

Rare Events, Costly Switches: How Attentional Distribution Asymmetry Modulates Switching Costs across Augmented Reality and the Real World

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Abstract

Augmented reality (AR) systems require users to distribute visual attention across virtual content and the physical world simultaneously. When this distribution is unequal, switching visual attention between the two sources may incur additional cognitive overhead. In this paper, we present a controlled within-subjects experiment in which participants completed an adapted Paced Visual Serial Addition Test (PVSAT) with an AR stimulus and a non-AR stimulus under five presentation ratios (9:1, 3:1, 1:1, 1:3, 1:9; for non-AR : AR), operationalized as a proxy for attentional distribution. Switch trials were significantly slower than repeat trials. Costs were larger under more extreme distributions on both sides of the ratio spectrum. Transitions toward the AR display were slower on average than transitions away from it, independent of ratio. Both effects appeared in response time alone; neither accuracy nor subjective workload differed. We discuss implications of switching costs and attentional distributions for AR systems.

CCS Concepts

• **Human-centered computing** → **Empirical studies in HCI**; **User studies**; • **Hardware** → **Emerging technologies**.

Keywords

Augmented Reality, Visual Attention, Attention Distribution, Cognitive Performance, PVSAT.

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1 Introduction

Augmented reality (AR) systems layer virtual content onto the user's view of the physical world rather than replacing it [33]. Users therefore can see both the real environment and AR information at the same time [2, 3, 12], and their visual attention at any moment can be split, to varying degrees, between the two. This distribution of visual attention can be thought of as a spectrum. At one end, the user is primarily engaged with the physical world and AR content plays a peripheral role; at the other, AR is the dominant source and the real environment fades into the background. Neither state is fixed: a technician following AR assembly guidance attends heavily to the overlay, while the same technician inspecting a physical component attends mainly to the real world. We call this relative weighting the *attentional distribution ratio*.

When attention is concentrated on one source, switching to the other can be cognitively costly. Sustained engagement biases processing resources toward the dominant source [1, 39], so switching requires a reorientation that takes time and effort [26]. The size of this cost likely depends on how rarely the switch occurs. The oddball paradigm, in which infrequent stimuli are embedded in a stream of frequent ones, suggests that rare events demand more processing than common ones, because they violate the expectation built up by the dominant stimulus [35]. The same logic can apply to AR, when a user has been attending mainly to the physical world, a demand from the AR overlay arrives as a rare event and may carry greater overhead than when attention is more evenly split. Whether the reverse direction behaves similarly, and how costs scale across the full distribution spectrum, remains an open question.

The direction of a switch may also matter independently of its rarity. Two mechanisms have been proposed. Shifting attention toward AR may require resolving virtual content rendered at a fixed focal plane against a physical scene at a different depth, an adjustment the reverse direction does not require to the same degree [17, 34]. AR overlays may also differ from the physical scene in perceptual salience, demanding more deliberate reorientation when they become task-relevant. Neither mechanism is settled, and the empirical evidence is mixed with Yu et al. reporting the opposite direction as slower in a visual search task [40].

Prior work has shown that switching between AR and non-AR sources carries measurable cognitive costs [2, 3, 40], and task-switching research has established that these costs depend on switch probability [7, 27] and task predictability [30]. However, prior work on AR switching fixed presentation demands at or near parity, characterizing costs at a single operating point rather than across the distribution spectrum. Designers therefore have little to no empirical basis for anticipating how switching overhead changes as one source becomes dominant.

To address this, we present a controlled within-subjects experiment varying display presentation ratio, the relative frequency of stimuli in AR and non-AR, as a proxy for attentional distribution, across five conditions: 9:1, 3:1, 1:1, 1:3, 1:9 (for non-AR : AR). Participants complete an adapted Paced Visual Serial Addition Test (PVSAT) [11] across an AR display and a co-located non-AR screen, which required participants to do rapid, serial arithmetic as a response to the stimuli. The stimulus order followed constrained pseudo-randomization to limit anticipation of the prevailing ratio.

We address two research questions:

RQ1: How do switching costs and information-processing efficiency change as attentional distribution ratio shifts from real-world-dominant to AR-dominant conditions?

RQ2: Are switching costs symmetric around the balanced distribution (1:1), or is switching *toward* AR more costly than switching away from it?

A *switch trial* occurs when the stimulus appears on a different source than the preceding one; a *repeat trial* occurs when consecutive stimuli appear on the same source. Switch trials also have a *direction*: towards AR (non-AR $\rightarrow$ AR) or away from it (AR $\rightarrow$ non-AR).

Our findings reveal three main effects. Switch trials were reliably slower than repeat trials. Switch costs were larger under more extreme distributions on both sides of the ratio spectrum. Switching toward AR was slower on average than switching away, independent of ratio. No effect was accompanied by an accuracy difference, ruling out a speed-accuracy trade-off. Together, these results show that switching between AR and the real world carries cognitive overhead across a wide range of attentional distributions, with a directional asymmetry that has implications for how task information should be distributed across them.

The remainder of this paper is structured as follows. Section 2 reviews related work. Section 3 describes the experiment. 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

Switching attention between AR and the real world can carry a measurable performance cost. When users alternate between them, attention allocation becomes a core cognitive challenge [4]. Visual search studies have consistently demonstrated this cost: participants are slower to acquire targets when they must shift between display sources than when they remain on one [2, 8, 13, 18, 40]. Gabbard et al. demonstrated this with a monocular AR display [12],

and follow-up studies by Arefin et al. demonstrated it with a custom haploscope, additionally finding that cost increased with target depth [2, 3]. These costs are not symmetric across directions: Yu et al. found that shifts from virtual to physical content were slower than the reverse [40], consistent with the broader task-switching finding that switching toward a more dominant or automatic task carries a larger residual cost [1, 26].

