Revisiting the explicit-implicit additivity assumption in visuomotor adaptation

Yifei Chen[1*] and Jordan A. Taylor[1,2]

[1]Department of Psychology, Princeton University, Princeton, New Jersey, United States, and [2]Princeton Neuroscience Institute, Princeton University, Princeton, New Jersey, United States

Corresponding author (*):
Yifei Chen: yifeichen2023@gmail.com; ORCID: 0009-0009-9271-5890

ABSTRACT: Explicit aiming strategies have been shown to play an important role in visuomotor adaptation – enabling rapid improvements in performance and affording flexibility – but their interaction and downstream consequences on implicit recalibration processes remain hotly debated. While early work assumed these processes combined additively, recent studies have challenged this view. However, these studies may have overlooked subtle spatial and temporal dynamics, which could influence how explicit aiming and implicit recalibration interact. Recent research shows that implicit recalibration anchors to where a person aims their movements, with aiming strategies directly shaping their spatial development. Moreover, implicit recalibration operates across multiple timescales, with both temporally volatile and persistent components. To examine whether these factors mask the true relationship between explicit strategies and implicit recalibration, we conducted a visuomotor rotation task while carefully accounting for the interplay of spatial and temporal dynamics. We employed two complementary tests: a direct test of strict additivity examining whether implicit and explicit components sum to total adaptation, and a slope analysis examining whether the two processes exhibit a compensatory tradeoff. After controlling for spatial dynamics (plan-based generalization) and temporal dynamics (forgetting), we found that strict additivity failed in both groups – implicit and explicit measures did not perfectly sum to total adaptation. Nonetheless, both groups showed a robust inverse relationship between explicit strategies and implicit recalibration, consistent with partial additivity. This pattern of compensatory tradeoff, despite imperfect summation, may result from simple methodological imprecision, the operation of additional but unobserved processes, or more complex nonlinear interactions between processes.

NEW & NOTEWORTHY: Recent work has challenged the idea that explicit strategy and implicit recalibration sum to produce visuomotor adaptation. We proposed that this apparent mismatch could arise from factors known to distort their relationship, including plan-based generalization and temporal decay. Using two complementary assays, we found that plan-based generalization shapes apparent additivity and the tradeoff between processes, but does not fully recover it. The residual gap likely reflects the interactions and contributions of additional learning processes.

INTRODUCTION

Sensorimotor calibration is essential for accurate and efficient motor execution of everyday activities. Traditionally, adaptation was thought to occur solely through implicit processes, operating unconsciously to adjust movement in response to changes of the body or environment (1, 2). More recently, explicit strategies – conscious, deliberate changes in motor planning – have been shown to play a larger role than previously thought, particularly in visuomotor rotation tasks (3, 4). These tasks are ideal for studying the potential influence of explicit strategies because the perturbations and their verbalizable solutions are in the same coordinate space. One key demonstration of this came from the aim-report paradigm (5), where participants explicitly indicated their intended aiming location before movement execution. This paradigm provided a direct measure of an explicit strategy on a trial-by-trial basis, allowing implicit recalibration to be inferred via the subtraction of the participant's reported aim from their executed reach (Figure 1a). It revealed that the stereotypical adaptation curve (e.g., power-law function) reflects the dynamic interplay between explicit strategies and implicit recalibration (Figure 1b). Because the initial aftereffects closely matched implicit recalibration inferred during training, albeit slightly smaller, it was taken as a validation of the aim-report method (5).

The aim-report paradigm assumed linear additivity between these processes, which was weakly supported by correlations between aftereffects and implicit recalibration (5). Since then, many studies have tacitly adopted this assumption, though recent efforts to validate the method using the Process Dissociation Procedure (PDP) have raised questions about its accuracy (6, 7, 8, 9, 10). The PDP method was originally developed to distinguish between conscious and unconscious processes in memory and decision-making (11, 6, 8, 12). In the context of visuomotor adaptation, PDP dissociates explicit and implicit processes by instructing participants to suppress (Exclusion trials; Figure 1c, left) or apply any strategy (Inclusion trials; Figure 1c, right) they may have learned to counteract the perturbation. In theory, the hand angle observed at the final stage of training and Inclusion trials should match, and the difference between Inclusion and Exclusion trials should provide an estimate of the explicit strategy (Figure 1d). Despite these predictions, the measures of explicit and implicit do not appear to perfectly match, calling into question the methodological attempts to dissociate the different processes and, potentially, the assumption of linear additivity (12).

Notably, both paradigms assume linear additivity, predicting that explicit strategies and implicit recalibration should sum to total adaptation and that increases in implicit recalibration should result in proportional decreases in explicit strategy when total adaptation remains constant. Whether the relationship reflects indirect interaction between independent processes or competition between the processes remains an open question (13). Evidence from aim-report and error-clamp studies suggests that implicit recalibration operates independently, proceeding in a stereotypical fashion regardless of error size or task relevance (14, 15, 16, 17). Under this view, explicit strategies simply compensate for the slack in implicit recalibration, achieving an angular value necessary for optimal performance. We should note that there is evidence for implicit recalibration also responding to changes in explicit strategies (18), but these interactions could be explained by two relatively independent processes operating in series or a feedthrough arrangement (19). Alternatively, their inverse relationship could be the result of a more direct interaction where implicit and explicit processes compete for error information: if one consumes the error signal, less remains available for another (13). In either case, an inverse relationship is assumed.

‘t Hart et al. (12) tested the inverse relationship using both aim-report and PDP paradigms to obtain independent measures of each process to avoid mathematical dependencies inherent in each method (5, 13). In the aim-report paradigm, implicit adaptation is derived by subtracting explicit aim reports from total adaptation, inherently inducing a mathematical dependency between the two measures. Likewise, in the PDP paradigm, explicit adaptation is inferred via subtraction of Exclusion from Inclusion. To test for a true correlation, at least two independent methods (e.g., aim-report and PDP) or, at least, data from two different phases of the experiment (e.g., PDP-Exclusion and washout phases) must be used. However, despite this dual-method approach, they found a lack of linear additivity using both independent and dependent measures of explicit and implicit processes, suggesting more complex interactions between the two. Notably, 't Hart et al. (12) tested additivity primarily through regression-based approaches: fitting slopes of implicit on explicit adaptation and checking whether 95% confidence intervals included −1 ('strict additivity'), or predicting total adaptation from weighted implicit and explicit measures and checking whether the slope of predicted versus actual adaptation included 1 ('loose additivity'). They did not conduct a direct summation test — i.e., testing whether the difference $D = A - (I + E)$ significantly deviates from zero for each participant. As we argue below, the regression approach conflates the question of whether processes sum within individuals with how they covary across individuals (20), making a direct summation test an important complement. Moreover, their simulation benchmark — which validated the -1 slope by generating data under additivity plus noise – assumed that all measures reflect a single static time point, without accounting for the possibility that processes change across the different task contexts in which they are measured.

While these findings are concerning, potentially invalidating more than a decade of research, two considerations complicate the interpretation of this null finding. There have been several studies demonstrating subtle yet complex spatial and temporal interactions that could explain the lack of an observed relationship between implicit recalibration and explicit strategies (21, 22, 23, 24, 25). First, plan-based generalization shows that implicit recalibration follows a Gaussian distribution centered at the explicit aiming location rather than the target location (Figure 1e; 21; 22). For example, McDougle et al. (22) found that aftereffects peaked near the probe target closest to participants' mean aiming direction (~26°), with aftereffects at the aiming location visibly larger than those at the training target (estimated from their Fig. 3A as approximately 13° vs. 10°, roughly a 20–25% reduction). During Exclusion trials, participants are instructed to reach directly to the target rather than their aiming location during training. Consequently, the measured implicit recalibration captures only a portion of the Gaussian distribution rather than its peak. Second, implicit recalibration has been shown to comprise temporally volatile and persistent components (24), with the volatile component dropping significantly after a 1-min delay, leaving only the persistent component remaining (Figure 1f; 26). The intertrial interval between successive reaches is often on the order of 5 seconds, and studies often have more than one target. Given typical intertrial intervals of a few seconds and multiple target locations, significant decay may occur between reaches to the same target. One or both of these factors, which arise from differences between training and PDP probe trials, could potentially explain the apparent lack of additivity between explicit strategies and implicit recalibration.

More broadly, interpreting regression slopes as direct tests of additivity conflates two distinct questions, a concern that applies to both ‘t Hart et al.’s analysis and perhaps the wider literature. Regression slopes between implicit and explicit measures reflect the covariance structure of learning processes — how they relate across individuals — rather than whether they sum within individuals (20). Under subtractive measurement, where one component is derived from total adaptation, the expected slope depends on the correlation between total adaptation and the measured component, not solely on whether additivity holds. Critically, the −1 benchmark assumes that total adaptation is constant across individuals. When total adaptation varies, the expected slope can deviate substantially from −1 even under linear additivity. For example, if individuals who adapt more also use larger explicit strategies, the expected slope will be shallower than −1. Even under independent measurements — where implicit and explicit processes are assayed separately — the slope still depends on the covariance structure between the two processes. Additivity places no constraint on this covariance; implicit and explicit processes can sum perfectly while being positively correlated, negatively correlated, or uncorrelated across individuals. Thus, slopes shallower than −1 may indicate individual differences in overall learning capacity rather than a violation of additivity (20). Nonetheless, the slope remains informative: a significant negative relationship provides clear evidence of a compensatory tradeoff, while the absence of a negative relationship is harder to interpret — it could reflect either true independence between processes or variance in total adaptation masking an underlying tradeoff.

Given these considerations, we adopt the following conceptual framework. Strict additivity refers to the case where implicit and explicit components sum exactly to total adaptation

$$A = I + E.$$

Partial additivity describes cases where the two components do not exactly sum up to total adaptation, but a compensatory tradeoff still exists, a significant inverse I-E relationship. Non-additivity refers to the absence of any systematic tradeoff, suggesting the processes operate independently. Under this framework, ‘t Hart et al.’s null finding could reflect either (1) methodological factors masking a true tradeoff, or (2) genuine non-additivity. The present study addresses both possibilities.

The present study tested the linear additivity assumption by independently measuring explicit and implicit processes while accounting for spatial (plan-based generalization) and temporal (decay) interactions. Using a within-subject PDP design, we manipulated target location and delay as participants adapted to a 45° visuomotor rotation, with one group reporting their aiming strategies and a control group without aim reports (to control for potential effects of the reporting procedure; see 8, 27). We employed two complementary tests: a direct test of strict additivity examining whether the difference between total adaptation and the sum of implicit and explicit components equals to zero, and a slope analysis examining whether implicit and explicit processes exhibit a compensatory tradeoff. Strict additivity failed in both groups—implicit and explicit measures did not sum exactly to total adaptation. Nonetheless, both groups showed a robust inverse relationship between explicit strategies and implicit recalibration, indicating that the processes trade off, but do not perfectly sum. In the aiming group, plan-based generalization significantly modulated this tradeoff, with stronger inverse relationships observed when adaptation was probed at participants’ actual aiming locations rather than the training target, and temporal decay showed a marginal effect. Despite these spatial and temporal influences on the detectability of the tradeoff, the systematic deviation from strict additivity was consistent across conditions and groups, suggesting that the remaining gap likely reflects methodological imprecision, the operation of additional unobserved processes, or more complex interactions between learning systems.

MATERIALS AND METHODS

Participants

Sixty participants were recruited from the research participation pool managed by the Department of Psychology at Princeton University in exchange for course credit. Two participants were excluded from analysis for not following instructions (see Data Analysis), resulting in a final sample of fifty-eight participants (37 females, 21 males; mean age: 19.64, SD: 1.24). Recruitment was limited to individuals who were right-handed, had normal or corrected-to-normal vision to receive visual feedback, and had normal color vision. Participants were assigned to either an aiming group (n = 29) or a control group (n = 29).

Task and Apparatus

All participants made rapid "shooting" movements with a digitizing stylus (Intuos 3, Wacom) to bring a virtual cursor (0.8 cm diameter) to a target (1 cm diameter) positioned 7 cm from the start position (Figure 2a). If the movement exceeded 300 ms, an auditory warning saying “too slow” was triggered. Visual stimuli were displayed on an LCD touchscreen monitor (Dell) mounted 23.5 cm above the tablet, preventing direct vision of the hand. Between trials, an empty circle dynamically adjusted in size based on the radial distance between the hand and the center of the tablet to guide participants back to the start position (1 cm diameter), where they held for 500 ms before proceeding. A desktop PC (Dell) running custom MATLAB software (28) controlled the task.

The experiment consisted of 400 trials spread over four blocks. The first block was designed to familiarize participants with all aspects of the task, which consisted of 40 trials (Baseline block). For the first 10 trials, participants were provided with veridical continuous feedback of the cursor location. Cursor feedback was removed for the next 10 trials to measure any potential movement bias, before being reintroduced for an additional 10 trials with feedback. For the last 10 trials, the aiming group practiced reporting their intended aiming location with their left hand prior to executing each reach with their right hand, while the control group continued reaching without reporting their aims.

