Implicit Adaptation is Fast, Robust and Independent from Explicit Adaptation

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Abstract

During classical visuomotor adaptation, the implicit process is believed to emerge rather slowly; however, recent evidence has found this may not be true. Here, we further quantify the time-course of implicit learning in response to diverse feedback types, rotation magnitudes, feedback timing delays, and the role of continuous aiming on implicit learning. Contrary to conventional beliefs, we affirmed that implicit learning unfolds at a high rate in all feedback conditions. Increasing rotation size not only raises asymptotes, but also generally heightens explicit awareness, with no discernible difference in implicit rates. Cursor-jump and terminal feedback, with or without delays, predominantly enhance explicit adaptation while slightly diminishing the extent or the speed of implicit adaptation. In a continuous aiming reports condition, there is no discernible impact on implicit adaptation, and both the rate of implicit and explicit adaptation progress at indistinguishable speeds. Finally, investigating the assumed negative correlation as an indicator of additivity between implicit and explicit processes, we consistently observe a weak association across conditions. Our observation of implicit learning early in training in all tested conditions reveals how fast and robust our innate adaptation system is.

Introduction

People constantly adapt their movements to changing circumstances, and this adaptation is driven by a combination of implicit and explicit processes. Implicit motor learning offers the advantage of preserving cognitive resources, thereby boosting performance efficiency. In contrast, employing explicit strategies demands more effort and may prove less efficient over the long term, even though it may manifest more quickly than implicit contributions to the learning process. People rely more on implicit processes when carrying out well-learnt motor tasks, but it is much harder to quantify these unconscious contributions to our performance, or even identify when they emerge and under what conditions. This study aims to investigate the time-course of implicit contributions during classical visuomotor adaptations and explore its sensitivity to different kinds of visual feedback.

Visuomotor adaptation is classically studied by having participants reach to targets with a misaligned hand-cursor that misrepresents their unseen hand. People rapidly adjust their hand in response to the deviated cursor motion. It is assumed that the initial compensation, achieved by directing the unseen hand elsewhere to move the cursor to the target, is driven by explicit strategy. Implicit contributions to adaptation are thought to emerge later and gradually replace the cognitive strategy as the movements become more automatic. Implicit adaptation is traditionally measured through reach aftereffects, which refer to the residual deviations in subsequent reaching movements even after the feedback is removed or returned to normal.

Recently, some studies (e.g., Kim et al., 2018; Morehead et al., 2017) have used clamped error feedback to assess implicit adaptation. In such paradigms, the cursor will always move in a straight line from the start position in a direction that misses the target by some predetermined amount. The distance from the home position typically matches the real distance from the home position, such that participants do feel some measure of control over the cursor. This situation where reaches always result in the same error is combined with instructions to participants to ignore this feedback and to keep moving the hand to the target as opposed to moving the cursor to the target. That is, participants are told to disregard and not learn from the only visible feedback they receive on their performance. Despite participants' best efforts to suppress any and all learning, their reaches do slowly start to deviate from the target. While the data from these types of paradigms are impressive reminders of the power of our implicit motor adaptation system, the participants' efforts to suppress learning likely reduces the speed of implicit adaptation, perhaps by orders of magnitude. Here, we assess the speed of implicit adaptation without any suppression.

Both studies using reach aftereffects and error-clamp paradigms indicate that implicit adaptation tends to saturate at around 10-20˚ independent of the rotation size (Kim et al., 2018; Morehead et al., 2017; Modchalingam et al., 2023). Because these implicit changes have been assumed to emerge much later, reach aftereffects are usually not measured until at least 60 or more perturbed training trials. However, recent research from our lab suggests that substantial implicit changes in hand movement can emerge rapidly within the first few trials of training with a visuomotor rotation (Ruttle et al., 2021). This paper will focus on testing implicit learning rates

during visuomotor adaptation using our method of interleaving visual cursor feedback trials, with no-cursor feedback trials.

In this study, we assess implicit learning rates across various paradigms that influence implicit, explicit, or overall adaptation. By fitting an exponential learning function across these initial trials, we can determine the rate at which implicit aftereffects develop during training.

First we test whether varying the cursor rotation size changes the rate of implicit adaptation. Previous research has indicated that the extent of implicit learning does not necessarily scale linearly with the magnitude of the perturbation, but seems to be capped at roughly 15˚ (Bond & Taylor, 2015; Kim et al., 2018; Modchalingam et al., 2019). However, it remains unclear whether the time course by which aftereffects emerge is consistent across different rotation sizes.

The second study investigates the impact of feedback types as specific kinds of visual feedback are known to affect the extent of implicit changes during visuomotor adaptation. We will employ two manipulations: terminal feedback and cursor jump. Terminal feedback offers limited visual feedback, providing cursor information only at the reach's endpoint (Fig 4). Cursor jump reveals the perturbation source and nature by jumping the cursor mid-reach (Fig 4). Previous research has produced mixed findings on the impact of terminal feedback on adaptation. Some studies (Taylor et al., 2014; Barkley et al., 2014; Hinder et al., 2008; Brudner et al., 2016; Schween & Hegele, 2017) suggest that terminal training may slow down adaptation and reduce aftereffects, while others (Heuer & Hegele, 2008; Song et al., 2020) find no significant differences. However, all of these studies typically assess aftereffects after 60 to 100 trials, making it unclear how the rate of these implicit changes develops over time. Cursor jump raises awareness of external perturbation (Gastrock et al., 2020), implying more reliance on explicit strategy. In this study, smaller implicit reach aftereffects were found following 90 trials of training. Thus, developing an explicit strategy during cursor-jump training could lead to a reduction in implicit-driven changes, and it is possible that it could also delay the onset of reach aftereffects. That is, both terminal feedback and cursor jump feedback seem to decrease the extent of implicit adaptation, but the effect of these types of feedback on the speed of implicit adaptation is unknown. We test that here.

