

Cognitive Performance During Display Switching in Optical See-Through Augmented Reality

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ABSTRACT

Optical see-through (OST) augmented reality (AR) displays can render virtual content directly in the user's view of the physical world. In certain use cases, the physical world can include fixed emissive displays. For example, pilots, ship captains, and power plant operators equipped with AR displays might switch their visual attention between AR elements, physical interface components, and imagery on emissive flat panel displays fixed in the environment. Switching attention between these targets involves aspects of dynamic perception and cognition. We conducted a controlled study using XREAL One Pro AR glasses to examine the cognitive effects of switching attention between content displayed on a nearby flat panel display (FPD)/ screen and content displayed on a head-worn AR display (HWD), under two ambient brightness levels (10 lux and 150 lux). Participants completed an adapted Paced Visual Serial Addition Test (PVSAT), a continuous cognitive task that measures information processing speed and working memory under sustained demand, while switching between displays on every trial. Contrary to expectation, we found no general cost of switching over repeating a display. Instead, performance costs were specific to the direction of the transition: transitioning to the screen was significantly slower than transitioning to the HWD, no matter where the transition began. Higher ambient brightness improved response speed, reduced missed trials, and did not increase subjective visual fatigue. These findings suggest that in unpredictable multi-display environments, what display you transition to matters as much as the act of switching itself, with implications for the design of OST AR interfaces where transitions between head-worn and external displays are frequent.

Index Terms: Augmented Reality, Optical See-Through Display, Display Switching, Cognitive Performance, PVSAT, Visual Fatigue.

1 INTRODUCTION

Optical see-through (OST) augmented reality (AR) displays are increasingly used across domains such as manufacturing, healthcare, scientific visualization, and knowledge work [39]. These systems present virtual information while preserving an unobstructed view of the real world, requiring users to integrate and shift attention within and between different information sources [1, 2, 16]. Because OST AR overlays are additive rather than emissive, the visibility and legibility of virtual content depend strongly on condi-

tions in the surrounding environment—especially ambient brightness, which can diminish contrast and make virtual elements appear more transparent [10, 12, 24, 32]. As a result, OST AR interaction involves a complex interplay between perceptual factors and cognitive processes such as working memory, attention allocation, and information-processing speed [16, 25].

Despite the growing adoption of AR systems, comparatively little is known about how users manage cognitive workload when rapidly alternating between real-world displays and virtual overlays under varying environmental conditions. Prior work has studied task switching and cognitive load in AR, yet few studies have isolated the cost of switching specifically between OST AR and non-AR displays using a continuous, controlled performance task [1, 2, 16, 48, 49]. Fewer still have examined how these switching costs are affected by ambient lighting, even though brightness is known to alter the perceptual clarity of AR content and thereby influence user performance. This gap is consequential, as OST head-mounted displays (HMDs) are routinely used in environments that range from dim indoor offices to bright outdoor or industrial settings [10, 11]. Designers currently lack empirical guidance on how display modality and lighting interact to shape moment-to-moment cognitive efficiency and visual comfort.

In this paper, we present a controlled laboratory experiment investigating the cognitive cost of switching between a non-AR display and two OST AR viewing modes—monocular and binocular—under low (10 lux) and high (150 lux) ambient brightness conditions. Using the XREAL One Pro headset and a calibrated non-AR screen, we implemented an adapted Paced Visual Serial Addition Test (PVSAT) [15] to induce continuous information-processing demands while eliciting rapid, unpredictable switches within and between displays [15, 43]. Participants completed nine transition types: *repeat trials*, in which consecutive stimuli appeared on the same display, and *switch trials*, in which consecutive stimuli appeared on different displays, with the start and destination displays fully crossed across the three display conditions. This design allowed us to separately assess arrival-based and departure-based switching costs. After each brightness condition, participants also reported visual fatigue, allowing us to relate environmental lighting not only to performance but also to subjective comfort.

Guided by prior work on task switching and AR perceptual constraints, we address the following research questions (RQs):

- **RQ1:** How does cognitive performance and information-processing efficiency differ as users transition within and between display modalities, including non-AR, binocular, and monocular OST AR displays?
- **RQ2:** Are display transition costs symmetric, or do users incur different costs when arriving at versus departing from a given display?
- **RQ3:** How does ambient brightness influence cognitive performance and visual comfort between and across display

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modalities, and does lighting interact with AR presentation to amplify performance degradation?

Our findings reveal that display-switching costs are not uniform across transition types. Contrary to our initial expectation, participants did not respond faster or more accurately on repeat trials than on switch trials. Instead, performance costs were specific to the direction of the transition: participants were significantly slower when arriving at the screen than when arriving at either AR condition, regardless of the preceding display. With respect to ambient illuminance, higher brightness supported rather than hindered performance: participants responded faster and produced fewer missed trials under 150 lux than under 10 lux, most likely because the higher ambient light improved stimulus legibility for the direct-view PC display. Together, these results indicate that the perceptual and attentional properties of individual display modalities are stronger determinants of cognitive efficiency in OST AR than the switching event itself, with implications for the design of multi-display AR environments where transitions between head-worn and external displays are frequent and unpredictable. Together, these results illuminate how ambient lighting and display modality jointly influence cognitive efficiency in OST AR, providing insights for designing AR systems that mitigate switching burdens and remain effective across environments with varying ambient brightness.

The remainder of this paper is structured as follows. Section 2 provides background information and an overview of related work. Section 3 describes the experiment we conducted. The results are presented in Section 4 and discussed in Section 5. Section 6 concludes the paper.

2 RELATED WORK

2.1 Attention Switching between Physical and Virtual Content

Much of prior research in virtuality switching has been performed using visual search tasks [2, 7, 16, 28, 48]. Gabbard et al. looked at the effect of virtuality switching and focal distance switching on a visual search task using a monocular display [16]. They found significant effects of virtuality switching and focal distance switching on task performance, with participants finding their targets significantly slower. Arefin et al. continued this work, again using a visual search task on a custom-built AR Haploscope [1, 2]. They found that, especially at higher target depths, accuracy and response time decreased with virtuality switching. Yu et al. also leveraged a visual search task to see the impact of virtuality switching and distance on performance [48]. They found that participants took significantly longer on virtual-to-physical transitions than on all-virtual or all-physical transitions. Interestingly, in contrast with prior findings [16, 20], the all-virtual transitions were also performed faster than all-physical transitions.