Following the baseline block, a 45° rotation was introduced between the movements of the hand and the visual feedback. Participants were then trained to overcome this rotation for 200 trials, while the aiming group continued to report their aim, and the control group did not (Training block). The direction of rotation was counterbalanced across participants. The location of the target was always at 90° (north) for the Baseline and Training blocks.

After the Training block, participants were introduced to the Process Dissociation Procedure (PDP), which unfolded over four phases of 30 trials (PDP blocks, Figure 2c). Each PDP block included 5 Exclusion trials, 10 Top-up trials, 5 Inclusion trials, and another set of Top-up trials. During Exclusion trials, cursor feedback was removed, and participants were instructed to reach directly toward the target without using any compensatory strategies (Figure 2b, bottom panels). The target turned from green to red to further indicate that participants should refrain from implementing any strategy. Exclusion trials are designed to assay implicit recalibration in relative isolation from explicit strategies (6, 8, 12). During Inclusion trials, cursor feedback was removed and participants were instructed to use any strategy they might have developed to counteract the perturbation in order to get their now unseen cursor on the target

(Figure 2b, top panels). Inclusion trials are assumed to assay the joint operation of explicit strategies and implicit recalibration (6, 8, 12). In between the Exclusion and Inclusion trials were Top-up trials where cursor feedback was restored, and participants were instructed to try to get the rotated cursor on the 90° training target. These trials were designed to recover any adaptation that may have decayed during Exclusion/Inclusion trials since cursor feedback was removed. The order of the Exclusion and Inclusion sets alternated after each PDP block, and they were always separated by Top-up trials. Throughout the PDP blocks, the aiming group continued to report their aiming location by tapping on the monitor using their left hand.

The four PDP blocks (Figure 2c) were designed to test if plan-based generalization and temporal decay affect linear additivity. Here, we used a within-subject 2x2 factorial design with factors of Target Location (at training location or aiming location) and Delay (no delay or 1-min delay). In the Training Location condition, the target appeared at 90° (north), which is the same target location as during the training block, for both Inclusion (Figure 2b, top-left) and Exclusion trials (Figure 2b, bottom-left). In the Aiming Location condition, the target appeared at the average aiming angle from the five most recent Top-up/adaptation trials before the current Inclusion/Exclusion set for the aiming group (Inclusion example: Figure 2b, top-right; Exclusion example: Figure 2b, bottom-right). For the Control Group, the aiming target location was fixed at 39° based on the average late adaptation aim reports (last 10 trials of the Training block) from our pilot study (29). Because this fixed location reflects a group average rather than each individual's actual aiming direction, some spatial misalignment is inevitable for control participants whose true aiming strategies deviate from 39°. This added noise may attenuate any plan-based generalization effects in the control group, which is one reason we do not conduct a factorial analysis of target location effects in this group. To test for the effects of temporal decay, a 1-min delay was introduced between the final trial of Top-up and the onset of Exclusion/Inclusion in the Delay condition. The order of the four PDP blocks (Target Location and Delay combinations) was randomized for each participant. The experiment ended with 40 no-feedback no-aim-report trials to wash out any adaptation (Washout block).

Data Analysis

Hand angles were calculated as the angular distances between the target and the hand's endpoint position. Aim reports were calculated based on the angular distances between where participants tapped on the screen relative to the target. To standardize the data, we flipped all hand angles and aim reports in the same direction, ensuring that positive values always reflected the direction of counteracting the rotation, regardless of whether the rotation was clockwise (-) or counterclockwise (+).

Testing Linear Additivity

To test the assumption of linear additivity, we employed two complementary approaches that address distinct questions. The primary test directly examined strict additivity by computing the residual difference

$$D = A - (I + E)$$

for each participant, where A is total adaptation, I is implicit adaptation, and E is explicit adaptation. Under strict additivity, mean (D) should equal zero, which we tested using a one-sample t-test.

The secondary test examined whether implicit and explicit processes exhibit a compensatory tradeoff by regressing implicit on explicit measures:

$$I = \beta \times E + Intercept.$$

As elegantly outlined in a recent study, regression slopes do not directly test additivity — the expected slope depends on the covariance structure rather than whether processes sum (20) — but a significant negative slope still indicates that the processes compensate for one another across individuals, while the absence of a negative relationship is harder to interpret.

We refer to these two statistical approaches as the summation test (D test) and the tradeoff test (slope analysis), respectively. Each test was applied at two levels of analysis: an overall analysis using measures collapsed across conditions in both groups, and a condition-specific analysis using the 2x2 factorial structure in the aiming group only. The overall analysis uses aftereffect as the implicit measure, Inclusion - Exclusion as the explicit measures, and Top-up as total adaptation, ensuring non-overlapping trial types of both groups. The condition-specific analysis uses Exclusion as the implicit measure, aim reports as the explicit measure, and Top-up as total adaptation, enabling within-subject comparison across Target Location and Delay.

Measures for Overall Analysis

For the primary test of strict additivity (residual D test) and the overall slope analysis, we used the same measures for both groups to allow direct comparison. Total adaptation (A) was defined as the mean hand angle across Top-up trials, which were interspersed between Exclusion and Inclusion trials throughout the four PDP blocks to maintain adaptation with feedback (see Figure 2c). Each participant's Top-up performance was averaged across all PDP blocks to yield a single value. Implicit adaptation (I) was defined as the mean hand angle over the first five trials of the Washout block (aftereffect), which reflects residual adaptation after participants were instructed to reach directly to the target without any strategy. Explicit adaptation (E) was computed as the difference between Inclusion and Exclusion trials (five trials each), averaged across all four PDP conditions. This yields one Inclusion value and one Exclusion value per participant, with explicit adaptation calculated as their difference.

This approach ensures that explicit and implicit measures are based on non-overlapping, independent trial types in both groups. Notably, the implicit measure (aftereffect) is fully independent of both Inclusion and Exclusion trials, reducing mathematical dependency between I and E. We collapsed across conditions for the main additivity test because the control group does not permit condition-specific comparisons. In the control group, implicit adaptation was measured only once in the Washout block and does not vary by condition, while only explicit measures (Inclusion - Exclusion) vary across the four PDP conditions. Comparing residuals or slopes across conditions would involve multiple comparisons using the same implicit data point, violating independence assumptions.

We also note that the D test can only be conducted using Top-up trials as total adaptation. Using Inclusion trials as total adaptation would introduce mathematical coupling because the explicit measure (Inclusion - Exclusion) shared Inclusion with the total, making the test circular.

Measures for Condition-Specific Analysis (Aiming Group)

To examine how plan-based generalization and temporal decay affect both the implicit-explicit tradeoff and the residual D, we conducted a separate within-subject 2x2 analysis in the aiming group using condition-specific measures. Here, total adaptation (A) was defined as the mean hand angle across ten Top-up trials within each PDP block (corresponding to each condition). Implicit adaptation (I) was defined as the mean hand angle across the five Exclusion trials in each condition. Explicit adaptation (E) was quantified as the mean aim report from the five Inclusion trials in each condition. This allowed us to assess how target Location (Training vs. Aiming) and Delay (No-Delay vs. 1-minute) affected the residual and slope within individuals. The analysis is possible only in the aiming group because aim reports provide independent, condition-specific measures of explicit adaptation that do not overlap with Exclusion trials.

Simulation-Based Slope Benchmark for Linear Additivity

To evaluate whether the observed slopes align with the linear additivity model, we first simulated data based on empirical data under the assumption of additivity. Because the commonly used −1 benchmark assumes zero variance in total adaptation (e.g., 20), we derived expected slopes via simulation using our empirical variance structure.

We ran simulations using two definitions of total adaptation to test how this choice influences the expected slope. We report results for both definitions because differences in measurement variance across operationalizations can change the observed slope. In our first definition, total adaptation (A) was measured on the Top-up trials, capturing feedback-based adaptation, and occurring immediately before the PDP probes. Using the means and standard deviations of Top-up performance and explicit adaptation (Inclusion - Exclusion) from each group average across all PDP blocks, we generated 10,000 simulated datasets under the additivity assumption: we sampled total adaptation and explicit adaptation as follow:

$$A_{simulated} \sim N(\mu_{Top-up}, \sigma_{Top-up})$$

$$E_{simulated} \sim N(\mu_{Inclusion\,-\,Exclusion}, \sigma_{Inclusion\,-\,Exclusion}).$$

Under the additivity assumption, total adaptation reflects the sum of implicit and explicit processes at probe time. Thus, we simulated implicit adaptation (I) as:

$$I_{simulated} = A_{simulated} - E_{simulated}.$$

The alternative approach used Inclusion trial performance as total adaptation (A), which is structurally tied to our PDP-based explicit measure and reflects adaptation at probe time without feedback. Here, we sampled Inclusion performance as A and Exclusion performance as I with our empirical parameters across all PDP blocks:

$$A_{simulated} \sim N(\mu_{Inclusion}, \sigma_{Inclusion})$$

$$I_{simulated} \sim N(\mu_{Exclusion}, \sigma_{Exclusion}),$$

then computed simulated explicit adaptation as the following:

$$E_{simulated} = A_{simulated} - I_{simulated}.$$

For each simulation approach, we fit linear regression models to the simulated data to derive the distribution of expected slopes under strict additivity. By presenting simulations under both operationalizations, we illustrate how the expected slope may vary depending on measurement choices.

Statistical Inference

Regression slopes and their significance were estimated using ordinary least squares (OLS). Subject-level bootstrap resampling (1,000 iterations) was used to construct confidence intervals for individual slopes and to derive sampling distributions for contrasts between slopes (including main effects, interactions, and comparisons against simulation benchmarks) where analytic distributions are not readily available. For condition-specific analysis, we resampled participants with replacement within each group and carried each selected participant's data across all within-subject cells (Target Location and Delay) to preserve pairing in each iteration. For every analysis, we report the observed slope and the 95% confidence interval from the bootstrap distribution.

For the main additivity test, we compared overall slopes between groups and against simulated predictions from the linear additivity model. Slopes were considered consistent with additivity if the observed 95% confidence interval overlapped with the simulated distribution, and inconsistent if they fell outside this range. For the condition-specific analysis in the aiming group, we conducted a 2x2 factorial analysis examining how Target Location and Delay affected the slopes β and residuals D. For each bootstrap iteration, we computed marginal means and derived main effects as differences between factor levels and interactions as differences-of-differences. Statistical significance was determined by whether 95% bootstrap confidence intervals excluded zero, with two-tailed p-values calculated as twice the smaller tail probability.

Testing Participants' Understanding of the Task

As the dissociation and test of additivity between explicit and implicit critically hinges upon participants' understanding of the specific instructions (29), we first verified participants' knowledge of the instructions. After being introduced to the task, they were asked to answer a few questions and would receive clarifying instructions if necessary before beginning the actual experiment. They were asked to indicate whether they should move directly toward the target or elsewhere in the following conditions: 1) baseline trials, 2) mismatch-on (mismatch between the cursor and the hand), and 3) mismatch-off trials. The correct answers were: 1) directly toward the target, 2) elsewhere, and 3) directly toward the target. Participants in the aiming group needed to answer an extra question about where to tap when reporting their aim (cursor vs. hand position), with the correct response being the hand position.

We then implemented a post-experiment survey to screen for individuals who failed to follow the instructions for both experiments. The post-experiment survey included 4 questions, each corresponding to a specific combination of Target Location (aiming location or training location) and trial type (Exclusion trial with a red target; Inclusion trial with a green target). The visuals matched those used in the actual task. For each question, participants indicated where they aimed by tapping along an "aiming ring" centered at the start position, with the target appearing on the ring. Note that the aiming location from the survey was the average aiming location from our pilot study (29), which was the same aiming location for the control group during the actual task. Individuals who aimed within 0.8 cm of the target (the target's diameter) during the Exclusion trials (both aiming and training locations) would pass the screening criteria and not be excluded, as aiming at a location other than the target in the Exclusion trials shows that they did not understand the instructions. Because Inclusion trials lack a dedicated instruction-check, we screened asymptotic performance by averaging the hand angle of each Inclusion block (the same trials used for the implicit–explicit correlation) across the four conditions and flagging participants whose mean hand angle was more than ±4 SD from the group mean. In the aiming group only, we applied the same ±4 SD criterion to the mean aim-reports to identify potential noncompliance or inconsistent reporting. As a result, two participants were excluded: (i) one control-group participant who failed to tap the target during an Exclusion trial when the target was at the aiming location, and (ii) one aiming-group participant whose aim-reports were extreme during an Inclusion block with the target at the aiming location.

All data and analysis code in Python can be openly accessed at: https://doi.org/10.17605/OSF.IO/ZT8PD.

RESULTS

Participants successfully adapted to the visuomotor rotation across groups

To investigate whether implicit and explicit adaptation linearly add up to total adaptation, we measured explicit and implicit processes while accounting for spatial (plan-based generalization) and temporal (decay) interactions. Specifically, the influence of spatial dynamics was examined by manipulating the Target Location (target at aiming vs training locations) and the influence of temporal dynamics by manipulating the Delay in cursor feedback (no delay vs 1-min delay) as participants adapted to a 45° visuomotor rotation in a within-subject design ($n = 29$). Importantly, to ensure that our assessment of the potential relationship between explicit and implicit processes was not the result of a necessary statistical correlation, we obtained independent measurements of each process via the aim-report and PDP techniques. We also included a control group ($n = 29$) who were not asked to report their aim on each trial to assess potential effects of the reporting procedure.