Following this, we sought to observe the temporal progression of implicit processes when employing a paradigm designed to minimize their influence. Studies have found that the implicit component is reduced, if not eliminated, when the terminal feedback of the cursor is delayed by varying durations: 5 and 1 seconds (Brudner et al., 2016), 1.5 seconds (Song et al., 2020), and 1.1-1.3 seconds (Tsay et al., 2023). In our Feedback-delay study, we aimed to investigate whether aftereffects decrease with a 1.2-second delay in feedback during training and whether there is a hindrance in their onset.

In all of the aforementioned conditions, we not only assessed reach aftereffects with a high level of temporal precision but also intermittently gathered aiming trials during the latter stages of training to evaluate explicit contributions to adaptation (Taylor et al., 2014, McDougle et al., 2015, Bond & Taylor, 2015, Wilterson & Taylor, 2021, Yin & Wei, 2020). For our final study, we introduced a condition in which we measured both reach aftereffects and aiming at the same high rate. This allowed us to compare the rate of changes across both, without

assuming that one measure could be derived from the other (i.e., without assuming that implicit and explicit contributions are necessarily additive).

Collecting frequent aiming responses and implicit measures allows us to determine the extent to which these two processes are interconnected in the learning process ('t Hart et al., 2022). While implicit adaptation has been explored through clamp trials (Morehead et al., 2017, Tsay et al., 2021, Avraham et al., 2021), our approach involves measuring the type of implicit motor changes that contribute to the types of motor adaptation we routinely experience when interacting with our dynamically changing environments. By understanding the time course of these more natural implicit changes during motor learning, we can gain valuable insights that will help us enhance training and adaptation for various real-world scenarios.

Methods

Participants

We used data from 347 volunteer participants. These all had normal or corrected-to-normal vision (mean age = 21, females = 223) from the Undergraduate Research Participation Pool (URPP), and the Kinesiology Undergraduate Research Experience (KURE), who all provided prior, written, informed consent. The procedures used in this study were approved by York University's Human Participant Review Committee and all experiments were performed in accordance with institutional and international guidelines.

Participants were randomly assigned to 10 experimental groups, first to the groups in the feedback type experiment (n=94), then to the delayed feedback group and its control (n=65) and the rotation size experiment groups (n=151), and finally to the continuous aiming group (n=37). We can only assess the speed of any adaptation process if there is some amount of adaptation. This is why we only used data from participants whose reach deviations in the last 20 trials of the rotated phase are on average countering at least 50% of the rotation in their condition (i.e. we do not select participants based on no-cursor reach deviations, the main measure of interest, nor based on aiming responses). The participants listed at start of the methods and used in the analyses did meet our criterion, however we had to remove 19 participants in the *Feedback Type* experiment, 20 participants from the *Feedback Delay* experiment, 68 participants from the *Rotation Size* experiment, and 4 participants from the *Continuous Aiming* group. The data from these participants is available on the OSF repository (https://osf.io/ajwyr/).

Experimental Set-up

Apparatus

Participants sat on a height-adjustable chair facing a digitizing tablet (Wacom Intuos3, 12" x 12" surface, sampled on every frame refresh) and screen (Fig 1A). The tablet was positioned at waist level so hand movements could be made along a horizontal plane (See Fig 1A for detail). On top of the tablet there was a stencil with a circular portion cut-out measuring 20 cm in diameter (further details found on OSF: https://osf.io/7pzrb/). Visual feedback was shown on a computer screen located approximately 60 cm from the tablet workspace (22" monitor, 1680x1050 pixels, 60 fps). A wooden shield was placed above the tablet work surface to obstruct participants' view of their arm movements. Participants used a digital stylus to move the cursor (0.7 cm in diameter) onto the target displayed on a vertical screen (Fig 1A). The trial began when the cursor was moved to the home position. Participants had to move the stylus 8.8 cm to reach the target, with a margin up to the edge of the stencil of 1.2 cm. The stencil, positioned atop the digital tablet, effectively restricted the radial movement of reaches toward the target. It achieved this by physically impeding any outward movement beyond this limit.