The vergence-accommodation conflict (VAC) offers an explanation for this asymmetry. Because AR headsets render content at a fixed focal plane, shifting attention toward AR requires resolving virtual content over a physical scene at a different depth, an adjustment that switching away from AR does not equally require. Lugtenberg et al. confirmed that reducing the VAC facilitated attention shifts between real and virtual objects [24]. Lisle et al. showed that depth-matching performance degraded with viewing distance, indicating the VAC distorts depth perception rather than simply adding latency [23]. Lee et al. found that increasing background complexity or viewing distance slowed visual search, consistent with VAC interference during attention shifts [21]. Imamov et al. showed a related effect in a dual-task design: performance was best when AR content was rendered at a greater depth than the primary task, and worse when above or below it [18]. The VAC thus predicts that switching toward AR can carry greater overhead than switching away, potentially independent of how frequently each switch occurs.

Prior work has therefore largely focused on controlling or characterizing optical confounds. Some studies used symmetrical attention distribution designs while other used asymmetric designs, but none has treated that asymmetry as something to manipulate or measure systematically. It remains an open question how switching costs scale as attention becomes concentrated on one source, or whether the directional asymmetry persists across the larger distribution spectrum. To address this, we held display depth constant at the AR focal plane [3, 8, 9, 12, 21, 40] and varied the attentional distribution ratio between AR and physical content as the primary independent variable.

2.2 Paced Visual Serial Addition Test and Oddball Paradigm

Cognitive load in AR research is most commonly measured with the NASA-TLX [5, 6, 16, 28], a subjective survey completed after each condition. The TLX captures perceived workload but not in-task performance fluctuations, and recent work has called for more objective alternatives [14]. Secondary information on a heads-up display has been shown to impose measurable response time costs on concurrent visual tasks [22], further motivating the need for an objective, trial-level measure sensitive to these costs. The Paced Visual Serial Addition Test (PVSAT) is one such measure. Originally developed for clinical assessment of processing speed and attentional fatigue [11, 37], it has been applied in AR research by Sasalovici et al. to measure how motion sickness affected cognition during OST-HMD use [31].

The PVSAT's structure makes it suitable for our purposes. Each trial requires the user to perceive a number, add it to the previous one, report the result, and update working memory [11]. The steps are individually simple but sustained attention taxes working

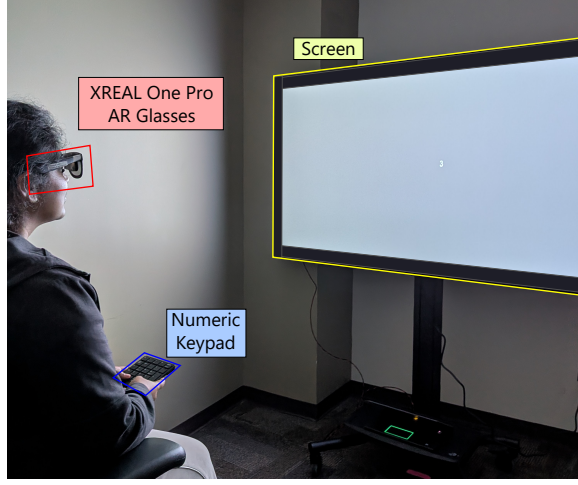


Figure 1: A participant wearing the XREAL One Pro AR glasses and holding a numpad keyboard. They are seated at a viewing distance of 2 m from the screen.

memory, as confirmed by MRI [10], and increases physiological arousal [19, 25]. Any factor that degrades working memory capacity can manifest as slower or less accurate responses, making the PVSAT sensitive to the attentional costs we aim to measure. Critically for our design, the high trial count it generates in a short period allows switching events to be embedded frequently enough to detect small performance differences.

The PVSAT is further well-suited to an oddball manipulation [35]. In the oddball paradigm, infrequent stimuli embedded in a frequent stream elicit greater processing overhead because they violate the expectation built up by the dominant stimulus. Switch cost magnitude is larger when the probability of a switch is low than when switches and repetitions are equiprobable [27, 36], and this effect scales with how rarely the switch occurs [36]. Even fully predictable switches carry a residual cost that preparation cannot eliminate [30], indicating that the overhead is not purely due to surprise but also to the reconfiguration required when changing sources. In our design, a switch trial, where the stimulus appears on a different source than the preceding one, functions as the oddball event. At extreme attentional distribution ratios, switches are rare and thus more unexpected, predicting larger costs than near the balanced condition. The PVSAT, combined with the oddball paradigm, therefore allows us to measure how switching costs vary with attentional distribution ratio within a single controlled task.

3 Experiment

This section describes the experiment we conducted. The experiment was approved by the ethics board of our university.

3.1 Participants

We recruited 22 participants for our experiment (13 male and 9 female; ages 18 to 42, $M = 24.1$, $SD = 5.43$) from the local university community. All of the participants had normal or corrected-to-normal vision. None of the participants reported known visual

or vestibular disorders, such as color or night blindness, dyschromatopsia, or a displacement of balance. Eight reported that they were slightly familiar with AR, five reported moderately familiarity, seven were very familiar, and two were extremely familiar.

3.2 Conditions

We wanted to examine how the distribution of attention across AR and the real world affects the cognitive cost of switching between them. As a proxy of attention distribution between the real world and AR, we use *display presentation ratio*, which is the relative frequency of stimuli across two display devices – AR and *non-AR*. Hence, we employed a within-subjects design in which participants performed the adapted PVSAT across the two display devices under five display presentation ratio conditions (see Table 1 and Figure 2).