In contrast, Lugtenberg et al. used a visual acuity task over a visual search task, where participants identified directional symbols while the displays varied the distances of vergence and accommodation (near and far) to exacerbate the vergence accommodation conflict (VAC) [28]. They found that minimizing VAC facilitated rapid shifts between real and virtual objects but did not significantly affect cognitive task load or accuracy. Imamov et al. also examined virtuality switching through a dual-task study in which the primary task was performed on a physical LCD display, and participants had to reference glanceable data on an OST display [21]. They found that participants performed better when the OST data was at a greater focal depth than the primary task, but worse when the OST data was farther from the primary task in angular distance. More recent work by Zhang et al. [49] examined multitasking between a physical monitor and virtual OST-HMD displays, varying both the spatial discontinuity (depth and lateral separation) between the displays and the level of task load. They found that the use of

AR, the spatial discontinuity between displays, and task load each affected multitasking performance and perceived workload, further suggesting that switching costs extend beyond simple visual search into continuous multitasking contexts.

Taking prior research into account, we kept the depth of the physical and virtual targets constant [2, 7, 9, 16]. In addition, instead of using a visual search task, we intend to examine how the physicality of the displayed data affects performance in a continuous cognitive task sensitive to processing speed and working memory [15]. Furthermore, we examine how the ambient brightness of the physical display can affect virtuality switching, which, to the best of our knowledge, prior work has not investigated.

2.2 Cognitive Load using Augmented Reality

Often in AR study designs, cognitive load is measured through self-assessment such as the NASA-TLX [18] or Paas scale [36]. These studies often have participants perform a task then fill out their perceived cognitive performance through a questionnaire [3, 5, 34]. For example, Baumeister et al. compared OST-HMDs, visual see-through HMDs, and spatial augmented reality using the Paas cognitive load questionnaire and found that cognitive load increases with a lower field-of-view [3].

Other studies use a cognitive load task in order to measure their conditions' impact on cognition. Visual search tasks, such as those used by Gabbard et al. and Yu et al. is one such example [17, 48]. Another cognitive task is the stroop task, where participants state the color of text that itself denotes a color (e.g., green text that says "red") [29]. Cometti et al. used a stroop task and a memory task to measure how wearing a hololens can impact cognitive load [6].

Cognitive load measurements can also be used to evaluate differences in design. For example, Hou et al. developed a VR simulation testbed where they had a high, medium, and low cognitive load scenario. Using NASA-TLX, they discovered a significant difference in task completion time, anxiety, frustration, and task performance between the high and low cognitive load scenarios. Using multi-modal sensor data, they then created a detection algorithm with a 95% accuracy for high cognitive load conditions, including a heart sensor to detect high heart rate [19].

Dual-task scenarios are another common tool for measuring cognitive load, where performance on a baseline task is impacted by performing another task. Nenna et al. used a dual-task scenario where they had participants perform a visual task (static, cognitive), a navigation task (active, motor-based), and both simultaneously while wearing an OST-HMD [34]. They found that performance on the visual task was significantly worse during the navigation task. Interestingly, the self-reported cognitive load for their study found no significant difference between the visual single-task as compared to the dual task despite the significant performance difference. Lu et al. also used a dual-task scenario pairing a cognitive information retrieval with a motor navigation task to evaluate glanceable interfaces [27].

From these various methods, we chose to use a cognitive load task with our study to quantitatively measure the differences in performance.

2.3 Paced Visual Serial Addition Test

The Paced Visual Serial Addition Test (PVSAT) (and its predecessor, the Paced Auditory Serial Addition Test (PASAT)) is a continuous, controlled, cognitively demanding task that is sensitive to processing speed degradation and attentional fatigue [15, 43]. It has been used mainly in the medical field to measure cognitive performance in individuals with traumatic brain injury, multiple sclerosis, depression, and irregular sleep patterns [23, 42–44]. Further research has shown it can be used to induce fatigue as a stressor [44]. Recently, it has been used to measure cognitive processing speed and working memory during tasks using an OST-HMD in

an automotive passenger scenario [38]. In particular, Sasalovici et al. used the test to assess how motion sickness affected cognitive processing, but found no significant differences across conditions.

The PVSAT involves a pipeline in which the user must perceive and comprehend the number, perform a simple arithmetic operation, report the number, and update working memory [15]. This has been shown to particularly tax working memory and its associated brain areas [14]. It is considered an easy process for adults to perform rapidly, while inducing physical arousal (heart rate and blood pressure), indicating the cognitive effort required [22, 31].

We use this task for our cognitive load performance measure because it generates numerous rapid trials that quickly cycle through the three display conditions and all source $\rightarrow$ destination pairs. Furthermore, working memory is useful in many AR scenarios such as quick glanceable information retrieval, sensemaking, and office work settings [26, 27, 37]. In all, this task provides a measure of cognitive load and working memory that effectively detects small changes in performance.

3 EXPERIMENT

This section describes the experiment we conducted. The experiment was approved by the Institutional Review Board (IRB) of our university.

3.1 Conditions

We used a within subjects design with three display devices – binocular AR (AR_{bin}), monocular AR (AR_{mono}), and a screen ($non-AR$) – and two background illuminance settings – 10lux and 150lux . To investigate the effects of multiple displays on users’ cognitive performance, with three devices, each condition could either be repeat trials (start device and destination device is the same) or switch trials (start device and destination device are different). This yielded in total nine conditions (refer Table 1).

Table 1: Three repeat trials and six switch trials yield total of 9 unique conditions.