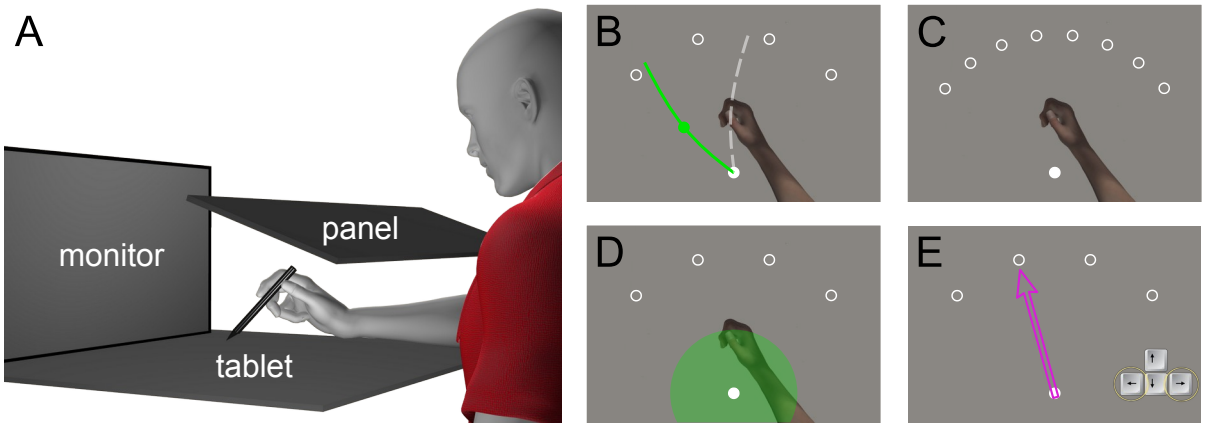

Fig 1. A. *Setup for all experiments. The stylus slides over a digitizing tablet while pen movements correspond to cursor movements on the connected upright screen. Hand view is blocked by a wooden panel.* ***B.*** *Reach training trial in the rotated phase of the experiment where the cursor position corresponded to the stylus position, rotated at the set perturbation depending on which condition the participant was assigned to.* ***C.*** *No-cursor trials would involve making a reach with no cursor feedback out to one of 8 trials.* ***D.*** *When performing the no-cursor trials, participants would see a green disk, which would help guide their movements out to the target as it informed them of their distance from the home position.* ***E.*** *Aiming trials used left and right arrow keys to move the arrow to point in a direction they were moving their hand to hit the target.*

Trial Types

For all 10 conditions, participants experienced a very similar main trial structure within the experiment. This involved alternating cursor trials (with further feedback details given in each experiment) with no-cursor trials (Fig 1C-D) where participants were told to directly move their unseen hand to the target. Later on in the experiment's rotated phase, we included eight aiming trials. Only in the experiment '*The Effect of Continuous Aiming*' did we have all three of these trials in succession of one another (aiming trial - cursor trial - no-cursor trial) for the entire experiment. Each of these components will be discussed below. We colour-coded the cursor and target to provide participants with extra feedback about the trial type as well as their performance. In both aligned and rotated phases (as well as washout), the cursor was white during reaching trials, and in the no-cursor trials the target was green. In the reach/training trials, the target was green when the cursor was aligned, and purple when the cursor was rotated. We also had the target change colour after the outward reach was completed to signify to the participant if they performed the trial according to our criteria outlined below. The target would turn blue when they met the criteria, and orange if they did not.

Cursor Trials

Cursor, or Reach-training trials involve participants making out-and-back reaching movements to hit a target. All conditions had four forward targets (0.7 cm in diameter) located at 45˚, 75˚, 105˚, and 135˚ as shown in Figure 1B. Participants reached to one of the four targets, and upon completion the target would vanish, and participants would receive feedback about movement position, and then move their cursor back to the home position.

No-Cursor Trials

These trials worked very similarly to the *Cursor Trials* with two notable differences. Primarily, participants were unable to see their hand position during the outward reach. Instead of a cursor, they used the green disc (shaded filled circle) which increased in size the farther away they moved from the home position during their outward reach. Subsequently, participants returned their hand to the home position without a cursor, with the assistance of the green disc that indicated the remaining distance to the home position. When the (unseen) tip of the stylus was within 2.1 cm of the home position, the cursor became visible again to ensure a precise return to home. Second, the target alternated between eight different locations, such that the current no-cursor target was located ± 7.5˚ degrees from the previous reach-training target. These locations were 37.5˚, 52.5˚, 67.5˚, 82.5˚, 97.5˚, 112.5˚, 127.5˚ and 142.5˚ (as shown as "No Cursor" in Fig 1C-D).

Aiming Trials

Aiming trials, shown in Fig 1E far right, are used to measure the explicit component of adaptation. Participants adjusted the arrow's direction using the left-and-right arrow keys to indicate the direction they planned to move their hand so that the cursor would hit the target. Once they got to the desired position, they would press the spacebar, and would be able to continue with the next reach-training trial. Following previous studies, we used aiming trials to measure the extent that adaptation may reflect a cognitive strategy. Crucially, the participants

were not told about the arrow trials before they adapted so as to prevent the concepts inherent to aiming trials ("in which direction would you move your hand in order to make the cursor hit the target") themselves from evoking strategy-based adaptation. This was done to ensure that we had a true measure of the natural time-course of implicit adaptation. Participants were given on-screen instructions before the aiming trials appeared. As expected, this led to some de-adaptation right after the first few aiming trials. While participants quickly recovered, this is why we included them later in the experiment so as not to affect the fitted initial change calculation. Of course, in the continuous aiming condition, the instructions were given in the familiarization phase and never re-appeared.

General Procedure

After providing informed consent and demographic information, all participants watched a basic instruction video in an effort to standardize the instructions received (these are all on OSF: https://osf.io/ajwyr/). They were allowed an opportunity to ask questions if something in the video was unclear or they could re-watch the video. The experiment consisted of three distinct phases, a practice phase for familiarization with the task, an aligned phase where baseline performance was established, and a rotated phase when the perturbation was introduced. For the first experiment, we also included a fourth washout phase to measure de-adaptation of the rotation.