Participants successfully adapted to the 45° visuomotor rotation, as indicated by an appropriate change in hand angle to counteract the perturbation over the last 10 trials of the Training block compared to Baseline in both aiming and control groups (Figure 3a; aiming: 43.75 ± 0.36°; $t(28) = 65.73$, $p < 0.001$; control: 43.15 ± 0.46°; $t(28) = 61.41$, $p < 0.001$), and the adaptation is comparable across groups ($t(56) = 1.01$, $p = 0.317$). This comparable performance persisted throughout the PDP blocks, with no significant differences between groups in Exclusion (Figure 3b-c; $t(56) = 0.55$, $p = 0.588$) or Inclusion trials (Figure 3b-c; $t(56) = 1.92$, $p = 0.060$) when we averaged across all PDP blocks.

Adaptation was maintained throughout testing, with no difference between Inclusion and Top-up trials when the probe-target was at the training location without delay (Figure 3b-c; aiming: Top-up, 43.89 ± 0.49°; Inclusion, 44.01 ± 0.89°; $t(28) = -0.14$, $p = 0.891$; control: Top-up, 43.47 ± 0.30°; Inclusion, 44.46 ± 0.82°; $t(28) = -1.27$, $p = 0.214$). Neither Target Location (aiming: $F = 1.57$, $p = 0.221$; control: $F = 0.576$, $p = 0.454$), Delay (aiming: $F = 0.31$, $p = 0.583$; control: $F = 1.43$, $p = 0.242$), nor their interaction (aiming: $F = 3.99$, $p = 0.055$; control: $F = 1.88$, $p = 0.181$) showed significant effects on Exclusion trials across groups. Similarly, Inclusion trials showed no effects of Target Location (Figure 3b; $F = 1.324$, $p = 0.260$), Delay ($F = 0.979$, $p = 0.331$) nor interaction ($F = 0.056$, $p = 0.815$) in the aiming group. While there was no effect of Delay (Figure 3c; F = 0.526, p = 0.474) or an interaction (F = 1.14, p = 0.295), there was a significant effect of Target Location (F = 17.79, $p < 0.001$), with participants consistently reaching below the optimal hand angle when the probe target was at the Aiming Location (t(28) = -4.218, $p < 0.001$). While we did not find a significant effect of our delay manipulation on average Exclusion or Inclusion performance (24, 26), trial-by-trial analysis revealed significant within-block decay in both Exclusion and Aftereffect measures (Supplementary Figure S5c). Combined with the relatively long intertrial interval on the first no-delay probe trial (Supplementary Figure S1; 12.19 ± 5.25 s), this suggests that substantial volatile decay had already occurred before the first probe, potentially creating a floor effect that limited our sensitivity to detect additional effects of the 1-minute delay.

Summation test: Implicit and explicit measures do not sum to total adaptation

Before examining the tradeoff between implicit and explicit adaptation, we first tested whether these components sum to total adaptation by computing the residual $D = A - (I + E)$ for each participant. Total adaptation was measured as mean Top-up performance across PDP blocks (aiming: 43.79 ± 2.02°; control: 43.95 ± 1.25°). Implicit adaptation was measured as the aftereffect (aiming: 13.18° ± 6.20°; control: 13.46° ± 7.65°), and explicit adaptation was computed as Inclusion - Exclusion collapsed across conditions (aiming: 26.16 ± 11.6°; control = 23.15 ± 10.9°). In both groups, the residual D differed significantly from zero (aiming: 4.44 ± 10.26°, t(28) = 2.33, p = 0.027; control: 7.34 ± 9.29°, t(28) = 4.25, $p < 0.001$), indicating that strict additivity was not supported.

To examine whether spatial and temporal factors affected the magnitude of the deviation from strict additivity, we conducted a parallel 2×2 analysis on the residual D in the aiming group using condition-specific measures (total adaptation = Top-up within each PDP block; implicit = Exclusion; explicit = aim reports). Neither Target Location (F = 0.26, p = 0.614) nor Delay (F = 0.16, p = 0.695) significantly affected the residual, and there was no interaction (F = 1.51, p = 0.229). This indicates that the systematic deviation from strict additivity is consistent across conditions and is not uniquely driven by plan-based generalization or temporal decay.

Tradeoff test: Robust inverse relationship indicates partial additivity, with plan-based generalization modulating the tradeoff between implicit and explicit processes

Having established that the sum of implicit and explicit adaptation does not match with total adaptation, we next asked whether implicit and explicit processes trade off given the recent findings suggesting competition between the processes (13, 12) while other work finds implicit to be highly stereotyped and operates with a relative degree of independence from explicit processes (30, 31, 19, 14, 15, 32, 33, 34). To test this, we regressed implicit on explicit measures at the individual level, where a significant negative slope would indicate a compensatory tradeoff. To ensure independence between measures, we used non-overlapping trial types for each component. In the aiming group, explicit adaptation was quantified as the mean aim-report using the Inclusion trials or the difference between Inclusion and Exclusion trials, while implicit adaptation was the average hand angle across the Exclusion trials. In the control group, explicit adaptation was calculated as the difference between Inclusion and Exclusion trials, with implicit adaptation measured as the aftereffect in the washout block.

In the aiming group, where participants explicitly reported their aiming directions, we found significant negative correlations between implicit and explicit adaptation across all conditions (Table 1; Figure 3d). This confirmed the expected inverse relationship — greater explicit strategy use was associated with reduced implicit recalibration. However, these slopes were consistently shallower than -1, the benchmark traditionally associated with strict additivity. As discussed below, whether these slopes deviate from additivity may depend on how total adaptation is operationalized in the simulation benchmark, as different measures have different variances. This pattern persisted even when analyzing only participants who strictly followed instructions (See supplementary Figure S2) using the same criteria from our pilot study, ruling out inattention or task misunderstanding as explanations (29).

Bootstrap hypothesis testing on the regression slopes revealed a significant main effect of Target Location, with stronger inverse relationships (more negative slopes) observed when the probe target was at the Aiming Location compared to the Training Location (Figure 3d; mean difference = -0.322, 95% CI [-0.438, -0.211], $p < 0.001$). There was a marginal effect of Delay (mean difference = 0.115, 95% CI [0.000, 0.254], $p = 0.050$), suggesting that the delay slightly weakened the implicit-explicit relationship. There was no interaction between Target Location and Delay (mean difference = 0.149, 95% CI [-0.064, 0.370], $p = 0.156$). These results indicate that plan-based generalization significantly modulates the strength of the implicit-explicit tradeoff when explicit strategies are independently measured through aim reports, while temporal decay has only a marginal influence.

The control group used the Process Dissociation Procedure without reporting aims, keeping the same 2x2 factorial structure as the aiming group. The parallel design allows us to isolate the effect of reporting aims and examine whether the same experimental manipulations would yield similar patterns when explicit strategies were measured through behavior rather than verbal report. Note, however, the design of the control group does not permit an independent measure of explicit aiming – there are no aim-reports – and the PDP method cannot estimate explicit adaptation without using implicit adaptation measured from Exclusion trials, which creates a statistical dependency. As a result, we cannot perform the same factorial analysis as in the aiming group. To obtain a more generic estimate of the relationship for the control group, we quantified explicit adaptation by averaging the difference between Inclusion and Exclusion trials across all PDP blocks and quantified implicit adaptation from the single washout block at the end of training. When we averaged across conditions for the control group, we again observed a strong inverse relationship (Table 1; Figure 3e).

While using the difference between Inclusion and Exclusion trials has been the traditional way to compute explicit measures using the PDP paradigm, it may be problematic when targets appear at novel locations, as participants could become confused about how to express their learned adaptation during Inclusion trials. To test this possibility, we recomputed explicit adaptation using Top-up trials, where the target always appeared at the trained location, instead of Inclusion trials. This alternative approach yielded a much stronger inverse relationship (Supplementary Figure S3b; $\beta = -1.061$, 95% CI [-1.350, -0.785], $p < 0.001$), approaching perfect linear additivity. This suggests that the traditional PDP approach may obscure the relationship between the two components when attempting to account for plan-based generalization by probing the adaptation at a new location.

To enable direct comparison between the two groups, we reanalyzed the aiming group data using the same methodological approach as the control group (aftereffects as the implicit measure, difference between Inclusion/Top-up and Exclusion as the explicit measure, averaging across conditions). Under these identical methodological conditions, the aiming group still showed partial additivity between the two processes (Supplementary Figure S3c-d), suggesting that the act of reporting itself may indirectly or directly affect the relationship itself. We should note, however, that these analyses are post hoc and may be subject to the problem of multiple comparisons.

Simulation benchmarks: Observed slopes confirm compensatory tradeoff but fall short of strict additivity

To evaluate whether the observed slopes align with linear additivity, we compared them against simulation benchmarks derived under two operationalizations of total adaptation (Table 2; Figure 4). The expected slope under additivity depends on the variance of total adaptation (20): greater variability leads to shallower expected slopes. Top-up trials showed relatively low variability (aiming: SD = 2.02°; control: SD = 1.25°), while Inclusion trials showed greater variability (aiming: SD = 6.67°; control: SD = 9.16°). Accordingly, the Inclusion benchmark yielded shallower expected slopes than the Top-up approach.

Under the Top-up operationalization, the observed slopes in both groups differed significantly from the simulated benchmark (Table 2). Even if we restrict the analysis to only the aiming group when the PDP probes were at the Aiming Location without any delay, we find that the bootstrapped distribution of slopes still falls short of -1 (Supplementary Figure S4). However, under the alternative operationalization, where Inclusion trials serve as total adaptation and the benchmark is accordingly shallower, neither group's observed slope differed significantly from the simulated benchmark (Table 2). This divergence reflects a general statistical property: because different operationalizations of total adaptation carry different amounts of variance, the same underlying additive relationship can produce different expected slopes and therefore different conclusions about whether the observed data deviate from additivity (see 20). Regardless of which benchmark is used, both groups showed significant negative slopes, confirming that implicit and explicit processes trade off. Moreover, when compared on the same measures (Explicit: Inclusion − Exclusion; Implicit: aftereffect), the two groups did not differ in the strength of this relationship (mean difference = 0.125, 95% CI [−0.167, 0.423], p = 0.428). Whether this tradeoff reaches strict additivity depends on measurement choices.

DISCUSSION

Summary

Motor adaptation is not driven by a single, unitary process but instead emerges from multiple processes. Most taxonomies divide these learning and memory processes at the phenomenological level, with explicit and implicit processes at the top branch, with a variety of different subprocesses going down the branches (35, but see 36). Along the explicit branch, there are at least two distinct components that have been identified: algorithmic-based (e.g., mental rotation) and retrieval-based (e.g., stimulus-response caching) strategies (37, 38, 39). Along the implicit branch, a number of different components have been identified, such as implicit recalibration, use-dependent learning, proprioceptive recalibration, and reinforcement learning (40, 41, 42, 43, 32, 44). The variety and complexity of these underlying processes raises a fundamental question: how do they combine to produce overall adaptation? Many studies have assumed linear additivity at the top branch of this explicit-implicit taxonomy such that total adaptation is equal to the sum of explicit and implicit components, often estimating implicit as the difference between total and explicit adaptation (14, 21, 45, 46, 47, 5). Recently, ‘t Hart et al. (12) found no significant inverse relationship between explicit and implicit measures using both aim-report and PDP methods, which they interpreted as evidence against linear additivity. However, this interpretation has been questioned on statistical grounds: regression slopes reflect how learning processes covary across individuals, not whether they sum within individuals, and the -1 benchmark holds only when total adaptation is constant (20).

Our study addressed this question while explicitly accounting for two factors known to shape adaptation: plan-based generalization and temporal decay. We used a within-subject 2 × 2 design (Target Location × Delay) and obtained independent estimates of explicit and implicit adaptation, allowing us to separate a compositional test of additivity from an assessment of between-subject covariation. We evaluated additivity in two complementary ways. A direct test of strict additivity showed that the measured implicit and explicit components did not add up to total adaptation in either group, such that $D = A - (I + E)$ systematically deviated from zero. While failing strict additivity, slope analyses revealed robust inverse relationships between explicit and implicit adaptation across individuals in both groups, indicating reliable tradeoff between the processes. Within the aiming group, probing at the target aligned with participants’ reported aiming direction strengthened the inverse relationship, consistent with the idea that plan-based generalization can obscure implicit–explicit coupling when probes are spatially misaligned. Introducing a delay produced a modest attenuation of this tradeoff. Interestingly, the deviation D did not vary across Target Location or Delay, suggesting that the systematic departure from strict additivity is not uniquely driven by the spatial or temporal factors manipulated in the study. The pattern is consistent with partial additivity, in which explicit and implicit contributions covary in the expected direction but do not satisfy the unit-weight identity required for perfect summation.