Start Device	Destination Device	Transition Type
AR_{mono}	AR_{mono}	Repeat
AR_{bin}	AR_{bin}	Repeat
$non-AR$	$non-AR$	Repeat
AR_{mono}	AR_{bin}	Switch
AR_{mono}	$non-AR$	Switch
AR_{bin}	AR_{mono}	Switch
AR_{bin}	$non-AR$	Switch
$non-AR$	AR_{mono}	Switch
$non-AR$	AR_{bin}	Switch

3.2 Participants

We recruited 20 participants for our experiment (14 male, 5 female, 1 non-binary; ages 18 to 44 $M=28.7$, $SD=8.28$). The participants were members of the local university community. All of the participants had normal or corrected-to-normal vision. We verified 20/20 vision for all participants before the experiment using a standardized Snellen chart test [13]. Additionally, we assessed eye dominance using the Porta test [30] and evaluated stereoscopic vision capability using the pencil depth test [35]. None of the participants reported known visual or vestibular disorders, such as color or night blindness, dyschromatopsia, or a displacement of balance. All had prior experience with AR.

3.3 Experimental Setup

Our experimental setup employed a dual-display configuration consisting of a large non-AR screen and an optical see-through AR

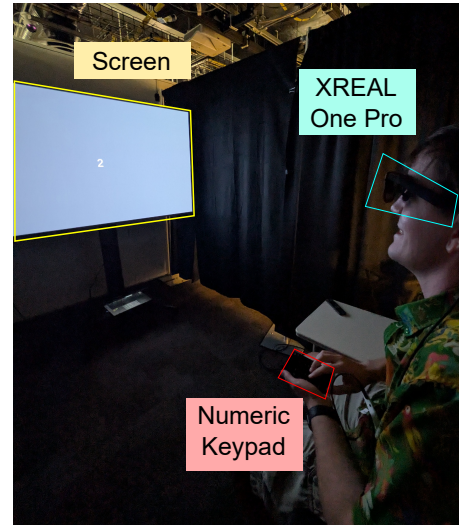


Figure 1: A participant wearing the XREAL One Pro AR glasses and holding a numpad keyboard. They are seated at a viewing distance of 2 m from the screen. The setup illustrates the background used to simulate two background illuminance conditions (10lux , 150lux).

headset. Participants were seated 2 m from the display in an enclosed room (dimensions: $7m \times 3.5m \times 10m$) outfitted with matte curtains to minimize reflections; see Figure 1. This environment ensured consistent lighting conditions across all trials.

For the non-AR presentation, we used an LG OLED83G5WUA 4K television. The display spans 83 in diagonally and provides a 3840×2160 (4K Ultra HD) resolution panel. For AR presentation, stimuli were shown using the XREAL One Pro optical see-through headset connected to the XREAL Beam phone¹. The headset delivers a resolution of 1920×1080 per eye at 120 Hz with a diagonal visible field of view of approximately 57° .

We characterized the background illuminance under two lighting conditions by measuring the screen luminance at a distance of 2 m using a Konica Minolta CS-100 Chroma Meter. The TV brightness was manipulated to achieve 10 lux in the low-illuminance condition and 150 lux in the high-illuminance condition. The AR headset was operated at its lowest brightness setting for all study conditions.

Stimuli consisted of white numeric characters presented against a mid-grey background ($\#7F7F7F$). All text was rendered in a fixed-width font subtending a visual angle of approximately 2.5° , following the specifications of Fos et al. [15]. The experiment was implemented in Unity 6.1, with stimulus presentation and response logging verified to within $\pm 1\text{ms}$ timing accuracy. Synchronization between the host PC and the XREAL AR glasses was achieved via Photon Unity Networking (PUN2), ensuring that both displays received identical timing signals within a single game loop tick. Responses were registered via a numeric keypad (Bluetooth connected) positioned at the participant’s dominant hand.

While our study employed the XREAL One Pro, the hardware characteristic central to our investigation is the *additive light model* shared by optical see-through displays: virtual content is rendered additively over the real world, making ambient brightness a direct modulator of virtual content visibility. This property is common across commercial additive-model OST HMDs, including the Microsoft HoloLens 2 and Magic Leap 2. We selected the XREAL One Pro because it is a commercially available, lightweight, and ergonomic device employing birdbath optics—a form factor of growing relevance for consumer AR. We therefore scope our findings to

¹ <https://www.xreal.com/beampro>

additive-model OST displays; conclusions are expected to generalize most directly to headsets sharing this optical architecture, while devices with fundamentally different light models (e.g., video see-through) fall outside this scope.

Prior to the study, we characterized the focal depth of the headset using a physical–digital alignment protocol with fine text targets, confirming the headset’s fixed focal plane at 2.0 m. Stimuli on both displays were therefore aligned to this distance, matching the 2 m viewing distance of the screen and holding accommodative demand constant across display conditions.

3.3.1 Stimuli Normalization

To ensure that participant performance was not confounded by differences in visual stimuli, the presentation was normalized between the XREAL One Pro and the 83-inch television. The primary objective was to maintain parity in both *Pixels Per Degree (PPD)* and *Visual Angle* across both hardware conditions.

We calculated the display density of the XREAL One Pro as 42 PPD based on the field-of-view and resolution². For a target stimulus height subtending a visual angle of 2.5° , the required vertical resolution was determined to be 105 pixels. To match this 42 PPD density on the physical display at a fixed viewing distance of 2 meters, the software resolution of the television was adjusted. Given the TV’s vertical field of view of approximately 29.7° at this distance, a custom resolution of 2218×1248 was applied. This configuration ensures that one degree of the user’s vision corresponds to exactly 42 pixels on both devices, maintaining consistent angular resolution.

Retinal size was verified through physical measurements to ensure the stimulus on the TV subtended the correct angle. For a 2.5° angle at a 2-meter viewing distance, a physical height of 8.73 cm was confirmed. By setting the Windows display scale to 100% and configuring the Unity Canvas to a *Constant Pixel Size* setting, 1:1 parity in retinal projection and pixel density was achieved across all experimental conditions.