Participants began by completing a practice phase of the experiment, consisting of 16 reach-training trials, and the interleaved 16 no-cursor trials. If the condition involved special kinds of feedback during reach-training trials, such as in the cursor-jump or terminal feedback condition, this was introduced in the second half of the practice phase. During the practice phase, participants were given feedback on their reaching movements. They would see "too slow!" if they did not complete the reach within 1500 ms, and "missed target!" if the cursor missed the target by 15˚ or more. Before the real task started, there was a break that allowed for any remaining questions. The experiment then started with an aligned phase of 20 reach-training trials and the interleaved 20 no-cursor trials. In both the practice phase and aligned phase, the direction of the hand-cursor motion was aligned with the unseen hand. After participants completed the 16th aligned pair of trials, a warning screen with instructions was shown telling them that in eight trials the original green target was going to turn purple and that they were going to have to move a bit differently for the cursor to hit the target. Most importantly, they were told to not slow down, but to keep making reaches at the established pace, and that they should keep making straight reaching movements. This was also mentioned to them in the instruction video prior to beginning the experiment. The colour feedback about performance was provided in order to keep participants motivated. This colour change to purple marked the beginning of the rotated phase. In this, they would complete 100 pairs of trials again with alternating reach-training trials and no-cursor trials. In the reach-training trials presented in this phase the cursor was shown at a location that represented the stylus position, rotated about the starting position. The exact amount of rotation differed across groups as described below, but was 45° in most groups. The no-cursor trials were the same as before, and participants were told to reach directly to the target. Before beginning the 56th pair of rotated trials, participants performed the first aiming trial, and did so 7 more times, once after every 4 pairs of trials. All

experiments were monitored by a Research Assistant to confirm that participants followed the instructions provided.

Data Analysis

Our experiment has two different trial types with similar analysis methods used in each. In both cursor and no-cursor trials, participants completed an out-and-back reach for which we calculated the deviation of the outward reach from a straight reach to the target. For both, we took the first sample further than 1.8 cm from the home position, and calculated the angle between a line through the home position and this sample and a line through the home position and the target. To quantify explicit awareness, we took the direction of the arrow relative to the direction of the target. In all aiming trials, the arrow started out 15° CCW relative to the target. Since in all rotations people moved their unseen hand in a direction CW to compensate for CCW rotation, participants *without* a strategy would need to move the arrow approximately to the target, and participants *with* a strategy would need to move it further CW. We rejected arrow aiming directions that were unreasonable, specifically those where the arrow did not move from its original 15° CCW direction or moved in the wrong direction relative to where their unseen hand should have moved to compensate for the visuomotor rotation. We did so because in these trials it is likely the participant either did not move the arrow due to a misunderstanding or erroneously ended the aiming trial.

For each dependent measure, reach deviations for reach-training trials and no-cursor trials and aiming deviations for aiming trials, a 0 indicates no adaptation and a value equal to the rotation would indicate full adaptation.

Analyses

The main goal of our study was to identify the time-course of implicit learning during classical visuomotor adaptation, i.e., reach aftereffects, in various conditions. For all plots and analyses, the results in the rotation conditions were baselined by subtracting out the averaged deviation in the aligned condition.

Learning Requirement

We wanted to investigate the rate of change in reach training trials, implicit no-cursor test trials, and of aiming responses. However, we cannot measure the rate of change unless we first validate that participants have actually changed their performance over the course of training with the rotation. This is why in all groups we looked at if participants had adapted to 50% of the perturbation by the end of the rotated phase. After this criteria was met, we retained 347 out of 458 participants.

Exponential Learning Function for Rate of Change

To rigorously quantify the time-course of the implicit process we used an exponential learning function which used error decay with an asymptote to identify a rate of change for each trial type in each feedback condition. We used the same equation, shown below, as used in Ruttle et al. 2021. The value of the process on the next trial (P_t) is the current process' value (P_{t-1}) minus the

product of the rate of change (L) multiplied by the error on the current trial, which is the difference between the asymptote (A) and the process' value on the current trial (P_{t-1}).

$$P_t = P_{t-1} - L \cdot (A - P_{t-1})$$

The parameter L was constrained to the range [0,1], and the parameter A to [-1,2*max(data)]. This model was fit to the rotated reach data and reach aftereffect data for all groups. In order not to overestimate the speed at which aftereffects arise, a zero was prepended to this time course. This accounts for the fact that responses in these trials already changed through the previous training trial. Each parameter was bootstrapped (5 k resamples per fit) across participants to get a 95% confidence interval which can then be compared, and values can be found in Tables 1-4.

The fitted rate of change is relative to the asymptote, but while the initial change in all four conditions in the first (rotation size) experiment are strikingly similar in absolute terms (degrees change in reach deviation) their asymptotes are very different.