Each trial was characterized by two categorical variables derived from the sequence of display events. *Transition type* classified whether the preceding and current display were the same (*repeat trial*) or different (*switch trial*). *Transition pair* identified the specific ordered combination of preceding and current display: *non-AR* → *non-AR* and *AR* → *AR* for repeat trials, and *non-AR* → *AR* and *AR* → *non-AR* for switch trials.

Table 1: Experimental conditions and their presentation ratios. Each condition defines the relative frequency of stimuli in non-AR and AR.

Presentation Ratio	Non-AR : AR
Non-AR extreme	9 : 1
Non-AR dominant	3 : 1
Balanced	1 : 1
AR dominant	1 : 3
AR extreme	1 : 9

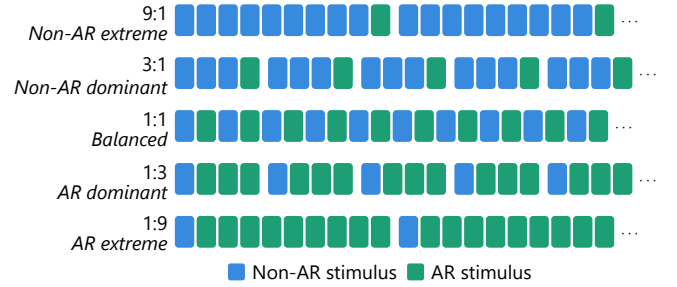


Figure 2: Illustration of a 20-trial excerpt from each condition. Blue and green squares represent stimuli in non-AR and AR, respectively. Stimulus sequences were pseudo-randomized and each participant completed 160 trials per condition.

3.3 Experiment Setup

Our experiment setup employed a dual-display configuration consisting of a large non-AR screen and an optical see-through (OST) AR headset. Participants were seated 2 m from the display in an enclosed room (dimensions: $7m \times 3.5m \times 10m$); see Figure 1.

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° .

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. [11]. The experiment was implemented in Unity 6.1, with stimulus presentation and response logging verified to within ± 1 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.

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 the consent form and provided their consent to participate in the experiment. The experimenter then described the task protocol. Participants completed a 10-trial practice block on the non-AR display to build familiarity with the PVSAT protocol and the response interface and told they could repeat the training until they were comfortable. On average, participants repeated the practice block four times until they reported sufficient comfort with both the numpad mapping and the task protocol, at which point they donned the AR glasses and advanced to the experimental trials.

The main task consisted of five blocks, one per presentation ratio condition (see Table 1 and Figure 2). Each block comprised 160 pseudo-randomized trials of the PVSAT (see Section 3.5). Block order was counterbalanced across participants using a balanced Latin square. Following each block, participants removed the AR glasses and the experimenter administered a self-timed rest break to reduce fatigue. During the break, participants completed the Raw TLX questionnaire to measure perceived workload for the preceding condition. This process was repeated for all five conditions.

Upon completing all conditions, participants filled out a post-experiment and demographics questionnaire. Participants were then debriefed and compensated for their time. The experiment took approximately 60 minutes in total.

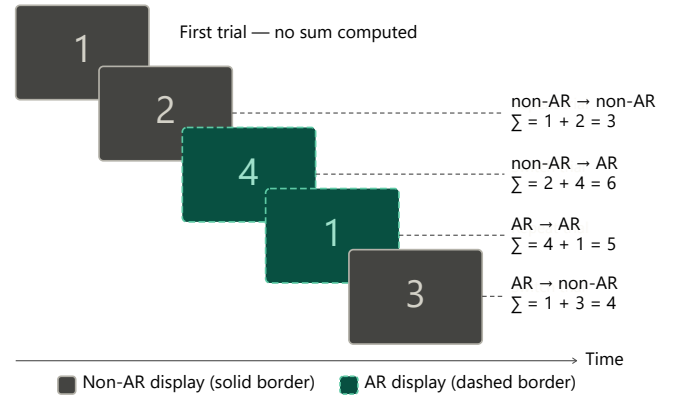


Figure 3: Participants completed the adapted PVSAT, in which digits (1–4) were presented for 450 ms followed by a 2000 ms response window. On each trial the active display was either non-AR or AR. Each block comprised 160 scored trials (≈ 8.5 min).

3.5 Cognitive Performance Task: Adapted Visual PASAT

To assess cognitive overhead induced by attentional switching, we administered an adapted version of the Paced Visual Serial Addition Test (PVSAT) [11].

3.5.1 Task Structure. On each trial a single digit (1–4) was presented centrally on the active display for 450 ms, followed by a fixed interval of 2000 ms. Restricting the digit range to 1–4 bounds all valid sums to the range 2–8, 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

¹<https://www.xreal.com/beampro>

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

subsequent stimulus were logged separately as *missed trials*. At least four practice blocks of 10 trials each preceded the main task to familiarize participants with the response procedure and the physical mapping to the numpad.

3.5.2 Stimulus Sequence Generation. Two display devices were used throughout the experiment: non-AR and AR. To ensure the psychological validity of the PVSAT oddball paradigm across the two devices, a constrained pseudo-randomization algorithm was implemented. The sequence of $N = 160$ trials was pre-generated according to a distribution ratio of non-AR:AR, where it represents the ratio in which whether stimuli is presented in AR and the screen. The larger value is the primary stimuli and lower value would be considered the “oddball.” To achieve pseudo-randomization, the following constraints were enforced:

- (1) **Balanced Block Allocation:** The total trial set was divided into n blocks of size S , where the ratio was $X : Y$ and $S = X + Y$. Within each block, the oddball stimulus was assigned to each possible ordinal position $(0, 1, \dots, S - 1)$ with equal frequency. This ensures that across the session, the oddball appears in every possible temporal slot an equal number of times, preventing position-dependent bias.
- (2) **Streak Limitation (Pattern Suppression):** In accordance with signal detection theory, human participants are prone to pattern-matching (or “gambler’s fallacy”) when a stimulus repeats. To mitigate this, a maximum streak limit of $k = 3$ was implemented for the primary stimulus. Any sequence of $X > 3$ was rejected and reshuffled. This prevents participants from entering a “rhythmic” or “automatic” response state, forcing sustained attentional engagement.