It is important to distinguish between different forms of deviation from strict additivity. While both of our experimental groups failed to show strict additivity, this failure does not imply that the processes do not trade off. We found robust inverse relationships in both groups, indicating partial additivity — when one process is larger, the other tends to be smaller. In contrast, non-additivity would be indicated by the absence of any tradeoff: a slope near zero or positive between implicit and explicit measures, suggesting the processes do not compensate for one another. We emphasize that our findings speak to whether a tradeoff exists, not to the underlying mechanism generating that tradeoff—whether through independent processes filling slack or competitive processes sharing error signals remains an open question (13).

First, this could be simply due to the added complexity of the aim-report method causing confusion in the participants and, thus, affecting clean process measurement. Both the aiming report and the PDP method heavily depend on participants' understanding of the instructions (29). Second, given the number of learning processes that have been identified in recent years, it is likely that the aiming report and PDP method do not fully capture the contributions of these other processes. Finally, asking participants to explicitly report their aiming location or providing instructions to use or stop using compensatory strategies could fundamentally change the interaction between the myriad of explicit and implicit processes in potentially nonlinear ways.

Spatial and Temporal Dynamics Interactions

Failing to account for the effects of spatial and temporal dynamics on the relationship between explicit and implicit adaptation appeared to be a likely candidate for partial- or non-additivity, as it may have led to misinterpretations of a number of phenomena, such as spontaneous recovery, savings, and interference (22, 47, 21, 45, 48). While early work in motor adaptation assumed that adaptation would generalize around the target location (49, 50, 51, 52, 53, 54), studies of force field adaptation suggested that generalization might instead be centered on the actual movement trajectory (55). More recent studies of visuomotor rotations suggest that generalization occurs at the level of the movement plan, specifically at the aiming location rather than the target or the actual movement location (21, 22, 23). In studies with a significant contribution of explicit aiming, plan-based generalization would predict an apparent reduction in implicit adaptation if it were probed at the training target during washout blocks or with the PDP method. As a result, additivity tests that infer implicit as the difference between total and explicit adaptation could appear partial- or non-additive because the probe is spatially misaligned with where learning is expressed.

To address this potential effect of plan-based generalization, we directly measured participants' explicit aiming strategies and tailored our probe target to each individual's Aiming Location to get a more accurate assessment of the implicit process. Indeed, we found that a significantly stronger inverse relationship between implicit and explicit processes emerged when implicit adaptation was measured at the Aiming Location. However, this relationship still fell short of full additivity, suggesting that plan-based generalization alone could not account for the deviation.

Temporal decay is another possible factor that could complicate the relationship between explicit and implicit processes. While explicit strategies remain relatively stable, implicit recalibration decays over time: a volatile component that nearly vanishes within 1 minute and a persistent component that remains stable (24, 26). In studies where multiple targets were presented, implicit adaptation could potentially decay since it would take some time to get back to the same target (12). Although the overall effect of our delay manipulation on adaptation was not significant, trial-by-trial analysis revealed clear within-block decay (Supplementary Figure S5c), consistent with a volatile component that dissipates rapidly even over short intervals (24, 26). The long pre-first-trial ITI in our design (Supplementary Figure S1) likely allowed substantial volatile decay before the first probe, reducing our sensitivity to detect additional effects of the 1-minute delay.

Even after aligning where and when learning is probed, the relationship between explicit and implicit components appears more complex than a simple linear sum. The modest but persistent deviation from additivity suggests that spatial and temporal factors, while important, cannot fully explain the partially additive relationship between explicit and implicit adaptation. The missing difference likely reflects the complex nature of adaptation: (i) methodological limitations in how we measure these processes, (ii) the operation of multiple unaccounted-for processes, or (iii) genuine interactions between learning systems.

Current Methodological Limitations in Process Measurement

Our current methodologies fail to cleanly isolate explicit and implicit processes. Both the aim-report and PDP methods depend heavily on participants' understanding of the instructions (29), and even subtle differences in how these methods are implemented can yield divergent estimates of explicit and implicit contributions (8, 27). Our findings also highlight important methodological considerations that can obscure the apparent relationship. Specifically, the null result from t'Hart et al. (12) may stem from unclear instructions and an improper suboptimal measurement approach. In a pilot study, we found that participants' understanding of task instructions is critical for accurate dissociation of learning components (29). Without clear task comprehension, participants may conflate explicit and implicit processes or fail to engage them appropriately.

The aim-report procedure requires participants to introspect about their internal state and translate this into a verbalizable response, but the very act of questioning can alter what is being measured. When asked to report their "strategy," participants face inherent ambiguity. Consider a participant adapting to a 45° rotation. They may report aiming 35° opposite to the rotation when asked about their strategy, yet they may only voluntarily control 30°. The 35° represents their reportable explicit knowledge: what they believe their strategy to be or what they think they are doing (8). The 30° represents their controllable explicit knowledge: what they can actually implement or suppress voluntarily (8). This 5° discrepancy cannot be attributed to simple motor noise; it reflects a fundamental dissociation between what participants think they are doing and what they can actually control. Should they report what they believe they are doing (35°), what they can actually control (30°), their intended full compensation (45°), or what they think the experimenter expects? The critical issue is that participants themselves may not have conscious access to these distinctions, and the ambiguity confounds the relationship between implicit and explicit processes when individuals are asked to report their explicit strategies.

In contrast, the PDP method seeks to minimally interfere by asking participants to express or suppress their strategy. This binary test bypasses much of the ambiguity of reporting, since it probes what participants can suppress rather than what they can articulate. However, the method raises limitations when accounting for plan-based generalization, as probing adaptation at a new location introduces additional complexity and confusion (e.g., the participant has never trained at that new location). Moreover, the PDP approach cannot provide an independent measure of explicit adaptation, leading to measurement limitations that can obscure true relationships between processes. These measurement challenges call into question studies claiming to find strong relationships between explicit and implicit adaptation. Albert et al. (13) reported substantial correlations and proposed a competition model, but critically, they inferred both explicit and implicit components from the same behavioral output without independent dissociation. This approach necessarily guarantees correlations: when both measures derive from the same reaching behavior and one is calculated as the residual of the other, mathematical dependency can create artifactual relationships. The problem is compounded when plan-based generalization is ignored. If implicit adaptation is measured at the wrong spatial location, the explicit component calculated by subtraction will be overestimated, inflating correlations further. Without truly independent measures and proper spatial alignment, it becomes nearly impossible to determine whether observed correlations reflect genuine interactions or methodological artifacts.

Alternative experimental approaches have attempted to isolate specific components of adaptation. The error clamp method fixes the visual feedback at a constant angle regardless of hand movement, isolating implicit adaptation by eliminating trial-by-trial error correction that might drive explicit strategies (15, 16). Gradual versus abrupt perturbation schedules have been proposed to differentially engage implicit and explicit processes, with the assumption that gradual rotations minimize awareness and explicit strategies while abrupt rotations provoke strategic aiming (56, 57, 58). Feedback delay manipulations, which introduce temporal gaps between movement and cursor feedback, selectively disrupt implicit adaptation while leaving explicit strategies largely intact, suggesting these processes have different temporal constraints (59, 60). Finally, imposing a restriction on preparation time has been shown to blunt the application of an explicit strategy (61); however, it does not prevent strategies from being developed between trials nor does it offer a means to independently measure explicit or implicit processes without relying on another method, such as PDP or aim reports.

Each of these methods seeks to privilege one process over another, with the hope that isolated components could be measured independently and then summed to reconstruct total adaptation. However, this isolation strategy encounters a fundamental problem: while these methods may successfully isolate processes at the group level, they necessarily eliminate individual differences in how these processes combine within a single person. For instance, one may utilize a more explicit strategy than implicit recalibration compared to another. At present, there is no clean method for measuring explicit and implicit adaptation that avoids either some degree of measurement interference or requires generous inferential leaps about how isolated components relate to their interaction in natural learning contexts. Alas, there appears to be something akin to an Observer Effect where we cannot measure one process without (potentially) affecting or, at least, obscuring the other process.

Multiple Implicit and Explicit Processes

Given the number of learning processes that have been identified in recent years, it is likely that the aim-report and PDP methods do not fully capture contributions of these other processes. Beyond the broad implicit versus explicit split, multiple subprocesses likely contribute within each system. On the implicit side, use-dependent learning creates repetition-induced biases that pull movements toward recently executed directions, even when error is minimized (62, 43). Proprioceptive recalibration shifts perceived hand position and the mapping between visual and proprioceptive estimates (40, 41). In addition, recent work proposes a distinct implicit component, implicit aiming, that biases action selection rather than execution per se and shows properties different from cerebellar-dependent recalibration (e.g., distinct temporal stability and contextual modulation), suggesting separable computations within implicit learning (36).

Similarly, the explicit "system" likely encompasses multiple distinct processes rather than a single unitary strategy. Algorithmic strategies involve deliberate mental rotation and geometric calculation of aiming angles, requiring working memory and executive control (38). In contrast, retrieval-based strategies involve caching and recalling successful stimulus-response associations from previous trials, more akin to declarative memory retrieval than online computation (39). These two forms of explicit adaptation may have different dynamics, different capacity limitations, and potentially different relationships with implicit processes. A participant might simultaneously engage in both algorithmic and retrieval strategies, with their relative contributions shifting across trials. They might also employ higher-order rules about when to use algorithmic versus retrieval approaches. The fact that we collapse all of these controlled, deliberate processes under a single "explicit" estimate necessarily creates measurement error and could contribute to apparent partial additivity.

The multiple subprocesses outlined here highlight, once again, the methodological limits of isolating distinct mechanisms within adaptation. The partial additivity we observe could reflect contributions from a mixture of processes and other components that were not isolated by our measures. Without independent assays for each subprocess, it remains unclear whether their contributions are additive or whether interactions among them produce differences from additivity.

Higher-order Interactions between Learning Systems

Beyond the complexity introduced by multiple subprocesses, these systems at a more global level can potentially engage in dynamic interactions that lead to partial additivity between the two systems. Albert et al. (13) proposed that when explicit and implicit systems are driven by a common error signal, they compete rather than cooperate. Their competition model suggests that as explicit strategies increase to reduce target error, they simultaneously suppress implicit adaptation, not through simple subtraction but through inhibition. When one system, particularly the explicit system, becomes more active in correcting the error, it reduces the error signal available to the other system, thereby influencing its learning trajectory and observed behavior.

This competitive dynamic parallels findings from other learning domains. Collins and Frank (63) showed that working memory (WM) and reinforcement learning (RL) systems interact in complex ways during instrumental learning. When WM successfully maintains stimulus-response mappings and successfully guides action selection, it reduces the signal available for the RL system. Conversely, when WM capacity is exceeded, RL must compensate, but does so less efficiently. This creates a fundamental tension where WM and RL dynamically share responsibility for action selection. Their relative contributions are weighted based on their estimated reliability or value, with the more reliable system having a greater influence on behavior at any given time. Working memory gates what gets learned through reinforcement, while RL signals influence what gets maintained in working memory.

Similar system-level interactions emerge between the hippocampus and basal ganglia, traditionally viewed as supporting independent declarative and procedural memory systems. Shohamy and colleagues have demonstrated that these systems engage in both competitive and cooperative dynamics depending on task demands (64, 65). During probabilistic learning, the hippocampus can actually interfere with basal ganglia-dependent habit formation by imposing explicit rules that override incremental statistical learning. In contrast, strong engagement of procedural systems can suppress hippocampal encoding of episodic details. These interactions are mediated by shared neuromodulatory signals, particularly dopamine projections from the midbrain that simultaneously influence both systems but with opposite effects depending on the specific receptors and circuits involved.

If implicit and explicit systems compete for error signals (13), share dynamic control based on their capacity (63), and receive conflicting neuromodulatory signals (64), then their contributions cannot simply be summed. Instead, engaging one system necessarily changes how the other operates. People with stronger working memory might show weaker implicit adaptation because their cognitive control systems more effectively suppress automatic recalibration (66, 67). Such interactions could produce the pattern of partial additivity we observe — a compensatory tradeoff that nonetheless falls short of strict summation.

Challenges with Current Taxonomic Frameworks

The persistent challenges in demonstrating additivity highlight a more fundamental issue: we are trying to measure and categorize learning processes using taxonomic frameworks that may not align with how these processes are actually organized. The explicit-implicit distinction has a long historical tradition, originating from studies of patient H.M. and other cases of amnesia that revealed dissociations between conscious recollection and preserved motor skills (2, 35). However, this distinction was originally developed for memory systems, not necessarily for the processes underlying motor adaptation. As studies have revealed increasing complexity within motor adaptation, multiple implicit/explicit processes and potential interactions between them, alternative frameworks have been proposed to better capture the underlying adaptation mechanisms. Yet, there remains no consensus on which framework best characterizes the space of learning processes.

One proposed alternative focuses on controllability rather than explicit awareness or knowledge. As Maresch and colleagues (8) proposed that we should differentiate between reportable explicit knowledge (the ability to describe the strategy being applied) and controllable explicit knowledge (the ability to deliberately choose to implement it). These reflect different aspects of conscious motor control and should not be treated as interchangeable. Reconceptualizing the framework around controllable versus uncontrollable components offers certain advantages. It avoids some of the ambiguity between explicit and implicit by focusing on what participants can actually modulate in behavior rather than what they can verbalize. This distinction may align better with experimental manipulations like the PDP, which operationalizes implicit learning as what persists when participants try to suppress their learned behavior.