Exponential Decay of Washout

In our analysis of washout for the rotation size experiment, we employ an exponential decay function to investigate the decay of adaptation after the rotation has been removed. This function also used two parameters, the first denoted as R, representing the retention rate within the range of [0,1]. This indicates the proportion of the current process value (P_{t1}) retained for the subsequent trial (P_t). Specifically, the process value at trial t (P_t) is calculated by multiplying the value of the preceding trial (P_{t1}) by the retention rate (R). This multiplication operation reflects the diminishing effect of past information on the current state of the process. The second parameter of the function is the initial value of each process, denoted as P_t at the first trial (t=0), which falls within the range of [-1,2*max(data)]. This range encapsulates the potential variability in initial values across different experimental conditions or datasets.

$$P_t = R \cdot P_{t-1}$$

Both of the above equations describe the change of a quantity over time with parameters determining the rate of change or decay. However, the first equation models a process approaching an asymptote with a relative rate of change, while the second directly scales previous values with a retention rate, specific to washout decay. We bootstrapped 5 thousand parameter values for each group, and the confidence intervals obtained from this are reported (see tables 1-4). In order to compare parameters between groups, we subtract all the bootstrapped values from one group from all bootstrapped values of the other group, for 25 million difference scores. This is used to get a 95% confidence interval for the difference between groups. If this interval includes 0, we do not consider the difference to be meaningful in this data set.

Bayesian Statistics

We employed Bayesian statistics to compare the extent of re-aiming during the rotated phase between different feedback types. Bayes Factors were used to determine whether there were significant differences or significant equivalences. Bayes Factors represent the ratio of the

likelihood of the alternative hypothesis (the presence of a difference) to the likelihood of the null hypothesis (equivalence), given the data and noninformative prior of √2/2. Using this factor maintains consistency with previous research and appropriately scales the expected effect sizes (Morey et al., 2011; Rouder et al., 2012). A Bayes Factor of 1 indicates an equal likelihood of both hypotheses. When the Bayes Factor falls within the range of 1/3 to 3, there is only anecdotal evidence and no strong preference for either hypothesis (Jeffreys H, 1961; Rouder et al., 2009). However, a Bayes Factor greater than 3 or less than 1/3 indicates moderate evidence in favour of the alternative hypothesis or the null hypothesis, respectively. Bayes Factors greater than 10 or less than 0.1 are considered strong evidence supporting one hypothesis over the other.

The approach was to first do a Bayesian "F-test" on the rate of change as well as the asymptotes across all groups within an experiment. If this indicated either little evidence one way or another or equivalence, no further test was done. If this indicated an effect of condition on either rate of change or asymptote, a series of Bayesian "t-tests" was done on only the parameters that showed an effect. In the rotation size experiment each pair of successively larger rotations was tested. In the feedback type and continuous aiming experiment, the control condition was compared with all other conditions. In the delayed feedback experiment, the terminal feedback condition without any delay was compared with all other conditions. In the continuous aiming experiment, there were only two conditions, such that no further tests were needed.

Results

To investigate potential changes in the extent and time-course of implicit adaptation in these groups, we compared rates of change (RofC) and asymptotes for both no-cursor trials and reach/training trials. We also calculate means and 95% confidence intervals for the re-aiming responses. The resulting values, along with their bootstrapped 95% confidence intervals, are presented in Tables 1-4.

The Effect of Rotation Size (n=151)

Previous results suggest that the extent of the implicit component of adaptation or aftereffects does not change with the size of the perturbation (Bond & Taylor, 2015), but it is unknown whether the time-course is affected or not. We tested this for four rotations; 15°, 30°, 45°, and 60° (Fig 2 with N of 17, 43, 23 and 52, respectively) with a continuously visible cursor (for the outward reach) across different groups of participants. This experiment also contained a washout phase where the cursor-rotation was removed, so we could investigate how quickly people de-adapt both in no-cursor trials and reach-training trials (with cursor).

Given that the magnitude of implicit aftereffects following adaptation typically remains relatively constant at around 15° regardless of the rotation size, we aim to investigate whether the time-course of these aftereffects is similarly unaffected by the magnitude of rotation. Our statistical evaluation came from the 5K parameter values for each group, which determined their confidence intervals (Table 1). We then calculated 25 million difference scores to compare groups and a 95% confidence interval for the group difference was obtained. If this interval

includes 0, the difference is not considered significant in this dataset. First, we look at reach-training trials during the longer learning phase for all successive rotation sizes. We find that rates of change are not different [95% CI for 15° to 30° (-0.034, 0.164); 30° to 45° (-0.100, 0.065); 45° to 60° (-0.057, 0.121)], but asymptotes increase with rotation size [95% CI for 15° to 30° (-11.494, -6.481); 30° to 45° (-18.952, -10.239); 45° to 60° (-13.666, -4.024)], as expected (Table 1). For no-cursors, the 30° group at 20.7% (15.5%-26.3%) has a larger RofC than the 45° group at 11.3% (7.5%-18.5%), meaning that people training with the 30° rotation change their no-cursor reach directions faster than the 45° group, as seen in Table 1 (95% CI 0.018, 0.161). Otherwise, there are no differences in learning rates in the comparisons here [95% CI for 15° to 30° (-0.099, 0.143); 45° to 60° (-0.016, 0.088)]. When looking at these fitted initial changes, after a single rotated reach trial, the deviations for no-cursor reaches over 4° for the 30° rotation, and over 2° for the other three rotations, as also seen in Table 1. Surprisingly, we found that the implicit aftereffects were not simply capped at 15° but varied slightly with rotation size, as shown in Fig 3B. Specifically, implicit asymptotes did not vary across rotations of 15°, 30°, and 45° [95% CI for 15° to 30° (-10.556, -6.745); 30° to 45° (-8.259, -0.182)]. Only when the rotation increased from 45° to 60° [95% CI for 45° to 60° (-7.776, 1.758)] did we fail to detect a difference in our large sample sizes.

