task instructions

and the unconsciousness assessment. At the same time, we suggest a more standardized approach about WM paradigms and target category.

Beyond moderators directly tested in our meta-analysis, other variables can be relevant for the unconscious WM effect. For example, the retention interval duration is a crucial parameter in WM studies (see Zhang & Luck, 2009). However, in our selection of studies the retention interval is often manipulated within the same experiment using very different durations undermining the possibility to have a unique meaningful effect. An extensive table with all extracted variables can be found at the OSF repository as a useful tool for future research to have a complete overview of all relevant paradigms' features.

In relation to the short-term retention vs working memory debate, we suggest using tasks where the information manipulation is clearly involved to understand the limit of this unconscious memory mechanism (as done in Trübtschek et al., 2019).

The use of meta-analysis in consciousness research is not widespread. To our knowledge only few consciousness topics have been meta-analyzed. For example, Van den Bussche and colleagues (2009) assessed the masked priming effect and Hedger and colleagues (2016) compared the impact of the consciousness paradigm in unconscious processing of fear. We suggest that using this approach is particularly useful for consciousness research in order to extract relevant moderators and generate specific hypotheses directly comparing theories and methods (Melloni et al., 2021).

Author Contributions

F.G. developed the study concept. F.G and N.D.P. performed data gathering and analysis. M.P. provided critical revisions on data analysis. F.G. and P.S. drafted the manuscript. N.T. provided critical revisions. F.G., N.T., N.D.P. and P.S. interpreted the data. All authors approved the final version of the manuscript for submission.

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Declarations of interest

none

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Figure caption

No color figures for the printed version

Figure 1 PRISMA diagram of the literature selection process

Figure 2. The general structure of an unconscious WM paradigm. The encoding phase manipulates the visibility of the to-be-remembered item using a blinding paradigm. The figure depicts three methods: continuous flash suppression, backward masking and attentional blink. After a delay interval, the encoded information is probed. The consciousness assessment can be performed before or after the WM task. Some studies also performed a consciousness assessment before or after the main experimental session.

Figure 3: Forest plot of included papers. Squares, dots and segments represent the median effect size and 89% HPDI for each included paper (level 3). Gray dots are the observed effects of each study in each paper (level 2) where the size is the meta-analytic weight computed as the inverse of the effect size variance. Red (squares) and blue (dots) elements are respectively published and unpublished papers. The vertical dotted line is the average effect, and the gray horizontal segment is the 89% prediction interval.

Figure 4: Funnel plot and Egger regression. Each observed effect size is plotted against the respective standard error. Note the y axis goes from 0 to 0.5 from top to the bottom. The dashed line represents the regression line. Red squares and blue dots are published and unpublished studies, respectively.

Figure 5: The moderator analysis for first authors as predictor. 89% HPDI is indicated by the black vertical lines. The solid red line is the upper limit of the ROPE (0.1).

Figure 6: Moderator analyses for other 4 factors. A) Blinding paradigm (AB = Attentional Blink, BM = Backward Masking, CFS = Continuous Flash Suppression and MM = Metacontrast Masking), B) Explicit and Implicit instructions and the difference. Note that for the difference the ROPE approach is not relevant. C) WM paradigm (CDT = change-detection task, DD = Delayed Detection, DET = Delayed estimation task and DMS = delayed match-to-sample task) and D) Target duration. Other aspects are the same as Figure 5. Within Table 1 is possible to find information about which authors/research groups used a particular experimental design. *Paradigms that are used only by a specific group of authors. The DET and the MM are used only by Trübutschek and colleagues. The AB the DMS and the 3000ms target duration are only used by Bergström and Eriksson. The implicit WM task is used only by Tagliatela-Scafati.

Table caption

Table 1: Details of studies included in the meta-analysis. The Published column indicates the source of the effect size (published or unpublished). The Mean Trials is the average number of included trials per participant. WM paradigm is the paradigm used in the study. Blinding is the paradigm to manipulate the conscious experience. Acc Measure is the measure to evaluate task accuracy (accuracy or d'). Target is the category of the to-be-remembered stimulus. WM Task is the relevant information about the target for the task (identity, location or both). Hedges's g is the computed effect size and SE is the standard error.

Table 2: Meta-regression table. For each model parameter is reported the estimated value with the standard error and the 89% HPDI. For relevant parameters is also reported the percentage of HPDI within /outside the ROPE. For heterogeneity estimation the ROPE is not reported. The ROPE is not reported for the implicit vs explicit instructions model because it is considered completely exploratory.

Table 3: LOOIC and R^2 values of meta-regression models. The weight column is the probability that a given model is the best according to the LOOIC criterion.