Given these overlapping constraints, we used the following heuristic. The sequence is generated and then checked against the rules above. If any violation is detected, the entire 160-trial array is discarded and regenerated. This makes that the final stimulus list more rigorous while remaining unpredictable to the participant.

At a total trial duration of 2450 ms (450 ms stimulus + 2000 ms interval), each condition lasted approximately 8.5 minutes. Each participant completed all five conditions which were Latin square balanced.

3.5.3 Dependent Measures. Two primary dependent measures were derived from the PVSAT:

- **Response Time (RT):** Mean response time 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; missed responses were treated as incorrect.

Following each condition, participants completed the Raw NASA-TLX to measure subjective workload [15].

At the end of the experiment, and prior to completing the demographic questionnaire, participants indicated whether they could distinguish stimuli on the AR display from those on the non-AR screen (*Yes / Maybe / No*).

3.6 Hypotheses

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

RQ1: Switching Costs with Distributed Attention.

- H1.1: Switch trials will be slower and less accurate than repeat trials.
- H1.2: Switch costs will be largest at extreme presentation ratios (9:1, 1:9) and smallest near the balanced condition (1:1), following an oddball-like pattern.

RQ2: Directional Asymmetry of Switching.

- H2.1: Switching toward the AR display will be costlier than switching away from it.
- H2.2: This asymmetry will be greatest under non-AR dominant ratios, where AR-directed switches are rare.

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 (LMMs) for continuous outcomes (response time) and generalized linear mixed-effects models (GLMMs) for binary outcomes (accuracy). Participant was included as a random intercept to account for individual differences in baseline performance, and trial number was included as a scaled co-variate to control for potential learning or fatigue effects. The significance of fixed effects was assessed via F -tests with Kenward–Roger degrees of freedom approximation for LMMs, and likelihood ratio tests for GLMMs, at the 5% significance level. Pairwise comparisons were conducted using estimated marginal means with Bonferroni correction [38]. Residual diagnostics (QQ plots, residual-versus-fitted plots, skewness, and kurtosis) confirmed that departures from normality were mild and consistent with typical response-time distributions, supporting the use of untransformed data [32]. All analyses were produced using R version 4.4.0, with the `lme4`, `lmerTest`, and `emmeans` packages.

Exclusions and Outlier Checks: Trial-level exclusions were applied prior to analysis. The first trial of each block was removed because no prior digit existed for the addition task. Anticipatory responses (response time < 150 ms [29]) and extreme responses ($|z| > 3$, trials exceeding ± 3 standard deviations from the participant’s average) were excluded. In total, 351 of 15372 trials (2.28%) were removed.

Participant-level screening was then applied using two criteria. First, overall accuracy was compared against a conservative chance level of $1/7 \approx 14.3\%$, corresponding to uniform random selection across the seven possible response keys (2–8). A one-tailed binomial test ($\alpha = .05$) was used to verify that each participant’s accuracy exceeded this threshold. Second, participants whose mean RT or mean accuracy deviated more than 3 SD from the group mean were flagged as potential outliers. Of the $N=22$ recruited participants, one met at least one of these criteria and was excluded, yielding a final analytic sample of $N=21$.

4.1 Response Time

4.1.1 Response Time Cost of Switching. To assess whether switching between devices incurred a response-time cost relative to repeating on the same device, we fitted a LMM with transition type (Switch vs. Repeat), presentation ratios, and their interaction as fixed effects, trial number (scaled) as a co-variate, and a random intercept for participant. Balanced ratio was excluded because the 1 : 1 presentation ratio produced no repeat trials by design. Table 2 summarizes the omnibus tests.

Table 2: Omnibus fixed-effect tests for the switch-cost model (transition type $\times$ presentation ratio) fitted to response times. Condition C excluded (no repeat trials).

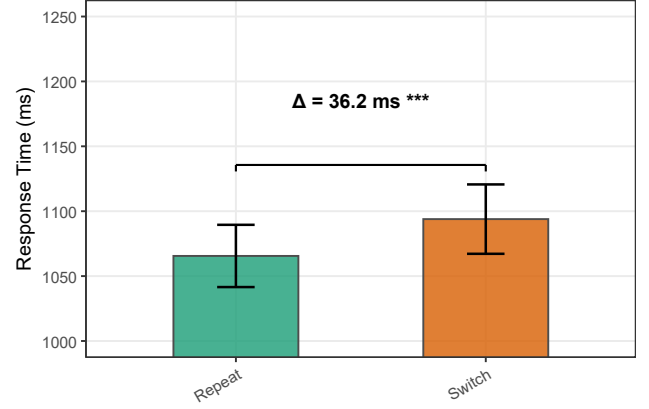
Measure	F	<i>p</i>	η_p^2
Transition Type	27.85	<0.001	0.002
Presentation Ratio	3.58	0.013	0.009
Trial Number (Scaled)	5.26	0.021	<0.001
Transition Type $\times$ Presentation Ratio	1.09	0.35	<0.001

In line with Hypothesis **H1.1**, transition type showed a significant main effect. Switch trials were an estimated 36.2 ms slower than repeat trials (*Switch* = 1100 ms, *SE* = 24.7; *Repeat* = 1064 ms, *SE* = 24.3; $t(11651) = 5.28$, $p < 0.001$). Presentation ratio was also significant, indicating that overall response speed varied across conditions irrespective of transition type. The Transition Type $\times$ Presentation Ratio interaction was not significant, suggesting that the magnitude of the switch cost did not reliably differ as a function of the presentation ratio. Trial number (scaled) was a significant co-variate, reflecting a learning effect across blocks.