Now we look at how these groups compare during washout. For reach training trials, we compare the 15° group with all other groups and find that rates of change did not differ [95% CI for 15° to 30° (-0.055, 0.0326); 15° to 45° (-0.045, 0.048); 15° to 60° (-0.044, 0.041)]; however, the 15° group has a smaller starting level (Table 1). These starting levels got larger as rotation size increased [95% CI for 15° to 30° (-13.313, -7.439); 15° to 45° (-14.378, -4.865); 15° to 60° (-16.322, -9.164)]. The same pattern is observed when examining implicit no-cursor rates of change [95% CI for 15° to 30° (-0.139, 0.068); 15° to 45° (-0.151-0.072); 15° to 60° (-0.118, 0.083)] and starting levels [95% CI for 15° to 30° (-8.824, -2.548); 15° to 45° (-14.378, -4.865); 15° to 60° (-16.322, -9.164)] (Table 1).

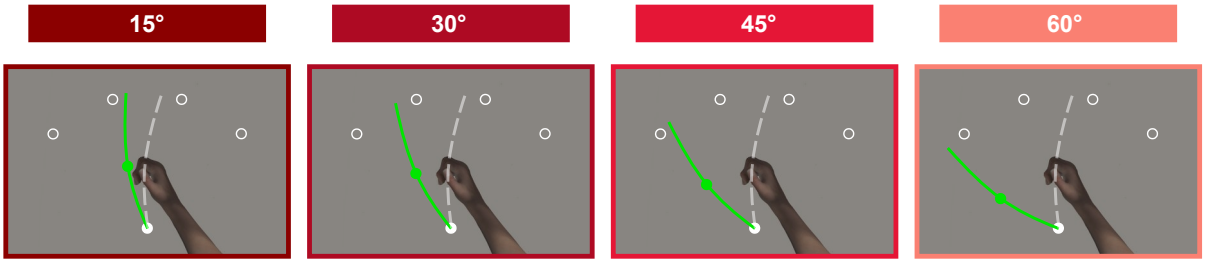

Fig 2. *Participants reached with a cursor to one of the four forward targets as quickly and as straight as possible. Participants in the four conditions would train with one of four rotation sizes: 15°, 30°, 45° or 60° CCW rotated feedback.*

Here we will turn our attention to the eight aiming trials we conducted to investigate explicit strategy. We expect little explicit strategy in the 15° and 30° conditions, and will see how this develops in larger rotations. Finally, we will also test if implicit adaptation can be predicted from explicit adaptation. We find an effect of rotation size on the magnitude of re-aiming responses (BF_{10} > 1000, as illustrated by the first 4 bars in Fig 9E). We then compared the re-aiming responses to 0 for each condition, and then completed follow-ups between successive rotations. The evidence goes to a small amount of re-aiming in the 15° condition (2.3° on average, BF_{10} = 7.087) and in the 30° condition (3.8°, BF_{10} > 1000). In the other two conditions, it is much more clear that almost all participants engage in some amount of re-aiming (BF_{10} > 1000, also seen in Fig 9E). Now comparing aiming between successive rotations, we find that re-aiming is not very different between the 15° and 30° groups (BF_{10} = 0.518). We do see the expected difference between the 30° and 45° group (BF_{10} > 1000), but there is no evidence when comparing the 45° and 60° group (BF_{10} = 1.029). For the most part, we observe comparable explicit changes in the two large rotations.

			15° (N=21)	**30° (N=47)**	**45° (N=24)**	**60° (N=59)**
Rotated phase	Reach training	RofC	21.4% (14.9%-32.8%) 2.0°	16.8% (13.2%-21.3%) 3.1°	17.3% (12.0%-26.1%) 5.7°	14.9% (11.0%-21.1%) 6.2°
		asymptote	9.5° (7.8°-11.2°)	18.5° (16.7°-20.4°)	33.1° (29.2°-37.0°)	41.9° (39.1°-44.7°)
	No-cursors	RofC	19.4% (13.0%-34.2%) 2.2°	20.7% (16.1%-26.4%) 4.2°	11.3% (7.8%-17.2%) 2.7°	8.7% (7.0%-11.0%) 2.4°
		asymptote	11.4° (10.1°-12.7°)	20.1° (18.7°-21.5°)	24.2° (20.5°-28.1°)	27.3° (24.5°-30.2°)
	Re-aiming	extent	2.3° (0.9°-3.8°)	3.8° (2.4°-5.4°)	15.2° (11.0°-19.6°)	21.8° (17.9°-25.9°)
Washout phase	Reach training	start level	8.7° (8.4°-12.5°)	19.4° (18.8°-22.9°)	19.7° (15.7°-24.4°)	23.7° (20.3°-26.1°)
	No-cursors	start level	4.0° (2.5°-7.5°)	9.8° (8.8°-12.5°)	9.3° (6.6°-12.6°)	12.8° (9.9°-15.6°)

Table 1. Rotation size group comparison of rates of change and asymptotes for both the rotated and washout phase. Both rotated and washout phases contain values for reach training trials with cursor feedback, and the implicit no-cursor trials. Average aiming responses across the eight aiming trials in the second half of the rotated phase are also shown above.