3.4 Procedure

Upon arrival, participants read through an information sheet and provided verbal consent to participate in the experiment. The experimenter then described the task protocol and introduced the three display devices used throughout the study. Participants completed a 20-trial practice block on a computer (non-AR) to build familiarity with the task and stabilize baseline response times. On average, participants repeated the practice block three times. Once they reported sufficient comfort and understanding, they advanced to the experimental trials.

The main task consisted of two blocks corresponding to two environmental brightness levels: 10 lux and 150 lux. Brightness block order was counterbalanced across participants using a Latin square design to control for learning effects. In each block, participants performed the Adapted PVSAT (see Section 3.5 and Figure 2). Participants wore the AR glasses continuously throughout each block, including on non-AR trials; no physical donning or removal occurred during the task. When the active display changed between trials, stimuli were routed entirely in software—to the screen (non-AR), to the participant’s dominant eye only (AR_{mono}), or to both eyes (AR_{bin})—so that switching required only a redirection of gaze and attention, not a change of equipment. Following each block, participants removed the HMD and the experimenter administered a self-timed rest break on average 5 minutes to reduce fatigue effects. During the break, participants completed the Convergence Insufficiency Symptom Survey (CISS) [4] to measure subjective fatigue. This process was repeated for both brightness conditions.

²<https://vr-compare.com/headset/xrealonepro>

Upon completing all conditions, participants filled out a demographics questionnaire. Participants were then debriefed and compensated for their time. The experiment took approximately 45 minutes in total.

3.5 Cognitive Performance Task: Adapted Visual PSAT

To assess cognitive overhead induced by cross-device display switching, we administered an adapted version of the Paced Visual Serial Addition Test (PVSAT) [15]. The PVSAT is intentionally abstract, following controlled cognitive paradigms (e.g., Stroop, visual search) that isolate attentional mechanisms under sustained demand. It abstracts the rapid, unpredictable attentional shifts common to monitoring, glanceable AR, and multi-display work (e.g., surgical dashboards, industrial AR overlays). Real-world workflows would confound display switching with task complexity and expertise; our controlled paradigm trades ecological validity for causal attribution of switching costs, as a first step toward more ecologically grounded studies.

3.5.1 Task Structure

On each trial a single digit (0–4) was presented centrally on the active display for 450 ms, followed by a fixed interval of 2000 ms. Restricting the digit range to 0–4 bounds all valid sums to the range 0–8, ensuring that every correct response requires a single keypress and eliminating the confound of variable motor execution time associated with two-digit sums. Participants were instructed to add each incoming digit to the *immediately preceding* digit (last-two rule) and to enter their response on a physical numeric keypad at their dominant hand as rapidly and accurately as possible. No running total was required; each response was independent of prior trials, limiting error accumulation and allowing performance differences to be attributed to attentional or processing mechanisms rather than mnemonic load. Response time was recorded from stimulus onset to keypress. Trials on which no response was registered before the onset of the subsequent stimulus were logged separately as *missed trials*. At least two practice blocks of 20 trials each preceded the main task to stabilize baseline RT and familiarize participants with the response procedure.

3.5.2 Device-Switching Protocol

Three display devices were used throughout the experiment: non-AR, AR_{bin} , and AR_{mon} restricted to the participant’s dominant eye. Within each experimental block, the active display was assigned pseudo-randomly on a trial-by-trial basis, subject to fixed condition counts: 20 trials per switch type and 10 trials per repeat type, yielding 150 scored trials per block (excluding the first trial of each block, which provides no valid prior digit). This trial-level randomized interleaving prevents strategic adaptation to any single device and mirrors the unpredictability of real-world multi-device use[33]. At a total trial duration of 2450 ms (450 ms stimulus + 2000 ms interval), each block lasted approximately seven minutes. Each participant completed two blocks, counterbalanced across high and low ambient brightness conditions.

3.5.3 Dependent Measures

- **Response Time:** Mean RT per transition condition per participant, computed over trials on which a valid response was registered, measured from stimulus onset to key-press.
- **Accuracy:** Proportion of correct responses per condition, computed over all scoreable trials including missed trials, which were treated as incorrect.
- **Error Types:** To decompose overall error rate into cognitively distinct components, we distinguished two error types—