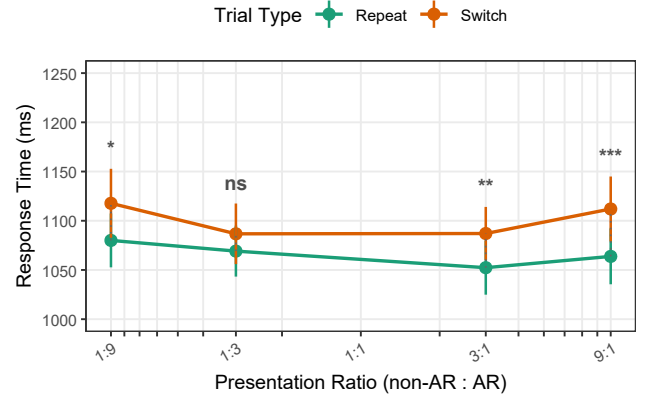
Table 3: Switch cost (Switch – Repeat) per presentation ratio. Positive Δ values indicate slower responses on switch trials in milliseconds.

Ratio (Non-AR : AR)	Δ (ms)	SE	<i>t</i>	<i>p</i>
AR extreme (1 : 9)	36.9	15.2	2.43	0.015
AR dominant (1 : 3)	19.7	12.1	1.63	0.104
Non-AR dominant (3 : 1)	33.6	12.0	2.79	0.005
Non-AR extreme (9 : 1)	54.7	15.2	3.60	< 0.001

To characterize the switch cost within each condition, we computed pairwise contrasts (Switch – Repeat) with Kenward–Roger degrees of freedom (refer Table 3). The switch cost was significant for the following ratios – AR extreme, non-AR dominant and non-AR extreme – but did not reach significance for AR dominant condition. This shows partial agreement with **H1.2**. The ordering is consistent on both sides of the spectrum: the cost is larger at 9:1 than at 3:1, and larger at 1:9 than at 1:3. However, the non-significant interaction cautions against treating this gradient as a reliable trend. Note also that the switch cost could not be estimated for the balanced (1:1) condition, which by design contained no repeat trials; the hypothesized minimum at balance is therefore not directly testable in this design.



(a) Repeat vs. Switch Trials



(b) Repeat vs. Switch Trials over Presentation Ratios

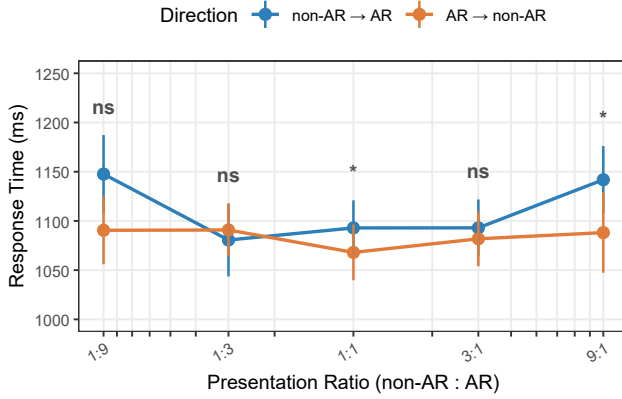
Figure 4: Experimental results for Response Time Cost of Switching. The x-axes shows (a) the transition type and (b) presentation ratio. The y-axes show response time in milliseconds. The vertical error bars indicate the standard error. The asterisks indicate statistical significance (** * $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, ns = not significant).

4.1.2 Response Time Cost of Switch Direction. To examine whether the direction of device switching affected response time, we fitted a LMM with transition pair (*non-AR* $\rightarrow$ *AR* vs. *AR* $\rightarrow$ *non-AR*), presentation ratios, and their interaction as fixed effects, trial number (scaled) as a co-variate, and a random intercept for participant. Only switch trials were included. Table 4 summarizes the omnibus tests. In line with Hypothesis **H2.1**, transition pair had a significant main effect: switching from the non-AR display to the AR display was an estimated 28.2 ms slower than the reverse direction (*non-AR* $\rightarrow$ *AR* = 1110 ms, *SE* = 25.2; *AR* $\rightarrow$ *non-AR* = 1082 ms, *SE* = 25.2; $t(6736) = 2.95$, $p = 0.003$). Presentation ratio also reached significance, however, Bonferroni-corrected pairwise comparisons among the five ratios revealed no significant differences (all $p > 0.12$). The Transition Pair $\times$ Presentation Ratio interaction was not significant, confirming that the direction asymmetry was consistent across

Table 4: Omnibus fixed-effect tests for the switch-direction model (transition pair $\times$ presentation ratio) fitted to response times. Only switch trials included.

Measure	F	p	η_p^2
Transition Pair	8.71	0.003	0.001
Presentation Ratio	2.56	0.036	0.002
Trial Number (Scaled)	0.93	0.34	<0.001
Transition Pair $\times$ Presentation Ratio	1.50	0.19	0.001

presentation ratios. Hence, **H2.2** was not supported: the directional asymmetry did not reliably vary with presentation ratio, including under non-AR-dominant conditions where AR-directed switches were rarest. Per-ratio contrasts (Figure 5) reached significance only at the balanced and non-AR extreme ratios; we do not interpret the remaining contrasts. Trial number (scaled) was not significant, indicating no learning effect within this subset of trials.