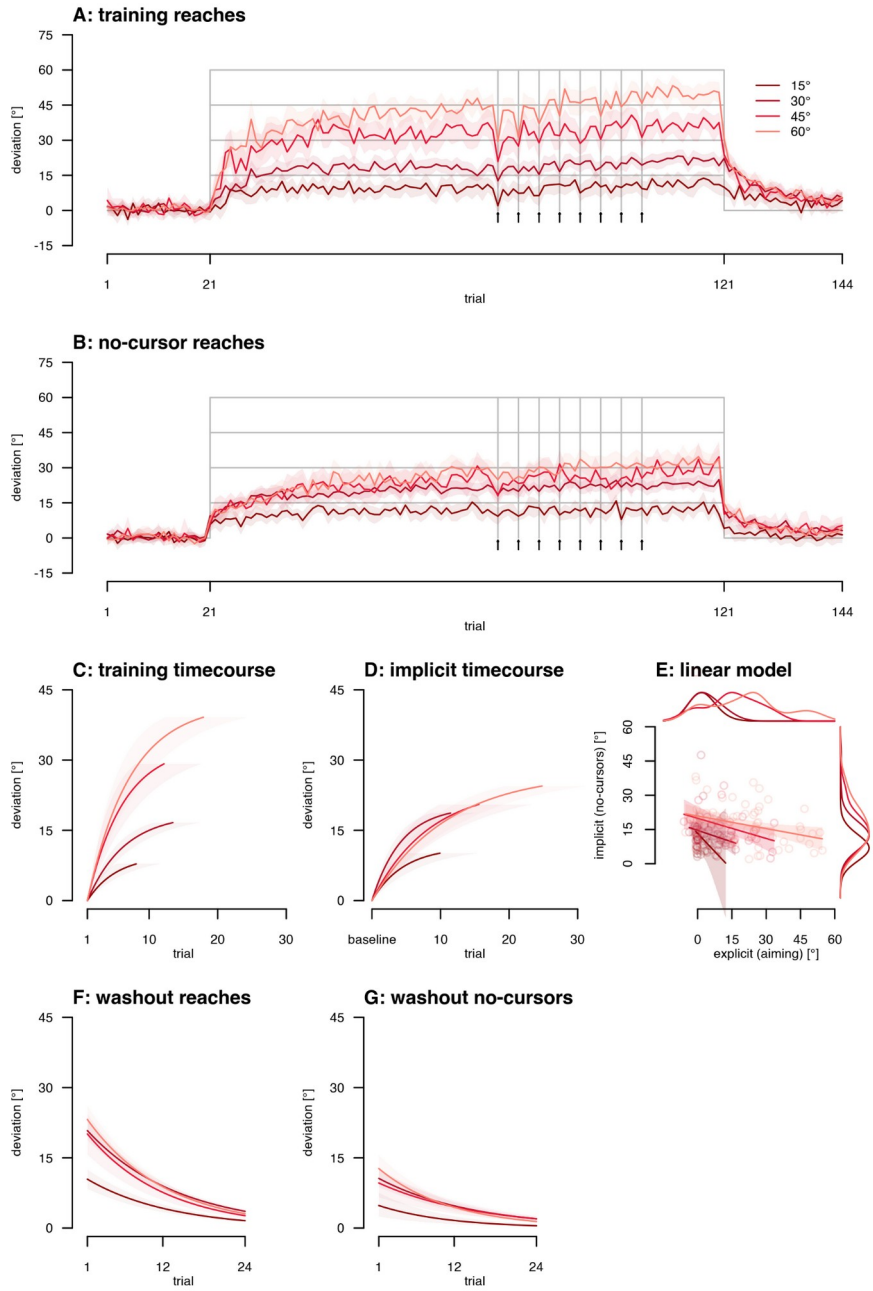

Fig 3. Rotation Size. *Shaded regions indicate 95% confidence intervals.* ***A.*** *Reach adaptation across trials, with eight aiming trials in the second half of the aligned phase (indicated by arrows and vertical lines).* ***B.*** *Implicit reach aftereffects across trials* ***C.*** *Fitted exponential curves for reach adaptation in the rotated phase.* ***D.*** *Fitted exponential curves for implicit reach aftereffects in the rotated phase.* ***E.*** *Individual data scatter plot with regression lines depicting the relationship between implicit and explicit learning processes. Each dot represents a participant.* ***F.*** *Fitted exponential decay for reach training trials in the washout phase.* ***G.*** *Fitted exponential decay for reach aftereffects in the washout phase.*

Finally, we will explore how explicit adaptation fares as a predictor of implicit adaptation. The slope is around -1 in the 15° group, but the linear relationship is not significant [r = -0.218, p = 0.342, slope: -1.059 CI (-3.335, 1.217)]. In fact, the wide range covered by the confidence interval further emphasizes the variability and lack of precision in the observed relationship within the 15° group. The 30° group also had no significance [r = -0.246, p = 0.103, slope: -0.337 CI (-0.745, 0.071)]. For the 45° and 60° groups, the linear relationship is significant, but the confidence interval for the slope does not include -1 [45°: r = -0.430, p = 0.036, slope: -0.294 CI (-0.568, -0.021); 60°: r = -0.332, p = 0.010, slope: -0.183 CI (-0.321, -0.045)]. Taken together, we find a weak and non-additive relationship between implicit and explicit adaptation.