Another alternative framework organizes processes by their goals: action selection versus action execution (36). This framework distinguishes between processes that determine which action to perform – where to move – in order to achieve a task goal (action selection) versus processes that ensure the selected movement is accurately implemented (action execution). Critically, action execution is hypothesized to be intrinsically implicit, operating in an automatic, obligatory manner (e.g., implicit recalibration, use-dependent learning at the execution level). In contrast, action selection can engage both explicit processes (e.g., deliberate strategic aiming) and implicit processes (e.g., implicit aiming, habits). This framework offers explanatory power for previously conflicting findings. Processes like implicit aiming that contribute to action selection show savings and contextual sensitivity, while processes like implicit recalibration that contribute to action execution show anti-savings and are context-insensitive. These distinct patterns reflect fundamentally different computational goals – flexible selection to optimize task performance versus automatic execution to maintain calibration – rather than simply different levels of explicit awareness. This proposed action selection and execution framework shares some conceptual overlap with the controllable–uncontrollable framework, but they place the distinction at different levels and viewpoints: the former emphasizes the goal of what is being learned (selection vs. execution), whereas the latter emphasizes how that learning is expressed or modulated.

At present, there is no consensus on the optimal taxonomic framework for characterizing motor adaptation. The explicit-implicit distinction remains the most commonly used framework, partly due to historical precedent and the availability of measurement techniques designed around this dichotomy. But any framework will begin with different assumptions about what matters most, which in turn shapes the experimental manipulations employed and how the results are interpreted. Future work will need to develop conceptual frameworks and measurement approaches that can better capture the multi-dimensionality and complexity of motor learning processes.

Assumptions and Limitations of Additivity Testing

Our tests of linear additivity rest on several assumptions that warrant explicit consideration. First the residual test ($D = A - (I + E)$) requires that all three measures reflect the same underlying state. However, total adaptation (Top-up trials) was measured with feedback, while implicit (aftereffect) and explicit (Inclusion - Exclusion) measures were obtained during no-feedback phases. If implicit or explicit components changed between these measurement points, due to temporal decay, instructional effects, or context-dependent expression, then $D \neq 0$ could reflect temporal instability rather than true non-additivity. The consistent offset between exclusion and aftereffect measures (Supplementary Figure S5c) illustrates this challenge. Our design attempted to minimize this by using Top-up trials temporally proximal to the PDP probes while maintaining adaptation, but some mismatch may be unavoidable.

Second, the expected slope under additivity depends critically on how total adaptation is operationalized. As our simulation benchmarks demonstrated, using Top-up trials versus Inclusion trials as total adaptation yielded different expected slopes, and consequently different conclusions about whether the observed data deviate from strict additivity. This sensitivity underscores that the -1 benchmark is not universal but depends on the variance structure of the specific measures used (see 20). We presented simulations under both operationalizations to allow readers to evaluate our findings under different assumptions. Regardless of operationalization, both groups showed significant negative slopes, confirming that implicit and explicit processes trade off. Whether this tradeoff reaches strict additivity or reflects partial additivity depends on measurement choices.

Lastly, our explicit measure (Inclusion - Exclusion) is not fully independent of the implicit component measured at probe time. While our primary implicit measure (aftereffect) is independent of both Inclusion and Exclusion trials, the explicit measure inherits variance from both trial types. This coupling could introduce systematic biases in the slope estimate.

Conclusion

While strict additivity fails — implicit and explicit measures do not perfectly sum to total adaptation — the two processes nonetheless trade off in a compensatory manner, consistent with partial additivity. This pattern is robust across both measurement approaches and both experimental groups. In the aiming group, aligning the probe location with participants' actual aiming direction and minimizing delay strengthened the observed tradeoff, confirming that spatial and temporal factors affect the detectability of the implicit-explicit relationship. However, the systematic deviation from strict additivity was consistent across conditions, suggesting it reflects factors beyond plan-based generalization and temporal decay. Whether the remaining gap stems from methodological imprecision, the operation of additional learning processes that combine in ways not captured by current methods, or genuinely nonlinear interactions between learning systems remains an open question. Resolving this will require novel methodologies that can independently assay the multiplicity of learning processes to determine whether they truly add up.

SUPPLEMENTAL MATERIAL

Supplementary figures and supporting statistical information are available at: https://doi.org/10.17605/OSF.IO/ZT8PD.

ACKNOWLEDGEMENT

We thank the members of the Intelligent Performance and Adaptation Laboratory (IPA Lab) in the Department of Psychology at Princeton University for their insightful feedback, especially Yiyu Wang and Chandra Greenberg. The research reported in this manuscript was supported by grants R01NS131552 and R01NS134754 (awarded to JAT) from the National Institute of Neurological Disorders and Stroke (NINDS) of the National Institutes of Health (NIH). The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the findings.

Reference

1. Shadmehr, R., Brandt, J., & Corkin, S. (1998). Time-dependent motor memory processes in amnesic subjects. Journal of Neurophysiology, 80(3), 1590-1597.

2. Corkin, S. (1968). Acquisition of motor skill after bilateral medial temporal-lobe excision. Neuropsychologia, 6(3), 255-265.

3. Hegele, M., & Heuer, H. (2010). Implicit and explicit components of dual adaptation to visuomotor rotations. Consciousness and Cognition, 19(4), 906-917.

4. Benson, B. L., Anguera, J. A., & Seidler, R. D. (2011). A spatial explicit strategy reduces error but interferes with sensorimotor adaptation. Journal of Neurophysiology, 105(6), 2843-2851.

5. Taylor, J. A., Krakauer, J. W., & Ivry, R. B. (2014). Explicit and implicit contributions to learning in a sensorimotor adaptation task. Journal of Neuroscience, 34(8), 3023-3032.

6. Werner, S., van Aken, B. C., Hulst, T., Frens, M. A., van der Geest, J. N., Struder, H. K., & Donchin, O. (2015). Awareness of sensorimotor adaptation to visual rotations of different size. PLoS ONE, 10(4), e0123321.

7. Gastrock, R. Q., Modchalingam, S., 't Hart, B. M., & Henriques, D. Y. P. (2020). External error attribution dampens efferent-based predictions but not proprioceptive changes in hand localization. Scientific Reports, 10(1), 19918.

8. Maresch, J., Werner, S., & Donchin, O. (2021). Methods matter: Your measures of explicit and implicit processes in visuomotor adaptation affect your results. European Journal of Neuroscience, 53(2), 504-518.

9. Modchalingam, S., Vachon, C. M., 't Hart, B. M., & Henriques, D. Y. P. (2019). The effects of awareness of the perturbation during motor adaptation on hand localization. PLoS ONE, 14(8), e0220884.

10. Vachon, C. M., Modchalingam, S., 't Hart, B. M., & Henriques, D. Y. P. (2020). The effect of age on visuomotor learning processes. PLoS ONE, 15(9), e0239032.

11. Jacoby, L. L. (1991). A process dissociation framework: Separating automatic from intentional uses of memory. Journal of Memory and Language, 30(5), 513-541.

12. 't Hart, B. M., Taqvi, U., Gastrock, R. Q., Ruttle, J. E., Modchalingam, S., & Henriques, D. Y. P. (2024). Measures of implicit and explicit adaptation do not linearly add. eNeuro, 11(8), ENEURO.0021-23.2024.

13. Albert, S. T., Jang, J., Modchalingam, S., 't Hart, B. M., Henriques, D., Lerner, G., Della-Maggiore, V., Haith, A. M., Krakauer, J. W., & Shadmehr, R. (2022). Competition between parallel sensorimotor learning systems. eLife, 11, e74066.

14. Bond, K. M., & Taylor, J. A. (2015). Flexible explicit but rigid implicit learning in a visuomotor adaptation task. Journal of Neurophysiology, 113(10), 3836-3849.

15. Morehead, J. R., Taylor, J. A., Parvin, D. E., & Ivry, R. B. (2017). Characteristics of implicit sensorimotor adaptation revealed by task-irrelevant clamped feedback. Journal of Cognitive Neuroscience, 29(6), 1061-1074.

16. Kim, H. E., Morehead, J. R., Parvin, D. E., Moazzezi, R., & Ivry, R. B. (2018). Invariant errors reveal limitations in motor correction rather than constraints on error sensitivity. Communications Biology, 1, 19.

17. Butcher, P. A., & Taylor, J. A. (2018). Decomposition of a sensory prediction error signal for visuomotor adaptation. Journal of Experimental Psychology: Human Perception and Performance, 44(2), 176-194.

18. Miyamoto, Y. R., Wang, S., & Smith, M. A. (2020). Implicit adaptation compensates for erratic explicit strategy in human motor learning. Nature Neuroscience, 23, 443-455.

19. Taylor, J. A., & Ivry, R. B. (2011). Flexible cognitive strategies during motor learning. PLoS Computational Biology, 7(3), e1001096.

20. Liddy, J. J. (2025). Mistaking covariance for combination in sensorimotor adaptation: Regression slopes do not test additivity [Preprint]. PsyArXiv. https://doi.org/10.31234/osf.io/4ur6x_v3

21. Day, K. A., Roemmich, R. T., Taylor, J. A., & Bastian, A. J. (2016). Visuomotor learning generalizes around the intended movement. eNeuro, 3(2), ENEURO.0005-16.2016.

22. McDougle, S. D., Bond, K. M., & Taylor, J. A. (2017). Implications of plan-based generalization in sensorimotor adaptation. Journal of Neurophysiology, 118(1), 383-393.

23. Poh, E., & Taylor, J. A. (2019). Generalization via superposition: Combined effects of mixed reference frame representations for explicit and implicit learning in visuomotor adaptation. Journal of Neurophysiology, 122(4), 1628-1640.

24. Hadjiosif, A. M., Morehead, J. R., & Smith, M. A. (2023). A double dissociation between savings and long-term memory in motor learning. PLoS Biology, 21(4), e3001799.

25. Hadjiosif, A. M., Abraham, G., Ranjan, T., & Smith, M. A. (2024). Subtle visual latency can profoundly impair implicit sensorimotor learning [Preprint]. bioRxiv. https://doi.org/10.1101/2024.03.14.585093

26. Zhou, W., Fitzgerald, J., Colucci-Chang, K., Murthy, K. G., & Joiner, W. M. (2017). The temporal stability of visuomotor adaptation generalization. Journal of Neurophysiology, 118(4), 2435-2449.

27. Hadjiosif, A. M., & Krakauer, J. W. (2021). The explicit/implicit distinction in studies of visuomotor learning: Conceptual and methodological pitfalls. European Journal of Neuroscience, 53(2), 499-503.

28. Brainard, D. H. (1997). The Psychophysics Toolbox. Spatial Vision, 10(4), 433-436.

29. Chen, Y., & Taylor, J. A. (2025). Spatial dynamics shape the interaction between motor adaptation processes. Proceedings of the Annual Meeting of the Cognitive Science Society, 47(0).

30. Mazzoni, P., & Krakauer, J. W. (2006). An implicit plan overrides an explicit strategy during visuomotor adaptation. Journal of Neuroscience, 26(14), 3642-3645.

31. Taylor, J. A., Klemfuss, N. M., & Ivry, R. B. (2010). An explicit strategy prevails when the cerebellum fails to compute movement errors. Cerebellum, 9(4), 580-586.

32. Kim, H. E., Parvin, D. E., & Ivry, R. B. (2019). The influence of task outcome on implicit motor learning. eLife, 8, e39882.

33. Wilterson, S. A., & Taylor, J. A. (2021). Implicit visuomotor adaptation remains limited after several days of training. eNeuro, 8(4), ENEURO.0312-20.2021.

34. Tsay, J. S., Kim, H. E., Parvin, D. E., Stover, A. R., & Ivry, R. B. (2021). Individual differences in proprioception predict the extent of implicit sensorimotor adaptation. Journal of Neurophysiology, 125(4), 1307-1321.

35. Squire, L. R. (2004). Memory systems of the brain: A brief history and current perspective. Neurobiology of Learning and Memory, 82(3), 171-177.

36. Wang, T., Lam, T., Taylor, J. A., & Ivry, R. B. (2025). Distinct implicit contributions to action selection and action execution in sensorimotor adaptation [Preprint]. bioRxiv. https://doi.org/10.1101/2025.09.12.675892

37. Pellizzer, G., & Georgopoulos, A. P. (1993). Mental rotation of the intended direction of movement. Current Directions in Psychological Science, 2(1), 12-17.

38. McDougle, S. D., & Taylor, J. A. (2019). Dissociable cognitive strategies for sensorimotor learning. Nature Communications, 10(1), 40.

39. Velazquez-Vargas, C. A., & Taylor, J. A. (2024). Working memory constraints for visuomotor retrieval strategies. Journal of Neurophysiology, 132(2), 347-361.