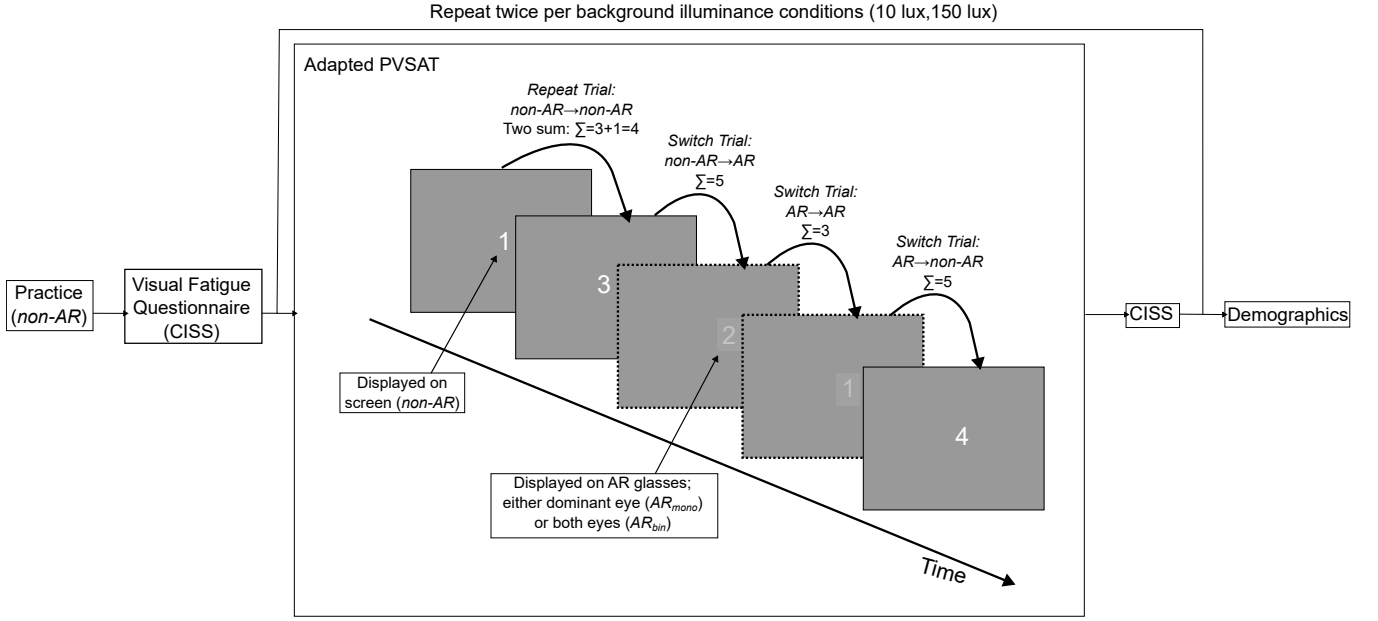


Figure 2: Experimental procedure. Participants completed an adapted PVSAT in which digits (0–4) were presented for 450 ms followed by a 2000 ms response window. On each trial the active display was either a screen (*non-AR*), the AR glasses restricted to the dominant eye (AR_{mono}), or both eyes (AR_{bin}), yielding six switch and three repeat transition types. Each block comprised 150 scored trials (≈ 7 min). Participants completed two blocks under high (150 lux) and low (10 lux) ambient illuminance, with block order counterbalanced across participants. The CISS questionnaire was administered before and after each block.

- *Missed trials*: defined as trials on which no keypress was recorded before the next number is presented, are interpreted as a failure of sustained attention or cognitive capacity. The user failed to produce a response within the allotted time.
- *Wrong responses*: defined as trials on which a keypress was registered but the submitted answer was incorrect, reflect a failure of mental arithmetic or working memory retrieval rather than a loss of attentional engagement.
- **Visual Fatigue**: We used the revised CISS [4]. This survey consists of 14 questions, assessing how one’s eyes feel when reading or doing close work, covering symptoms of visual fatigue.

3.6 Hypotheses

Drawing on our motivation and research questions outlined in Section 1, together with the related work reviewed in Section 2, we developed the following hypotheses:

- H1**: Repeat trials will yield faster response times and higher accuracy than switch trials.
- H2**: Switching costs will be asymmetric: the cost of arriving at a target display will differ from the cost of departing the same display.
- H3**: Response time and accuracy will be worse under high ambient brightness than low ambient brightness.
- H4**: Subjective visual fatigue will be higher following the high brightness condition than the low brightness condition.

4 RESULTS

In this section, we present the descriptive statistics and inferential statistical tests for the hypotheses outlined above. Given the repeated-measures structure of the data and the presence of missing observations due to unanswered trials, we analyzed the data using linear mixed-effects models (LMEMs) for continuous outcomes (response time) and generalized linear mixed-effects models (GLMEMs) for binary outcomes (accuracy). Participant were included as a random intercept to account for individual differences in baseline performance.

The significance of fixed effects was assessed via F -tests with Kenward-Roger degrees of freedom approximation for LMEMs, and likelihood ratio tests for GLMEMs, at the 5% significance level. Pairwise comparisons were conducted using estimated marginal means with Bonferroni correction [45]. All analyses and figures were produced using R version 4.4.0.

Table 2: Significant pairs for response time (in ms).

Contrast (Slower > Faster)	Δ ms	t	p
$AR_{mono} \rightarrow non-AR > non-AR \rightarrow AR_{bin}$	136	6.58	< .0001
$AR_{bin} \rightarrow non-AR > non-AR \rightarrow AR_{bin}$	113	5.43	< .0001
$AR_{mono} \rightarrow non-AR > non-AR \rightarrow AR_{mono}$	147	7.20	< .0001
$AR_{bin} \rightarrow non-AR > non-AR \rightarrow AR_{mono}$	124	6.03	< .0001
$AR_{mono} \rightarrow non-AR > AR_{mono} \rightarrow AR_{bin}$	131	6.35	< .0001
$AR_{mono} \rightarrow non-AR > AR_{mono} \rightarrow AR_{mono}$	106	4.12	.0014
$AR_{mono} \rightarrow non-AR > AR_{bin} \rightarrow AR_{bin}$	113	4.45	.0003
$AR_{mono} \rightarrow non-AR > AR_{bin} \rightarrow AR_{mono}$	104	5.06	< .0001
$AR_{bin} \rightarrow non-AR > AR_{mono} \rightarrow AR_{bin}$	108	5.20	< .0001
$AR_{bin} \rightarrow non-AR > AR_{mono} \rightarrow AR_{mono}$	82	3.21	.0489
$AR_{bin} \rightarrow non-AR > AR_{bin} \rightarrow AR_{bin}$	89	3.52	.0159
$AR_{bin} \rightarrow non-AR > AR_{bin} \rightarrow AR_{mono}$	81	3.91	.0033