**Figure 5: Experimental results for Response Time Cost of Switch Direction. The x-axes shows five presentation ratios. The y-axes show response time in milliseconds. The vertical error bars indicate the standard error. The asterisks indicate statistical significance (* $p < 0.05$, ns = not significant).**

4.2 Accuracy

4.2.1 Accuracy Cost of Switching. To assess whether switching between devices affected accuracy relative to repeating on the same device, we fitted a GLMM with transition type (Switch vs. Repeat), presentation ratios, and their interaction as fixed effects, trial number (scaled) as a co-variate, a random intercept for participant, and a logit link function. Balanced ratio (1:1) was excluded (no repeat trials by design); missed trials were coded as incorrect. Table 5 summarizes the omnibus tests.

Contrary to Hypothesis **H1.1**, transition type was not significant, estimated accuracy was nearly identical for switch and repeat trials ($Acc_{Switch} = 0.809$, $SE = 0.029$; $Acc_{Repeat} = 0.813$, $SE = 0.029$; $OR = 1.02$, $z = 0.45$, $p = 0.656$). Presentation ratio had a significant main effect, with estimated accuracy ranging from 0.800

Table 5: Omnibus Wald χ^2 tests for the switch-cost model fitted to accuracy (logistic GLMM). Condition C excluded (no repeat trials).

Predictor	χ^2	df	p
Transition Type	0.40	1	0.529
Presentation Ratio	11.18	3	0.011
Trial Number (Scaled)	44.52	1	<0.001
Transition Type $\times$ Presentation Ratio	3.62	3	0.306

(AR dominant 1:3) to 0.822 (AR extreme 1:9). However, we did not find support for Hypotheses **H1.2**. Bonferroni-corrected pairwise comparisons revealed no significant differences between individual conditions (all $p > 0.31$), so the source of the omnibus effect cannot be localized to any specific contrast. Trial number was again significant, confirming a within-block learning effect. The Transition Type $\times$ Presentation Ratio interaction was also not significant, and no per-condition switch-cost contrast reached significance (all $p > 0.18$).

4.2.2 Accuracy by Switch Direction. To examine whether the direction of device switching affected accuracy, we fitted a GLMM with transition pair (*non-AR* $\rightarrow$ *AR* vs. *AR* $\rightarrow$ *non-AR*), presentation ratios, and their interaction as fixed effects, trial number (scaled) as a co-variate, a random intercept for participant, and a logit link function. Only switch trials were included; missed trials were coded as incorrect. Table 6 summarizes the omnibus tests.

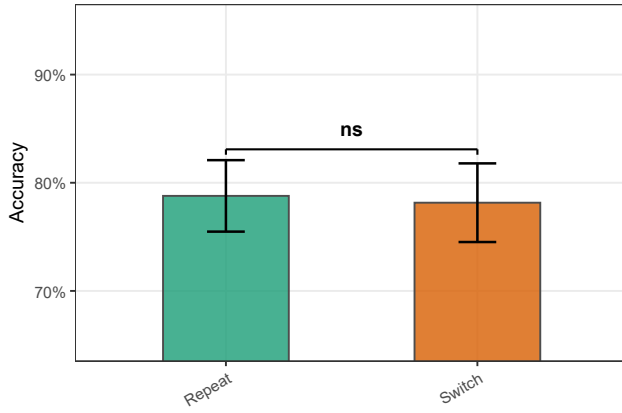
Table 6: Omnibus Wald χ^2 tests for the switch-direction model fitted to accuracy (GLMM). Only switch trials included.

Predictor	χ^2	df	p
Transition Pair	1.86	1	0.173
Presentation Ratio	2.64	4	0.621
Trial Number (Scaled)	38.43	1	<0.001
Transition Pair $\times$ Presentation Ratio	3.01	4	0.555

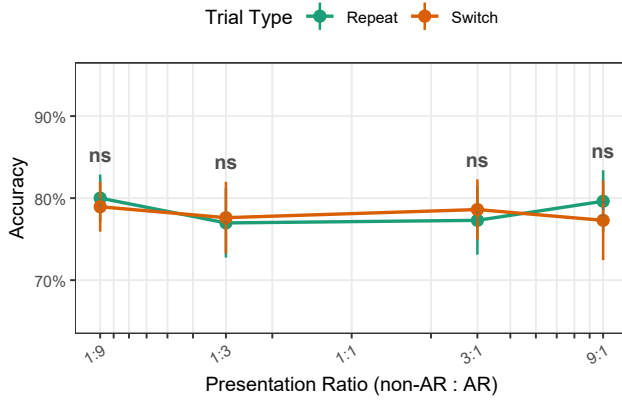
Contrary to Hypothesis **H2.1**, neither transition pair, nor presentation ratio, nor their interaction, reached significance. Estimated accuracy was similar for both switch directions ($Acc_{non-AR \rightarrow AR} = 0.812$, $SE = .028$; $Acc_{AR \rightarrow non-AR} = 0.815$, $SE = .028$; $OR = 0.99$, $z = -0.21$, $p = 0.831$). Condition-level accuracy on switch trials ranged from 0.801 (non-AR extreme ratio 9:1) to 0.830 (balanced ratio 1:1), with no pairwise contrast significant after Bonferroni correction (all $p > 0.35$). Trial number was the sole significant predictor, $\chi^2(1) = 38.43$, $p < 0.001$, indicating a learning effect whereby accuracy improved over the course of each block.

4.3 Subjective Responses

4.3.1 Task Load per Condition. We conducted a one-way repeated-measures ANOVA for each subscale with condition as the within-subjects factor. As shown in Table 7 and Figure 8, no subscale revealed a significant effect of condition (all $p > 0.54$), suggesting



(a) Repeat vs. Switch Trials



(b) Repeat vs. Switch Trials over Presentation Ratios

Figure 6: Experimental results for Accuracy Cost of Switching. The *x*-axes shows (a) the transition type and (b) presentation ratio. The *y*-axes show accuracy percentage. The vertical error bars indicate the standard error (*ns* = not significant).

that participants experienced comparable workload regardless of the attentional distribution ratio.