40. Cressman, E. K., & Henriques, D. Y. P. (2009). Sensory recalibration of hand position following visuomotor adaptation. Journal of Neurophysiology, 102(6), 3505-3518.

41. Ostry, D. J., Darainy, M., Mattar, A. A. G., Wong, J., & Gribble, P. L. (2010). Somatosensory plasticity and motor learning. Journal of Neuroscience, 30(15), 5384-5393.

42. Izawa, J., & Shadmehr, R. (2011). Learning from sensory and reward prediction errors during motor adaptation. PLoS Computational Biology, 7(3), e1002012.

43. Huang, V. S., Haith, A., Mazzoni, P., & Krakauer, J. W. (2011). Rethinking motor learning and savings in adaptation paradigms: Model-free memory for successful actions combines with internal models. Neuron, 70(4), 787-801.

44. Tsay, J. S., Kim, H., Haith, A. M., & Ivry, R. B. (2022). Understanding implicit sensorimotor adaptation as a process of proprioceptive re-alignment. eLife, 11, e76639.

45. McDougle, S. D., Bond, K. M., & Taylor, J. A. (2015). Explicit and implicit processes constitute the fast and slow processes of sensorimotor learning. Journal of Neuroscience, 35(26), 9568-9579.

46. McDougle, S. D., Boggess, M. J., Crossley, M. J., Parvin, D., Ivry, R. B., & Taylor, J. A. (2016). Credit assignment in movement-dependent feedback generalization. Proceedings of the National Academy of Sciences of the United States of America, 113(24), 6797-6802.

47. Morehead, J. R., Qasim, S. E., Crossley, M. J., & Ivry, R. B. (2015). Savings upon re-aiming in visuomotor adaptation. Journal of Neuroscience, 35(42), 14386-14396.

48. Schween, R., Taylor, J. A., & Hegele, M. (2018). Plan-based generalization shapes local implicit adaptation to opposing visuomotor transformations. Journal of Neurophysiology, 120(6), 2775-2787.

49. Pine, Z. M., Krakauer, J. W., Gordon, J., & Ghez, C. (1996). Learning of scaling factors and reference axes for reaching movements. Neuroreport, 7(14), 2357-2361.

50. Pearson, T. S., Krakauer, J. W., & Mazzoni, P. (2010). Learning not to generalize: Modular adaptation of visuomotor gain. Journal of Neurophysiology, 103(6), 2938-2952.

51. Brayanov, J. B., Press, D. Z., & Smith, M. A. (2012). Motor memory is encoded as a gain-field combination of intrinsic and extrinsic action representations. Journal of Neuroscience, 32(43), 14951-14965.

52. Taylor, J. A., Hieber, L. L., & Ivry, R. B. (2013). Feedback-dependent generalization. Journal of Neurophysiology, 109(1), 202-215.

53. Krakauer, J. W., Pine, Z. M., Ghilardi, M. F., & Ghez, C. (2000). Learning of visuomotor transformations for vectorial planning of reaching trajectories. Journal of Neuroscience, 20(23), 8916-8924.

54. Tanaka, H., Krakauer, J., & Sejnowski, T. (2012). Generalization and multirate models of motor adaptation. Neural Computation, 24(4), 939-966.

55. Gonzalez Castro, L. N., Monsen, C. B., & Smith, M. A. (2011). The binding of learning to action in motor adaptation. PLoS Computational Biology, 7(6), e1002052.

56. Saijo, N., & Gomi, H. (2010). Multiple motor learning strategies in visuomotor rotation. PLoS ONE, 5(2), e9399.

57. Fernandez-Ruiz, J., Wong, W., Armstrong, I. T., & Flanagan, J. R. (2011). Relation between reaction time and reach errors during visuomotor adaptation. Behavioural Brain Research, 219(1), 8-14.

58. Kagerer, F. A., Contreras-Vidal, J. L., & Stelmach, G. E. (1997). Adaptation to gradual as compared with sudden visuo-motor distortions. Experimental Brain Research, 115(3), 557-561.

59. Brudner, S. N., Kethidi, N., Graeupner, D., Ivry, R. B., & Taylor, J. A. (2016). Delayed feedback during sensorimotor learning selectively disrupts adaptation but not strategy use. Journal of Neurophysiology, 115(3), 1499-1511.

60. Schween, R., & Hegele, M. (2017). Feedback delay attenuates implicit but facilitates explicit adjustments to a visuomotor rotation. Neurobiology of Learning and Memory, 140, 124-133.

61. Haith, A. M., Huberdeau, D. M., & Krakauer, J. W. (2015). The influence of movement preparation time on the expression of visuomotor learning and savings. Journal of Neuroscience, 35(13), 5109-5117.

62. Diedrichsen, J., White, O., Newman, D., & Lally, N. (2010). Use-dependent and error-based learning of motor behaviors. Journal of Neuroscience, 30(15), 5159-5166.

63. Collins, A. G. E., & Frank, M. J. (2012). How much of reinforcement learning is working memory, not reinforcement learning? A behavioral, computational, and neurogenetic analysis. European Journal of Neuroscience, 35(7), 1024-1035.

64. Shohamy, D., & Adcock, R. A. (2010). Dopamine and adaptive memory. Trends in Cognitive Sciences, 14(10), 464-472.

65. Foerde, K., & Shohamy, D. (2011). The role of the basal ganglia in learning and memory: Insight from Parkinson's disease. Neurobiology of Learning and Memory, 96(4), 624-636.

66. Anguera, J. A., Reuter-Lorenz, P. A., Willingham, D. T., & Seidler, R. D. (2010). Contributions of spatial working memory to visuomotor learning. Journal of Cognitive Neuroscience, 22(9), 1917-1930.

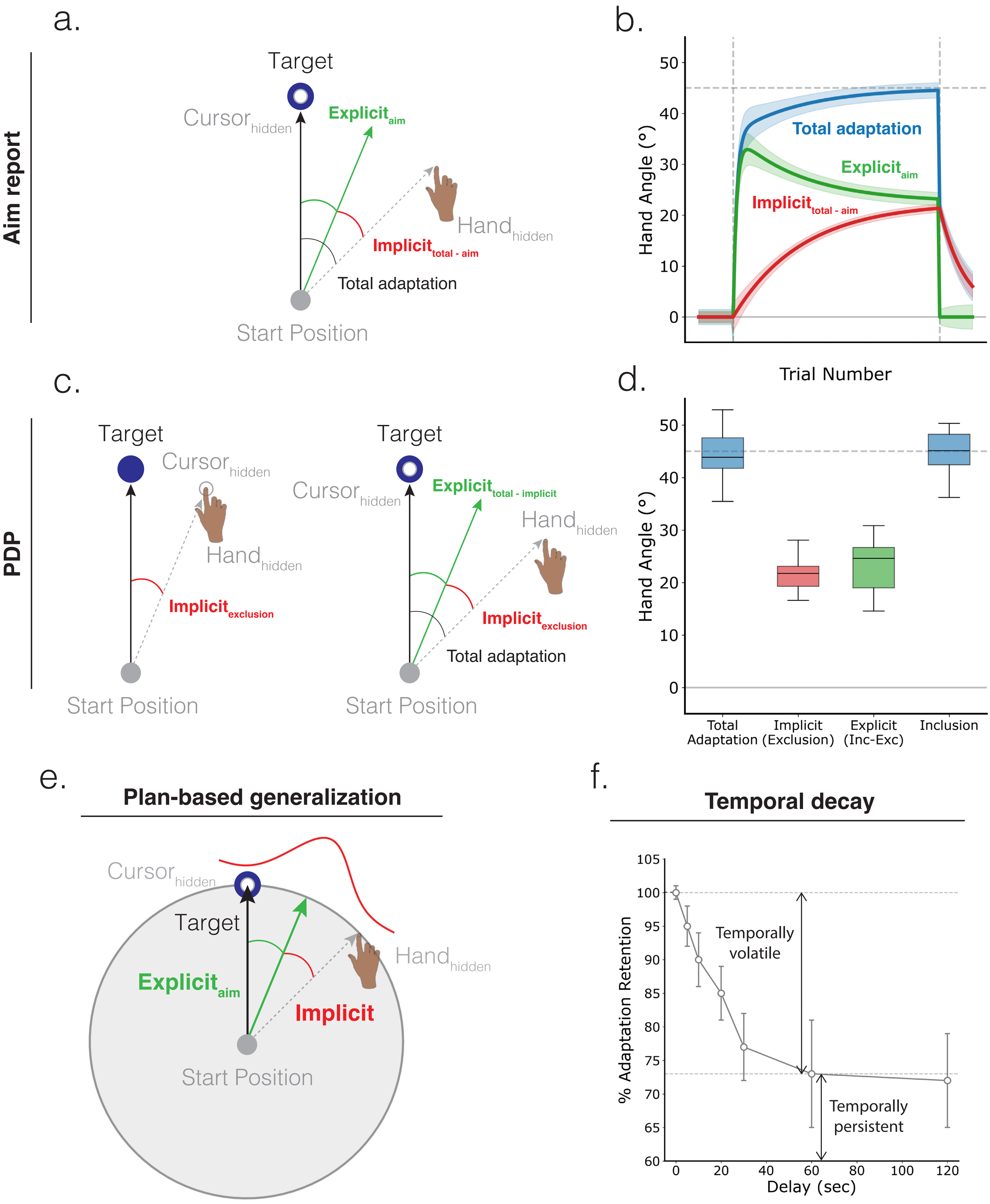
67. Christou, A. I., Miall, R. C., McNab, F., & Galea, J. M. (2016). Individual differences in explicit and implicit visuomotor learning and working memory capacity. Scientific Reports, 6(1), 36633.

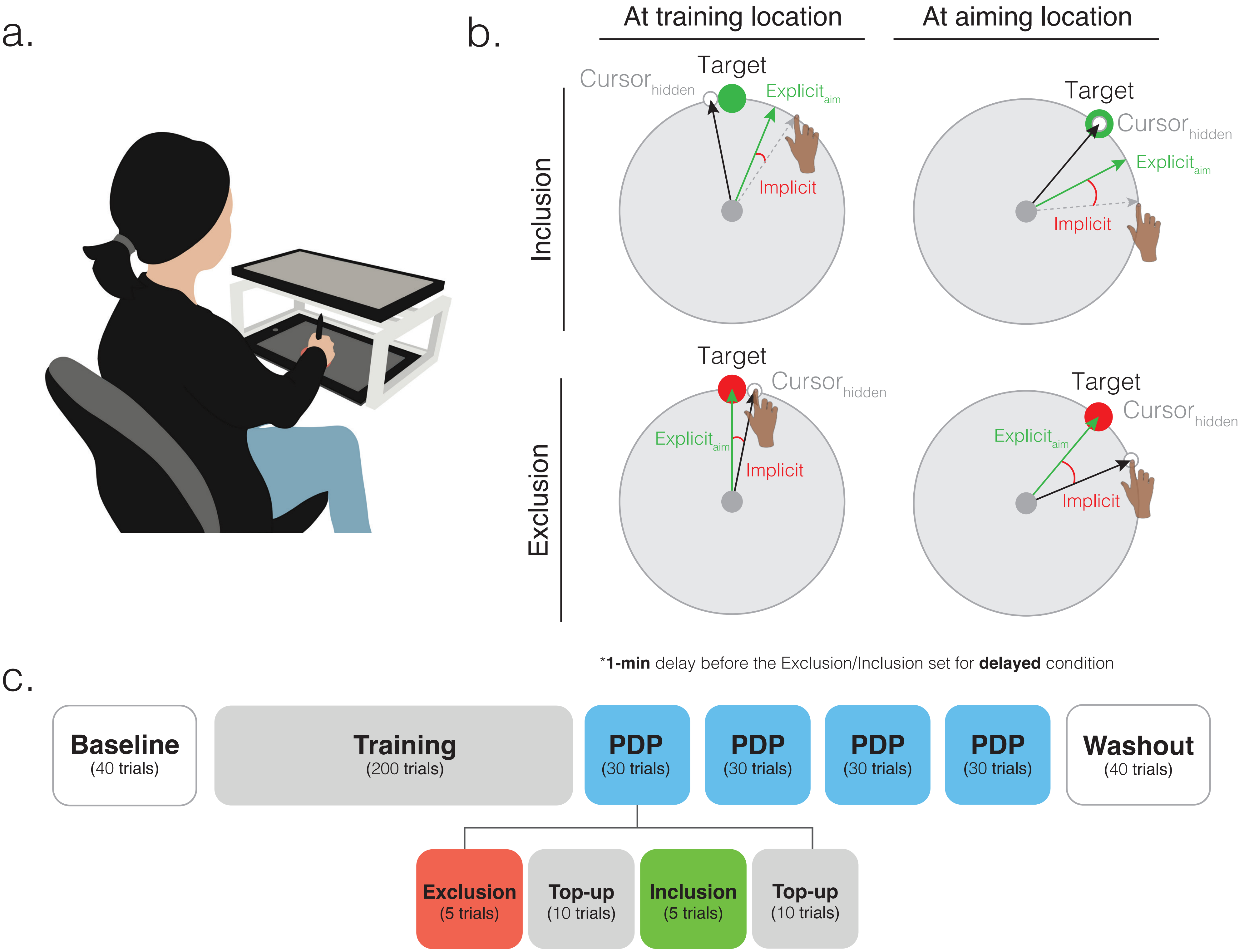
Figure 1. Different paradigms are used to dissociate implicit and explicit processes and how plan-based generalization and temporal decay may affect the relationships between implicit recalibration and explicit strategy. Aim-report paradigm: (**a**) Explicit adaptation measured via movement intention reports; implicit adaptation calculated as total adaptation minus reported aim. (**b**) Provides trial-by-trial component measures. PDP paradigm: (**c**) Inclusion trials measure total adaptation while Exclusion trials isolate implicit adaptation through strategy suppression; (**d**) Explicit adaptation is derived from differences between Inclusion and Exclusion. (**e**) Plan-based generalization: Implicit adaptation peaks at the aiming location rather than the target location. (**f**) Temporal decay: Adaptation exhibits two components: a volatile component that nearly vanishes, and a persistent component that remains after 30 to 120s delay (recreated from Zhou et al. 2017).