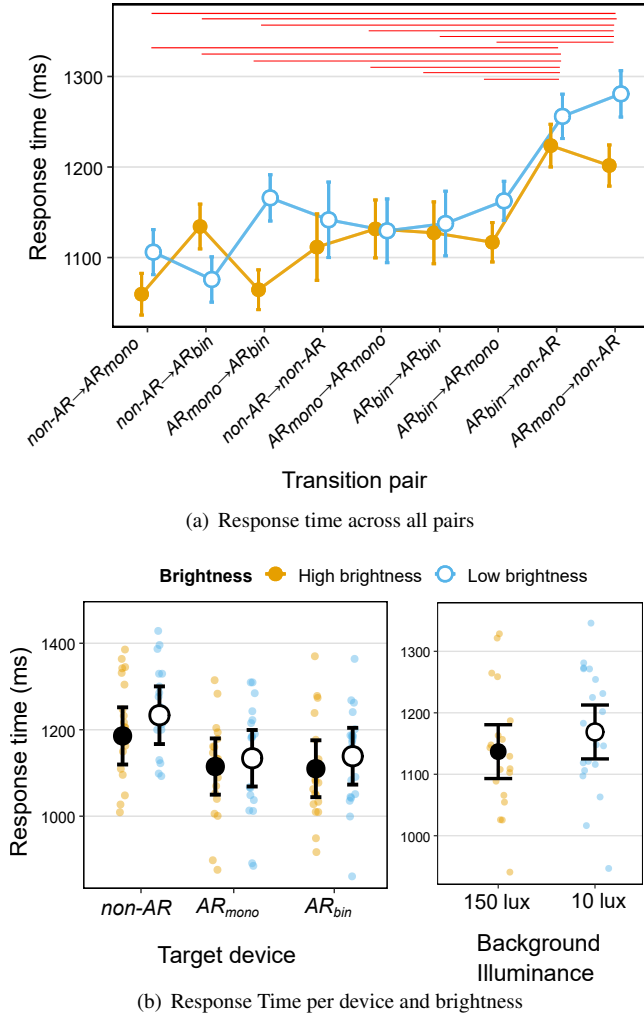


Figure 3: Results for response time in ms across (a) all transition pairs, and (b) per device and illuminance conditions. For (a) the x-axes indicate the nine transition pairs (three repeat pairs and six switch pairs), while for (b) it indicates the three target devices (*non-AR*, *AR_{mono}*, *AR_{bin}*) and two illuminance conditions (10 lux, 150 lux). Horizontal bars indicate statistical significance ($p < 0.05$).

4.1 Response Time

We first examined whether transitions between devices affected response time by performing a Type III ANOVA with transition pair and brightness as fixed effects (including their interaction), and trial number as a covariate. The analysis revealed a significant main effect of transition pair on response time, $F(8, 4480.7) = 11.87$, $p < .001$, indicating that the specific device-to-device transition substantially influenced how quickly participants responded. There was also a significant main effect of brightness, $F(1, 4481.1) = 6.18$, $p = .013$, indicating that higher display luminance led to faster response times, refer Figure 3. Trial number did not significantly predict response time, $F(1, 4481.0) = 0.13$, $p = .721$, indicating no systematic learning or fatigue trend across trials.

The interaction between transition pair and brightness was not significant, $F(8, 4480.6) = 1.93$, $p = .051$. Main effects are therefore reported collapsed across brightness. Pairwise comparisons with Bonferroni correction revealed several significant differences centered on *non-AR* arriving transitions (see Table 2 and Figure 3).

Collapsing across transition direction, we next examined

whether response times differed as a function of the *target device* (see Figure 3). Estimated marginal means showed that trials ending on the *non-AR* were markedly slower (1236 ms) than those ending on either *AR_{bin}* (1128 ms) or *AR_{mono}* (1136 ms). Pairwise comparisons confirmed this pattern: responses were significantly slower when arriving at the *non-AR* compared to arriving at *AR_{bin}* ($\Delta = 107$ ms, $t(4494) = 8.15$, $p < .0001$) and compared to arriving at *AR_{mono}* ($\Delta = 99$ ms, $t(4493) = 7.59$, $p < .0001$). In contrast, *AR_{bin}* and *AR_{mono}* did not differ from each other ($\Delta = 8$ ms, $p \approx 1.000$). Overall, these results indicate a clear device-level asymmetry: *arriving at the non-AR incurs longer response times than arriving at either AR headset, independent of where the transition originated*.

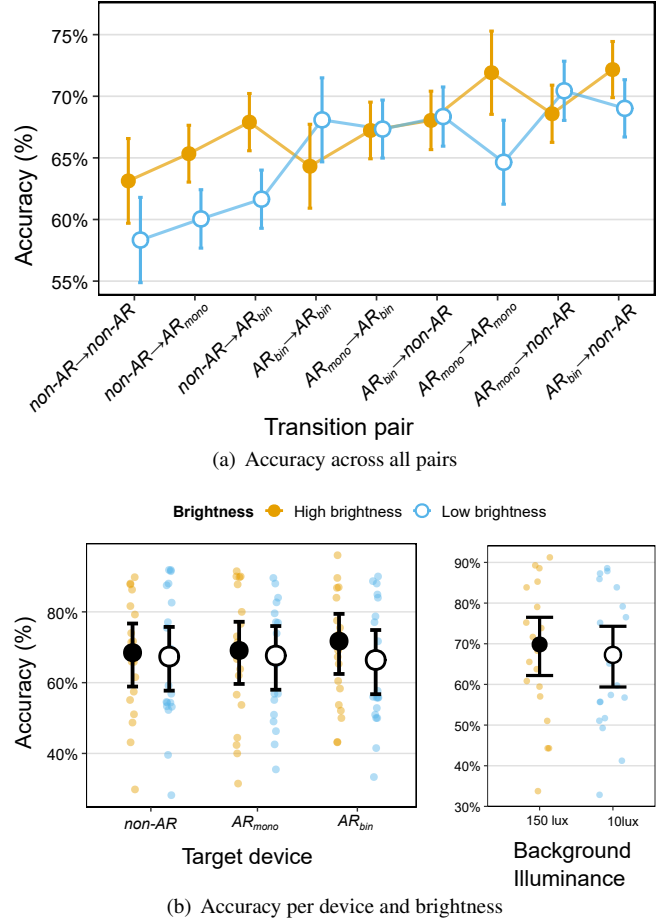


Figure 4: Results for accuracy in % across (a) all transition pairs, and (b) per device and illuminance conditions. For (a) the x-axes indicate the nine transition pairs (three repeat pairs and six switch pairs), while for (b) it indicates the three target devices (*non-AR*, *AR_{mono}*, *AR_{bin}*) and two illuminance conditions (10 lux, 150 lux).

4.2 Accuracy

To assess whether device transitions influenced trial-level accuracy, we fitted a logistic mixed-effects model with transition pair, brightness, and their interaction as fixed effects, and a random intercept for participant; trial number was included as a covariate, and missed trials were coded as incorrect. A likelihood-ratio test comparing this model to a null model (containing brightness, trial number, and participant) revealed a significant main effect of transition pair, $\chi^2(8) = 19.55$, $p = .021$ (see Figure 4). Although statistically reliable, the magnitude of this effect was small: the model's marginal

$R^2 = .007$ indicated that fixed effects explained less than 1% of the variance in accuracy.