Table 7: Omnibus fixed-effect tests for NASA-TLX subscales across conditions.

Measure	F	p	η_p^2
Mental Demand	0.081	0.988	< 0.001
Physical Demand	0.778	0.542	0.03
Temporal Demand	0.011	1.00	< 0.001
Performance	0.343	0.848	0.01
Effort	0.027	0.999	< 0.001
Frustration	0.058	0.994	< 0.001

4.3.2 Subjective Display Identification . At the end of the experiment, and prior to completing the demographic questionnaire,

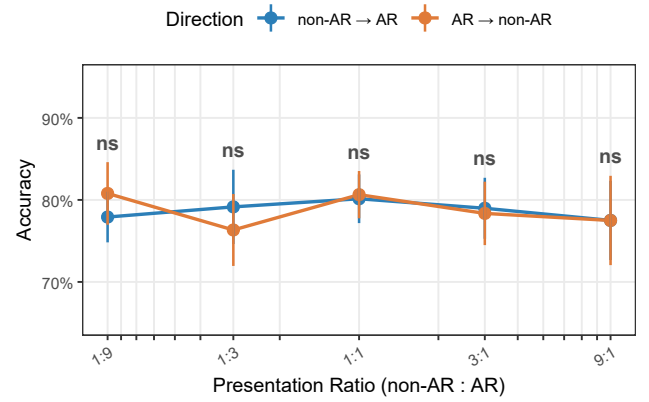


Figure 7: Experimental results for Accuracy by Switch Direction. The *x*-axes shows five presentation ratios. The *y*-axes show accuracy percentage. The vertical error bars indicate the standard error (*ns* = not significant).

participants indicated whether they could distinguish stimuli on the AR display from those on the non-AR screen (*Yes, Maybe, No*). Of the 21 participants, 15 reported that they could reliably distinguish the two displays, 2 were uncertain, and 4 reported that they could not. Among those who answered *Yes*, the most common reasoning was that the AR stimulus had a glow artifact surrounding it.

5 Discussion

The present study examined how the distribution of attention across AR and the real world affects the cognitive cost of switching between them. Three main findings emerged: First, switching between them imposed a processing-speed cost compared to not switching. Second, the cost was present at three of the four ratios where it could be estimated and was numerically larger under more extreme distributions on both sides of the spectrum, although the interaction with ratio was not statistically reliable. Third, transitions toward AR were on average slower than transitions in the opposite direction, with this average effect holding across ratios but individual per-ratio contrasts mostly unresolved. Across all conditions, neither accuracy nor perceived workload differed significantly. We discuss each finding.

5.1 Switching Carries a Cost

We found that responses on switch trials were slower than on repeat trials. This is consistent with prior work showing that switching between AR content and other information sources can be cognitively costly [2, 3, 40]; however, these studies used balanced attentional distribution between sources. In contrast, our results demonstrate that this cost is robust across a range of attentional distributions.

From a design perspective, systems requiring frequent switching between AR and the real world should treat each switch as carrying a small but cumulative temporal cost, even when the visuals are well-matched. This also raises a forward-looking question: as AR technologies advance toward greater perceptual coherence, such as

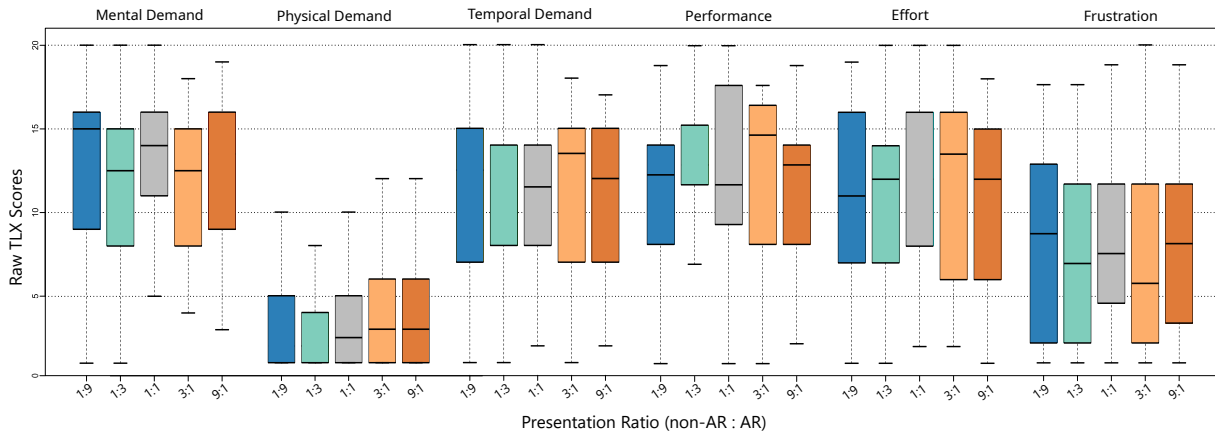


Figure 8: Raw TLX subscale scores across the five presentation ratio conditions. Lower scores are better.

higher-fidelity optics, better color matching, tighter spatial registration, will this switch cost diminish, or does it reflect something more fundamental about attending to information in AR? Because our task fixed stimuli centrally and required only paced arithmetic, it isolates the transition event but does not reproduce the spatial guidance, object manipulation, or navigation demands of applied AR. We expect the presence of a switch cost to generalize more readily than its magnitude, which is on the order of tens of milliseconds and is best read as accumulating across sustained use rather than as a consequential per-event penalty.