Figure 2. Experiment setup and design. (**a**) Participants performed fast-reaching movements by sliding a stylus on a digital tablet positioned under the monitor, which presented the visual workspace. (**b**) Example schematic of Inclusion trials (top) and Exclusion trials (bottom) for the target at the training location (left) and at the aiming location (right). (**c**) Experiment time course. The study employed a 2×2 within-subject design (Target Location: at training or at aiming locations × Delay: 1-min delay or no delay), resulting in four Process Dissociation Procedure (PDP) blocks.

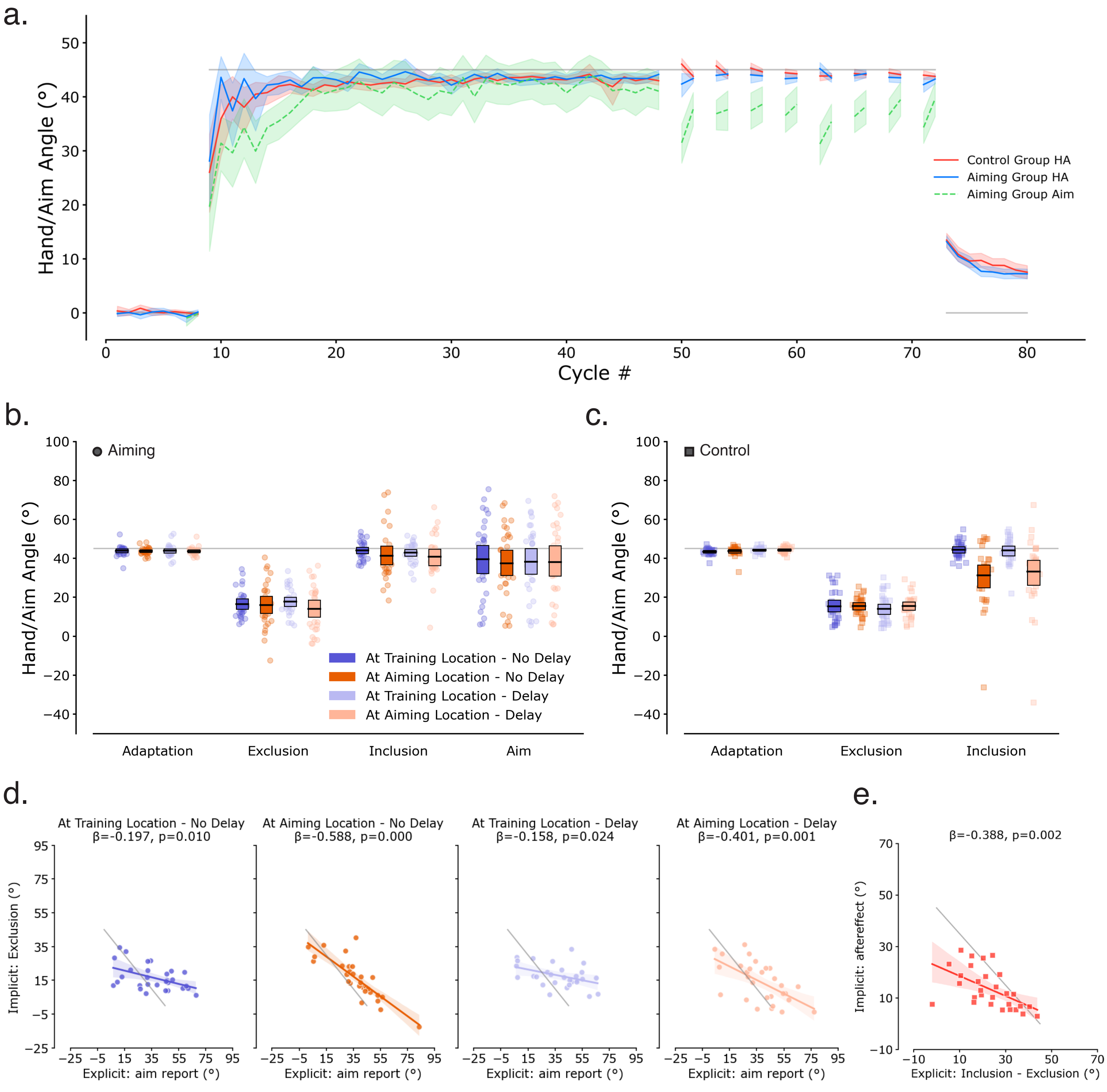
Figure 3. Significant but partially additive relationships between explicit and implicit adaptation. (**a**) Time course of hand angle and aim report across trials. Solid lines show hand angle for aiming (blue, $n = 29$) and control (red, $n = 29$) groups; dashed green line shows aim report for the aiming group; grey line indicates the imposed rotation. Shaded regions represent 95% bootstrap CIs. (**b, c**) Hand angles during Top-up, Exclusion, and Inclusion trial blocks for (b) the aiming group with aim reports and (c) the control group without aim reports. Boxes indicate 95% CIs; dots show individual participants (circles: aiming group; squares: control group). (**d, e**) Relationship between explicit and implicit adaptation for (d) the aiming group across the 2×2 factorial design (Target Location × Delay) and (e) the control group collapsed across conditions. Colored lines show regression fits with the significance of the slopes (β) indicated. Grey diagonal lines represent the predicted inverse relationship under perfect linear additivity, where explicit + implicit = total adaptation.

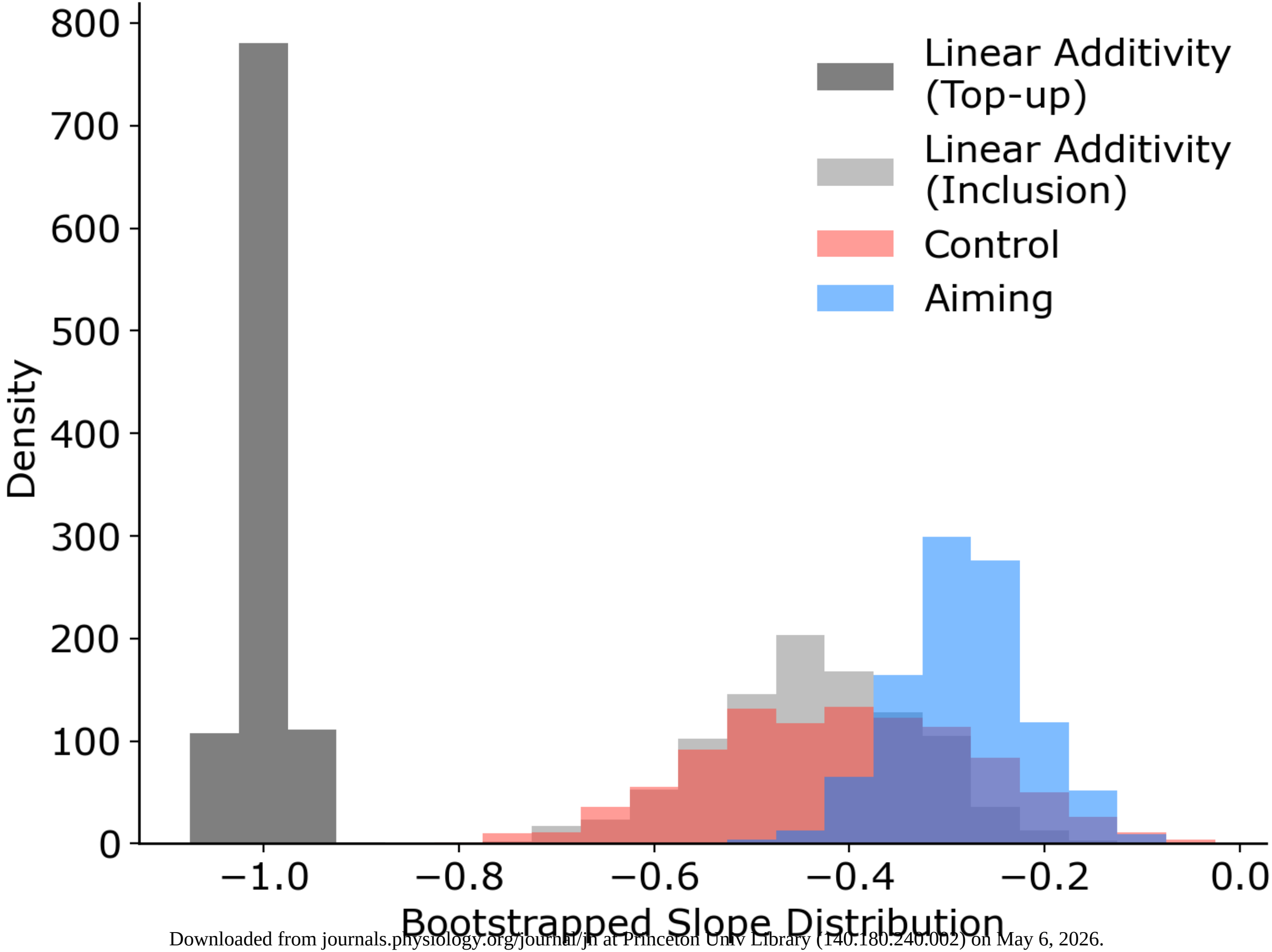
Figure 4. Distribution of bootstrapped slopes in aiming and control groups vs the linear additivity model. Colors denote different groups. Dark gray = linear additivity (Top-up as total adaptation); light gray = linear additivity (Inclusion as total adaptation); blue = aiming group; red = control group.


a.
Aim report
Target
Cursor hidden
Explicit aim
Implicit total - aim
Hand hidden
Total adaptation
Start Position
b.
Hand Angle (°)
50
40
30
20
10
0
Total adaptation
Explicit aim
Implicit total - aim
Trial Number
c.
PDP
Target
Cursor hidden
Hand hidden
Implicit exclusion
Start Position
Target
Cursor hidden
Explicit total - implicit
Hand hidden
Implicit exclusion
Total adaptation
Start Position
d.
Hand Angle (°)
50
40
30
20
10
0
Total Adaptation
Implicit (Exclusion)
Explicit (Inc-Exc)
Inclusion
e.
Plan-based generalization
Cursor hidden
Target
Explicit aim
Implicit
Hand hidden
Start Position
f.
Temporal decay
% Adaptation Retention
105
100
95
90
85
80
75
70
65
60
Temporally volatile
Temporally persistent
0
20
40
60
80
100
120
Delay (sec)



*__1-min__ delay before the Exclusion/Inclusion set for **delayed** condition


a.
Hand/Aim Angle (°)
Cycle #
0
10
20
30
40
50
60
70
80
Control Group HA
Aiming Group HA
Aiming Group Aim
b.
● Aiming
Hand/Aim Angle (°)
100
80
60
40
20
0
−20
−40
At Training Location - No Delay
At Aiming Location - No Delay
At Training Location - Delay
At Aiming Location - Delay
Adaptation
Exclusion
Inclusion
Aim
c.
■ Control
Hand/Aim Angle (°)
Adaptation
Exclusion
Inclusion
d.
At Training Location - No Delay
β=-0.197, p=0.010
At Aiming Location - No Delay
β=-0.588, p=0.000
At Training Location - Delay
β=-0.158, p=0.024
At Aiming Location - Delay
β=-0.401, p=0.001
Implicit: Exclusion (°)
95
75
55
35
15
−5
−25
Explicit: aim report (°)
−25
−5
15
35
55
75
95
e.
β=-0.388, p=0.002
Implicit: aftereffect (°)
70
50
30
10
−10
Explicit: Inclusion - Exclusion (°)
−10
10
30
50
70