Neither brightness nor trial number showed a significant effect on accuracy. Brightness had a small, nonsignificant coefficient ($\beta = -0.25$, $SE = 0.22$, $z = -1.13$, $p = .260$), indicating that changes in display luminance did not meaningfully alter accuracy. Trial number was also nonsignificant ($\beta = -0.02$, $SE = 0.03$, $z = -0.75$, $p = .454$), providing no evidence of learning or fatigue effects across the session. The interaction between transition pair and brightness was not significant either, $\chi^2(8) = 7.01$, $p = .536$. These results indicate that the influence of transition pair on accuracy did not depend on brightness level.

Given the absence of a transition-by-brightness interaction, we report main effects collapsed across brightness (see Figure 4). Bonferroni-corrected pairwise comparisons revealed *no statistically significant differences* between any transitions (all p 's $\geq .054$, with most p 's ≈ 1.000). The corresponding log-odds ratios were uniformly small, consistent with the marginal R^2 and indicating negligible practical differences in accuracy across transition conditions.

To mirror the response-time analysis, we also examined accuracy as a function of the *target device* by collapsing across transition direction (Figure 4). Bonferroni-corrected pairwise comparisons revealed no significant differences between any device pairs (non-AR vs. AR_{mono} : $z = 0.284$, $p \approx 1.000$; non-AR vs. AR_{bin} : $z = 0.948$, $p \approx 1.000$; AR_{mono} vs. AR_{bin} : $z = 1.398$, $p \approx 1.000$). The corresponding log-odds ratios were all close to zero, indicating negligible effect sizes. Together, these results show that accuracy did not vary as a function of the device participants were transitioning to.

Overall, accuracy remained high across conditions (unlike response time) did not exhibit a clear device-level asymmetry. *Accuracy did not reliably differ as a function of transition pair, brightness, or target device.*

4.3 Errors

We were focused on two error types – missed trials and wrong responses.

Missed trials: A Type III ANOVA revealed a significant main effect of brightness on missed trial rates, $\chi^2(1) = 4.97$, $p = .026$, with a large effect size ($\eta_p^2 = .68$). Participants showed higher missed trial rates under low brightness conditions ($M = .18$, $SE = .04$) compared to high brightness ($M = .09$, $SE = .03$), indicating that stimulus visibility substantially reduced attentional lapses. In contrast, transition type did not significantly predict missed trials, $\chi^2(1) = 1.77$, $p = .184$, $\eta_p^2 = .43$, suggesting that the cognitive demands of different transition types did not independently increase task disengagement.

The interaction between transition type and brightness was not significant, $\chi^2(1) = 2.38$, $p = .123$, $\eta_p^2 = .50$, indicating that the protective effect of high brightness was consistent across transition difficulties. These results suggest that missed trials reflect attention-driven failures related to stimulus visibility rather than working memory or arithmetic complexity.

Wrong responses: A Type III ANOVA revealed *no statistically significant effects* on the wrong response rate. Transition type did not significantly predict wrong response rates, $\chi^2(1) = 1.19$, $p = .139$, $\eta_p^2 = .89$. Brightness similarly showed no significant effect, $\chi^2(1) = 0.05$, $p = .821$, $\eta_p^2 = .16$. The interaction between transition type and brightness was also non-significant, $\chi^2(1) = 0.27$, $p = .604$, $\eta_p^2 = .50$.

These null findings indicate that once participants engaged with the task (i.e., attempted a response), the accuracy of their mental arithmetic was not meaningfully influenced by either the transition context or display brightness. Unlike missed trials, which showed

strong sensitivity to visibility, wrong responses appear to reflect task-intrinsic difficulty rather than stimulus or contextual factors.

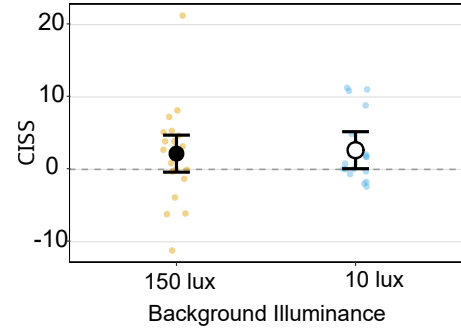


Figure 5: Results for CISS as a function of brightness. The x-axes indicate the two brightness levels — 10 lux and 150 lux.

4.4 Visual Fatigue

The repeated-measures ANOVA examining changes in visual fatigue (CISS) revealed no significant main effect of background illuminance, $F(1, 19) = 0.11$, $p = 0.739$, $\eta_p^2 = 0.006$. This indicates that performing the PVSAT task across 10 lux and 150 lux did not visually fatigue participants significantly (refer Figure 5).

5 DISCUSSION

This study examined the cognitive cost of switching between non-AR and AR displays during a continuous cognitive task. We adapted the PVSAT to create a protocol where participants could not predict which display would show the next digit, forcing rapid and unprepared attention shifts across devices on every trial. This distinguishes our work from prior studies of multi-display interaction, which have largely relied on visual search tasks [2, 17, 48]. The PVSAT places concurrent demands on visual perception, working memory updating, and arithmetic processing, meaning that a display switch can disrupt performance at any stage of the response pipeline – not just the perceptual stage. Response time and accuracy together index how well participants maintained this pipeline throughout the transitions.

5.1 No General Switch Cost

We predicted that *switch* trials would produce slower and less accurate responses than *repeat* trials (H1), based on the established notion that task switches incur a reconfiguration cost [33]. This effect was not observed. We attribute its absence to the unpredictable nature of the protocol: because participants did not know which display would be active on the next trial, they could not build up a task-set specific to the current device and therefore had nothing to reconfigure on a switch. In standard task-switching paradigms the repeat benefit arises precisely because participants can prepare for a predictable repetition [8]. When the next display is unknown, that preparation advantage disappears and repeat trials offer no benefit over switch trials. This finding has a practical implication: in real-world multi-display environments where transitions are unpredictable, the repeat vs switch distinction may be less meaningful than the identity of the target display itself.