5.2 Attentional Distribution Modulates Switching Cost

Switch costs were reliably present at the AR extreme (1:9), non-AR dominant (3:1), and non-AR extreme (9:1) ratios, and did not reach significance at the AR dominant ratio (1:3). Numerically, the cost was larger at the more extreme ratio on each side of the spectrum: 54.7 ms at 9:1 against 33.6 ms at 3:1, and 36.9 ms at 1:9 against 19.7 ms at 1:3. This ordering is what an oddball account predicts, and it holds independently in both arms of the distribution. We note two constraints on interpreting it. First, the Transition Type $\times$ Presentation Ratio interaction was not significant, so we describe this as a consistent numerical trend rather than a reliable effect of ratio. Second, the balanced (1:1) condition contained no repeat trials by design and therefore contributes no switch-cost estimate; the oddball account also predicts a minimum at balance, but our design cannot test that prediction. What our data support most firmly is that switch costs persist across a wide range of attentional distributions, rather than being an artifact of the balanced designs used in prior work. When one source dominates an AR user's attention, a transition to the minority source arrives against their established expectations and demands greater reorientation.

This finding has direct design relevance. In most practical AR systems, attention is rarely divided equally between virtual and real-world content. A surgeon monitoring an AR overlay may glance at it only occasionally; a warehouse worker following AR instructions may attend to the physical environment only when navigating

between shelves. Our results suggest that switch costs are not confined to balanced attentional conditions and should be anticipated wherever users alternate between AR and physical content – including, and perhaps especially, when one source dominates. Whether the cost scales monotonically with imbalance remains open.

One open question concerns the boundary of this effect: the present design sampled five discrete points along the attentional distribution ratio spectrum and cannot identify whether costs plateau beyond 9:1 or 1:9 ratios. The balanced condition additionally contributes no switch-cost estimate, so the shape of the cost function near parity is unconstrained by these data. Future work should examine a finer gradient of ratios and the role of factors that may shift the curve, including transition predictability, the perceptual salience of the stimuli, and user expertise – our participants came from a single university community and ranged from slightly to extremely familiar with AR, which we report descriptively.

5.3 Switching Attention to AR is Slower than to the Real World

Beyond the general switch cost, we observed a directional asymmetry: transitions from the real world to AR were slower than transitions in the opposite direction. As a proxy for the real world we used a non-AR screen, which is a panel with stable, familiar optical characteristics. This choice was necessary to match retinal size and pixel density across sources, but it also removes the depth variation, motion, and clutter of an un-instrumented physical scene, each of which plausibly modulates re-orientation cost. For the AR stimuli, we used the XREAL OST-AR glasses which use an additive light model and rely on birdbath optics, which constitute a promising lightweight architecture that is seeing widespread adoption in recent years. Using this architecture, light from a micro-display reflects off a semi-reflective surface toward the eye while simultaneously transmitting ambient light [20]. This itself may introduce visual differences between the two stimuli, hence we made deliberate efforts to minimize visual perceptual differences by matching stimulus resolution, size, and spatial location (see Section 3.3.1). Despite that we still found a direction dependent effect. Subjective

responses at the end of the experiment indicated that most participants could distinguish stimuli in AR from those on the screen (see Section 4.3.2). Among those who could, the most commonly cited reason was a visible glow effect on AR stimuli, which is a characteristic artifact of birdbath displays. This suggests that transitioning towards AR might involve an additional perceptual adjustment step, and that the optical characteristics of current-generation AR hardware may be a contributing factor in the observed asymmetry.

Our directional result runs counter to Yu et al. [40], who found virtual-to-physical switches slower than the reverse in an optical see-through visual search task. We cannot resolve this discrepancy from our data, but two differences are worth noting. Their task required spatial search across a scene, whereas ours fixed stimuli centrally and isolated the transition itself; a search task loads re-orientation costs that a fixed-locus task does not. The optical architectures also differ. We therefore treat the direction of the asymmetry as hardware- and task-contingent rather than as a general property of AR attention switching, and we regard reconciling these results as an open question rather than one our study settles.

The present experiment does not isolate whether the asymmetry originates due to optical factors. Our results demonstrates the asymmetry itself and its consistency across attentional distribution ratios. For design purposes, this can have the following practical consequence: if transitions toward AR are systematically slower, then time-critical information delivered exclusively through the AR layer can place the user in a disadvantageous position, especially when having been attending elsewhere. Emergency notifications, navigation alerts, or other urgent cues may therefore benefit from stronger perceptual salience of AR stimuli, or its earlier onset, to compensate for the additional transition cost.

6 Conclusion

In this paper, we presented a controlled within-subjects experiment investigating the cognitive costs of switching visual attention between AR virtual content and real-world content, and how these costs are affected when attention is asymmetrically distributed between them. Using an adapted Paced Visual Serial Addition Test, participants completed trials under five presentation ratios (9:1, 3:1, 1:1, 1:3, 1:9; non-AR : AR), which served as a proxy for how attention is distributed between real world and AR content. Our results showed that switching between them imposed a measurable processing speed cost compared to not switching. The cost was present across most of the ratios tested and was numerically larger under more extreme distributions on both sides of the spectrum, an ordering consistent with an oddball account. We did not, however, find a statistically reliable interaction between switch cost and ratio, and the balanced condition contributes no switch-cost estimate by design. Switching costs were also direction-dependent on average, with transitions toward AR slower than transitions away from it, though the mechanism behind this asymmetry remains unresolved. These findings carry direct implications for AR interface design, especially for time-critical information. We discuss design implications, possible explanations and suggest future work to better characterize attentional switching between the real world and AR content.

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