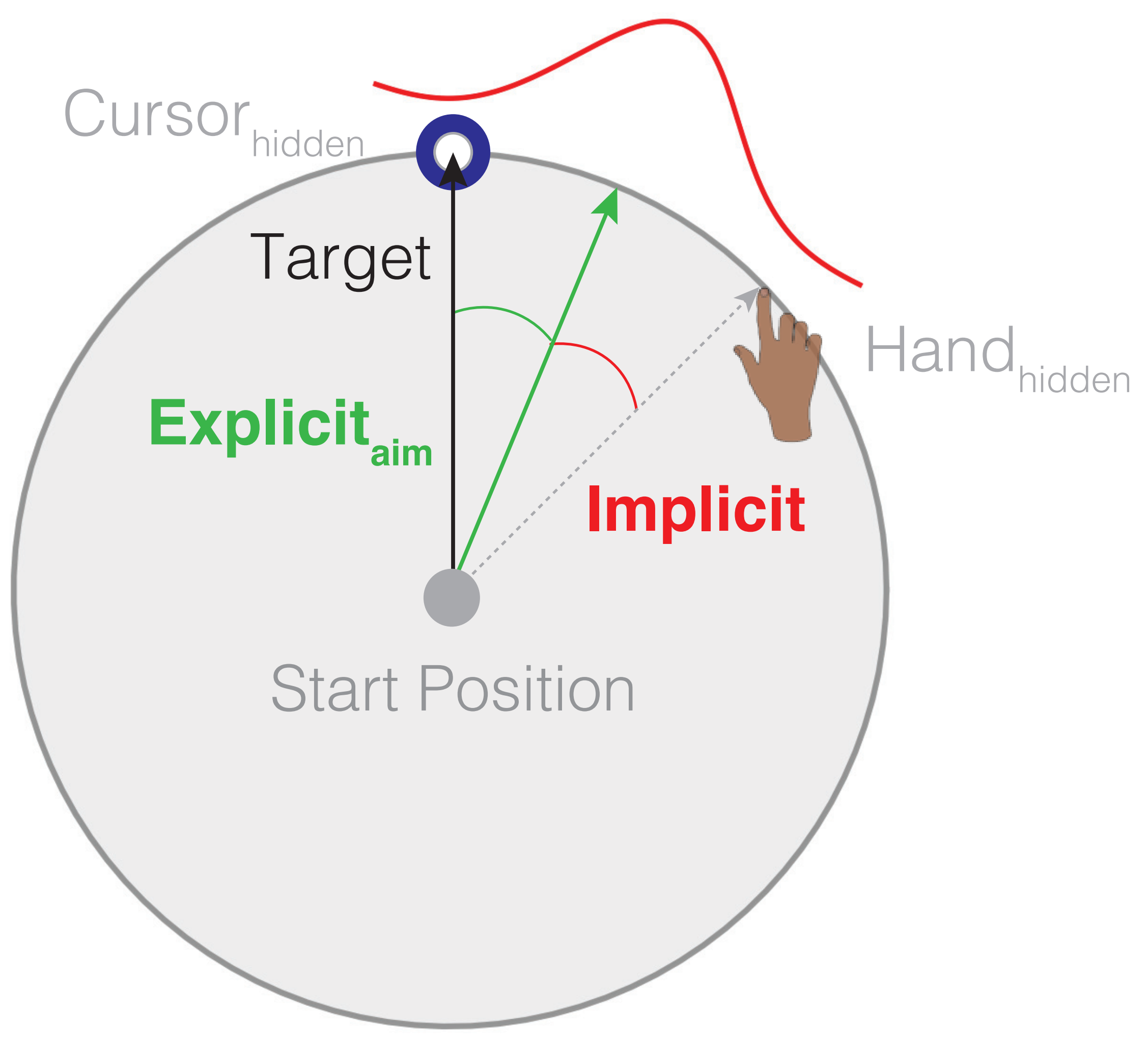

Linear Additivity (Top-up)
Linear Additivity (Inclusion)
Control
Aiming
Density
800
700
600
500
400
300
200
100
0
−1.0
−0.8
−0.6
−0.4
−0.2
0.0
Bootstrapped Slope Distribution

Group	*Target Location*	*Delay*	*Slope (β)*	*Confidence Interval*	*p-value*
Aiming	**Training**	**No**	-0.197	[-0.346, -0.047]	0.010
		1-min	-0.158	[-0.266, -0.031]	0.024
	Aiming	**No**	-0.588	[-0.712, -0.477]	< 0.001
		1-min	-0.401	[-0.575, -0.217]	0.001
Control			-0.388	[-0.671, -0.156]	0.002

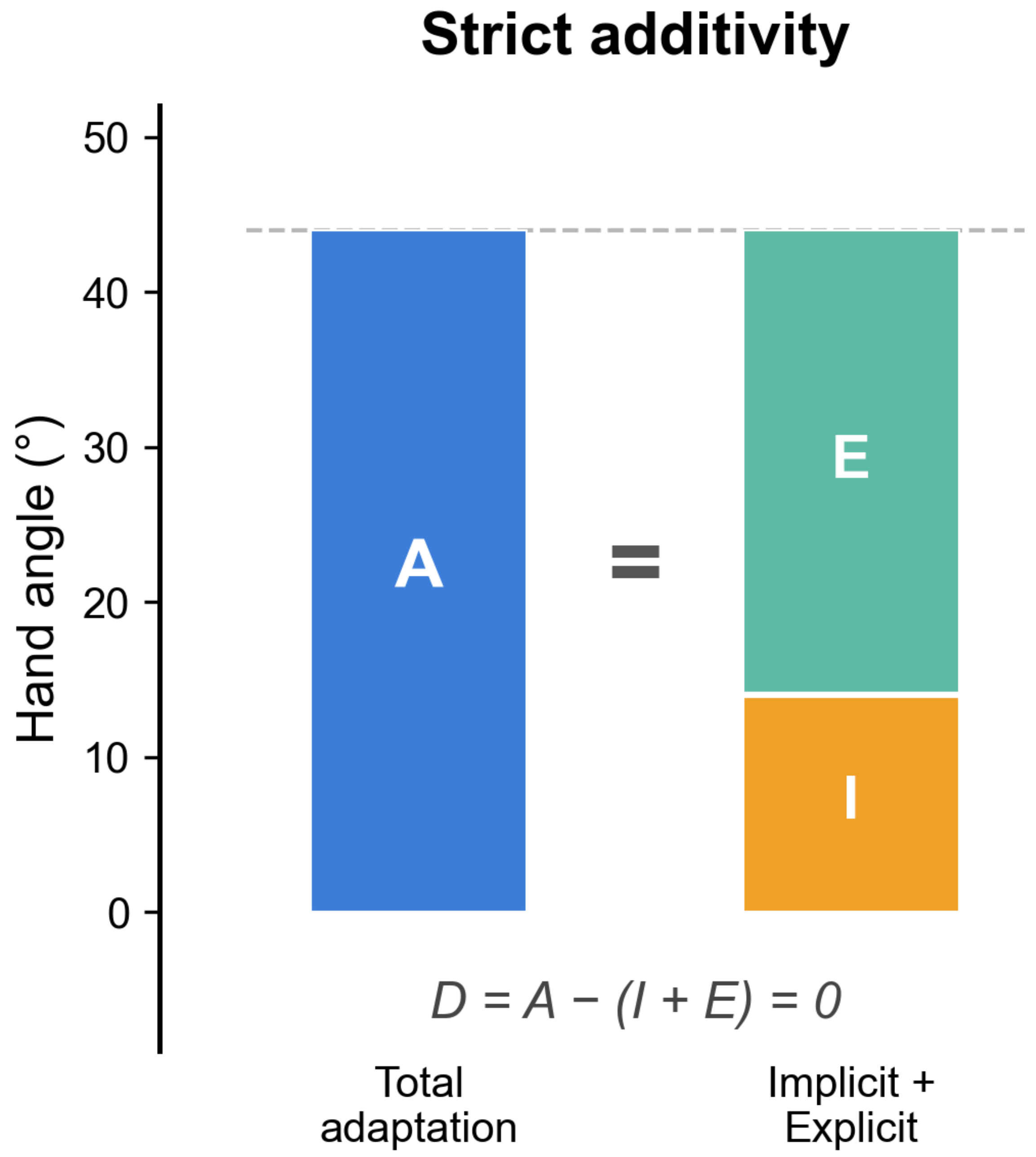
Table 1. Regression slopes between implicit and explicit adaptation across experimental conditions. Note: *β* represents the regression slope between implicit recalibration and explicit strategy. Under perfect linear additivity, the expected slope is -1.0. Bootstrap confidence intervals and *p*-values derived from 1,000 iterations.

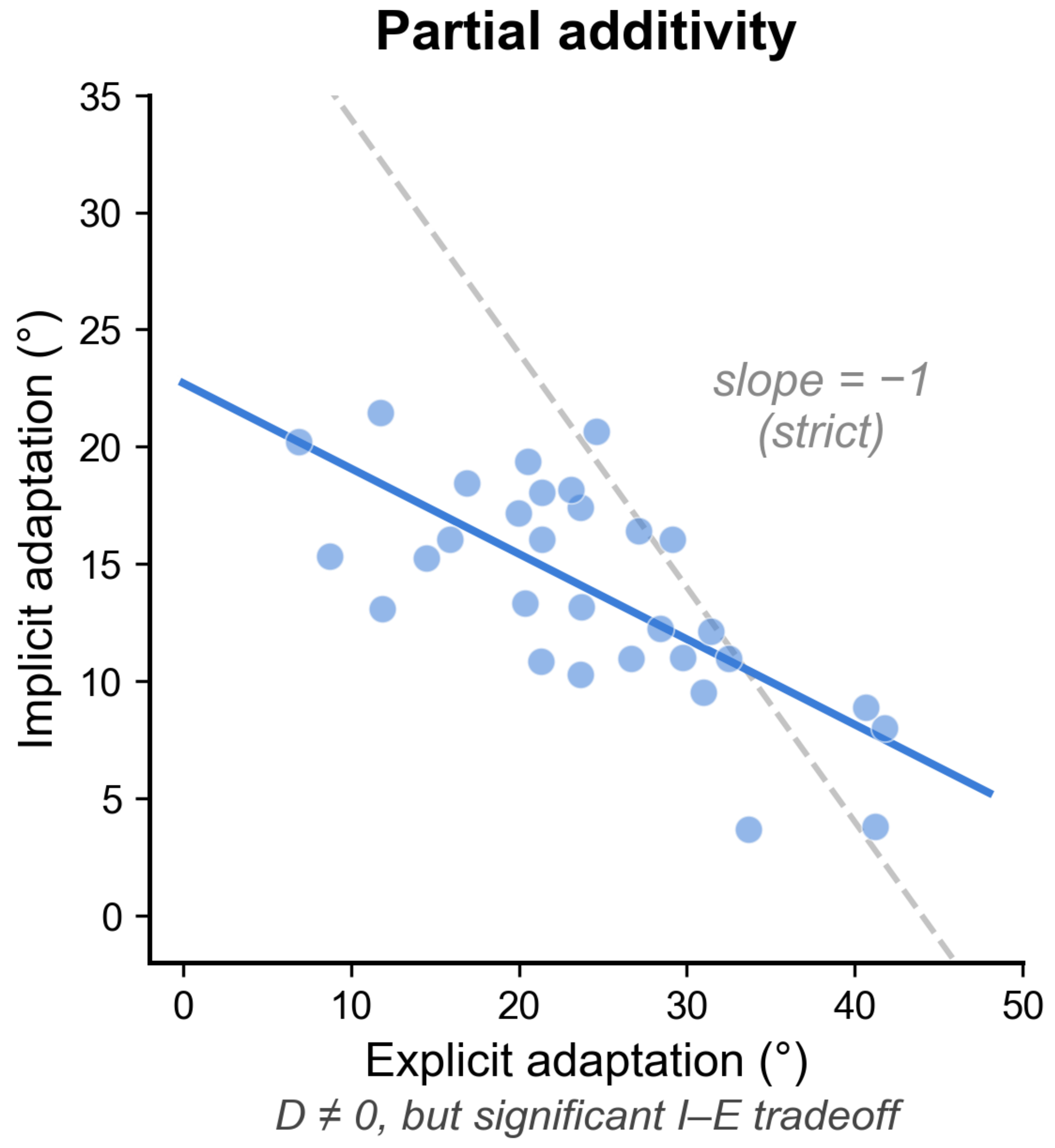
Group	***Observed Slope (95% CI)***	***Benchmark 1: Top-up as A (95% CI)***	***Difference from Benchmark 1 (p)***	***Benchmark 2: Inclusion as A (95% CI)***	***Difference from Benchmark 2 (p)***
Aiming	-0.281, [-0.408, -0.148]	-1.001, [-1.039, -0.958]	<0.001	-0.438, [-0.649, -0.234]	0.209
Control	-0.388, [-0.671, -0.156]	-1.001, [-1.039, -0.958]	<0.001	-0.438, [-0.649, -0.234]	0.903

Table 2. Comparison of observed slopes against simulation benchmarks under two operationalizations of total adaptation. Observed slopes reflect the collapsed analysis (aftereffect as implicit, Inclusion - Exclusion as explicit). Top-up benchmark simulates additivity using Top-up trials as total adaptation; Inclusion benchmark uses Inclusion trials. Differences indicate observed minus simulated slope; negative values indicate observed slopes were shallower than predicted.



Explicit and implicit adaptation trade off but do not perfectly sum — and whether slopes match additivity depends on how total adaptation is measured.


Strict additivity


Partial additivity