5.2 Direction of the Transition Matters

Although there was no general switch cost, we found that the direction of the transition significantly affected performance (H2). Transitions arriving at the real-world screen were associated with significantly slower response times than transitions arriving at either AR condition, regardless of which device the participant had

just come from. The reverse was not observed: leaving the real-world screen did not produce faster responses. This asymmetry indicates that the bottleneck is specific to arriving at the external display, not to departing from it.

We propose two complementary explanations for why screen arrival is particularly costly.

5.2.1 Attentional Capture by the Head-Worn Display

Work on AR headsets and HUD-style displays has shown that head-worn imagery can capture attention in a way that reduces awareness of content outside the AR display [46]. Research from aviation has demonstrated that while HUDs improve performance for the tasks they directly support, pilots become slower to detect unexpected events in the external world — such as runway obstructions — because the brain treats the overlaid display as the primary signal and the real world as background [47]. A similar dynamic may be operating in our paradigm, participants may be treating the AR content as the primary signal. On trials where the digit appeared on the screen instead, this attentional bias had to be overcome, introducing additional latency before processing could begin. This account predicts that participants would show faster saccade landing times on AR-arriving trials than screen-arriving trials. Future work could test directly using eye tracking metrics such as saccade latency and fixation duration.

5.2.2 Optical Recalibration Between Display Modalities

The two display modalities differ fundamentally in their optical properties. The physical screen is a direct-view flat panel: light travels from the screen surface to the eye without intermediate optics, providing a stable and predictable visual input. The XREAL One Pro headset uses near-eye displays with Birdbath optics, which are popular with lightweight AR glasses: Light from a micro-display is reflected off a semi-reflective mirror (shaped like a bird bath) toward the eye while letting external light pass through, which can introduce differences in chromatic aberration and visual field uniformity compared to a direct-view display. This experimental setup is similar to those seen in Gabbard et al.'s and Arefin et al.'s work [2, 16]. Switching from the AR glasses to the screen may therefore require a brief period of oculomotor recalibration as the visual system adjusts to the direct-view optical environment. The specific contribution of individual optical properties — including focal distance, field uniformity, and chromatic properties — to this recalibration cost could not be isolated from the present data, and controlled measurement of these properties in future work would help disambiguate oculomotor from attentional accounts of the observed asymmetry.

5.2.3 Stimulus Presentation Ratio

Because two of the three display conditions were AR-based, stimuli appeared on the AR glasses twice as often as on the screen. This presentation ratio is analogous to the oddball paradigm, in which infrequent stimuli embedded in a stream of frequent ones demand additional processing because they violate the expectation built up by the dominant stimulus [40]. Under this account, the slower responses on screen-arriving trials may partly reflect the relative rarity of screen presentations rather than properties of the display itself. This offers a complementary explanation for the observed asymmetry that our design cannot disentangle from the attentional and optical accounts above; future work balancing presentation frequency across displays (e.g., equal per-display trial counts) is needed to separate these mechanisms.

5.3 Effect of Background Illuminance

Participants responded significantly faster and produced fewer missed trials under the high illuminance condition (150 lux) compared to low illuminance (10 lux) (H3). No significant difference

in accuracy was observed, and subjective visual fatigue scores did not increase meaningfully after either block (H4). Together these results suggest that for our specific display setup, the high illuminance condition was supportive rather than fatiguing. The most likely explanation is straightforward: the stimuli — white digits on a mid-grey background (#7F7F7F) — were more legible under higher ambient light. At 10 lux the low ambient luminance reduced the effective contrast available to the visual system, slowing digit identification and increasing the number of trials where participants failed to produce a response within the 2000 ms window. At 150 lux the improved viewing conditions reduced this perceptual constraint, freeing processing capacity for the arithmetic and response stages. Furthermore, these brightness values are well within normal indoor lighting conditions where a maximum of 1000 lux is expected [41].

Notably, the performance benefit of higher illuminance emerged in the absence of any difference in subjective visual fatigue between conditions. This dissociation is theoretically meaningful: brightness-related performance costs were observed independent of participants' subjective experience, suggesting that ambient brightness is an important design variable whose effects are not fully captured by fatigue-based self-report measures alone. Designers evaluating OST AR deployments should therefore complement subjective comfort assessments with objective performance measures under representative lighting conditions.

Our findings apply most directly to settings that share the structure of our paradigm: frequent, externally driven shifts between head-worn overlays and fixed emissive displays showing brief symbolic content, as in monitoring, supervisory control, and glanceable AR. In predictable workflows, where operators can prepare upcoming transitions, repeat/switch effects absent here may re-emerge, and content requiring extended reading or depth changes may alter the magnitude of the arrival asymmetry. Within the scope of additive-model OST displays under indoor lighting (Section 3.3), our results identify the factors that matter—the identity of the arrival display and the ambient legibility of emissive content—as hypotheses for ecologically grounded follow-up work.

6 CONCLUSION

In this paper, we presented a controlled study investigating the cognitive costs of switching between a physical screen and OST AR displays during a continuous cognitive task, and how ambient brightness modulates these costs. We measured information processing speed and accuracy using an adapted Paced Visual Serial Addition Test (PVSAT) with XREAL One Pro AR glasses under two ambient brightness levels (10 lux and 150 lux), across nine display transition conditions comprising six switch and three repeat pairs. Our results showed that switching costs were asymmetric and direction-dependent: arrivals at the screen were significantly slower than arrivals at either AR condition, while no general cost of switching over repeating a display was observed. Higher ambient brightness supported cognitive performance by improving stimulus legibility, and did not increase subjective visual fatigue as measured. These findings contribute to a broader understanding of attentional and perceptual factors in multi-display OST AR environments, and suggest that the identity of the target display is a strong factor. We discuss possible explanations for these results and suggest future work to better characterize the attentional and optical factors underlying display-switching costs in OST AR environments.

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