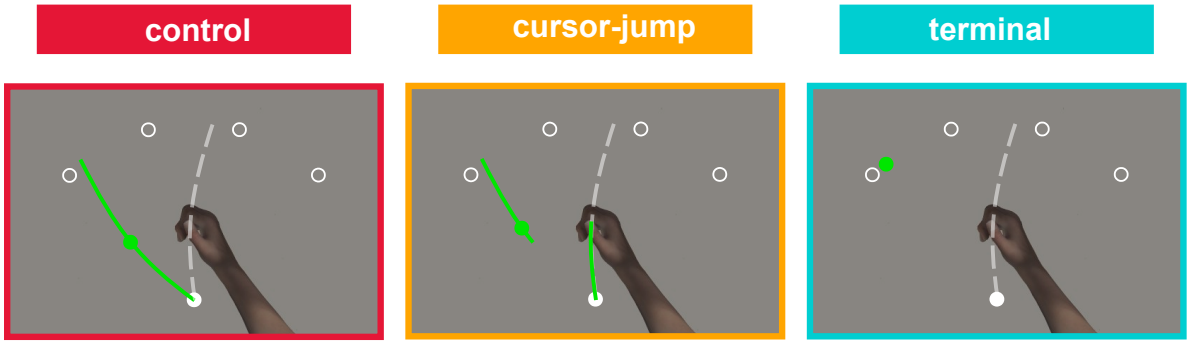

Fig 4. *Participants reached with a cursor to one of the four or eight forward targets as quickly and as straight as possible. Participants in these three groups experience different trial types through three kinds of rotated cursor feedback: continuous, terminal (cursor only shown at end of reach trial), and cursor jump (cursor jumps 45° CCW mid-reach on every trial). Each participant would perform a trial with one of the above feedback types interleaved with no cursor trial.*

The Effect of Feedback Type (n=94)

We tested three different types of feedback in the Feedback-Type experiment: Continuous, Terminal and Cursor Jump (Fig 4). There were 69 participants in the *Continuous-feedback or Control Group,* the cursor was continuously visible during the outward cursor movement. This included the 23 participants from the previous experiment who did the 45° condition. The 27 participants who were in the *Terminal Group* were only shown the rotated cursor at the end of the outward movement. Specifically, the cursor was not displayed until the hand had moved 8.8 cm radially from home position. There was also only 1 static cursor position shown for 750 ms, regardless of any subsequent movements by the participant. That is, visual feedback consisted of knowledge of results only. The *Cursor Jump Group* consisted of 26 participants and the cursor for this group was aligned with the hand for the first half of the distance to the target. When this 50% distance was reached, the cursor rotation of 45° (CCW) was applied for the rest of the trial. This type of feedback was similar to a task performed in Gastrock et al. (2020) which increased explicit strategy.

		Control (N=51)	Cursor-jump (N=32)	Terminal (N=35)
Reach training	RofC	19.8% (14.8%-27.9%) 6.2°	13.5% (8.7%-19.3%) 4.5°	31.1% (19.7%-47.2%) 9.6°
	asymptote	31.5° (28.7°-34.5°)	33.1° (29.9°-36.2°)	30.7° (27.9°-33.4°)
No-cursors	RofC	12.0% (9.0%-15.8%) 2.8°	12.5% (8.0%-18.2%) 2.2°	10.7% (6.5%-20.4%) 1.6°
	asymptote	23.7° (21.2°-26.5°)	17.6° (14.5°-21.4°)	14.7° (11.7°-18.3°)
Re-aiming	extent	14.4° (11.8°-17.2°)	25.1° (22.0°-28.5°)	25.8° (22.5°-29.1°)

Table 2. Feedback type group comparison of rates of change and asymptotes for the rotated phase. RofC and asymptote values shown for reach training trials with cursor feedback, and implicit no-cursor trials. Average aiming responses across the eight aiming trials in the second half of the rotated phase are also shown above.

Next, we wanted to understand how rapidly the implicit components of adaptation emerge in response to the type of visual information during classical visuomotor adaptation. From our bootstrapped parameters, we computed confidence intervals for each group (Table 2). We then compared the groups by calculating difference scores and deriving a 95% confidence interval for their difference. If this interval includes 0, the difference is not deemed significant in this dataset. We first compare each test group with the now combined control group. For all subsequent control groups, we merged the 45° group from the rotation size experiment, given its identical rotated phase. First looking at reach training in Fig 5, it seems that the three groups have similar rates of change, and this is confirmed by the 95% confidence intervals for the difference between groups [control and terminal (-0.280, 0.025); control and cursor jump (-0.013, 0.156)]. Similarity is also observed for reach training aymptotes [control and terminal (-3.111, 4.854); control and cursor jump (-5.911, 2.734)]. We now compare the test groups with the control group on parameters describing the time-course of implicit reach aftereffects. We find no effect on rates of change [control and terminal (-0.085, 0.070); control and cursor jump (-0.069, 0.054)], but the asymptotic level of implicit reach aftereffects is larger for the control group than for either test group [control and terminal (4.568, 13.122); control and cursor jump (1.570, 10.287)], with the control group at 23.7° (21.2°-26.7°) and terminal and cursor jump having asymptotic values of 14.7° (11.7°-19.0°) and 17.6° (13.8°-21.1°), respectively.

We then test for an effect of feedback type on re-aiming, and find an effect on explicit strategies (BF_{10} > 1000, as seen in Fig 9E). We can see in Table 2 and Fig 5 that terminal and cursor jump have 170% the amount of explicit strategy as compared to the control group (~14° and ~25° of strategy), and comparing the control group to the others we find differences from

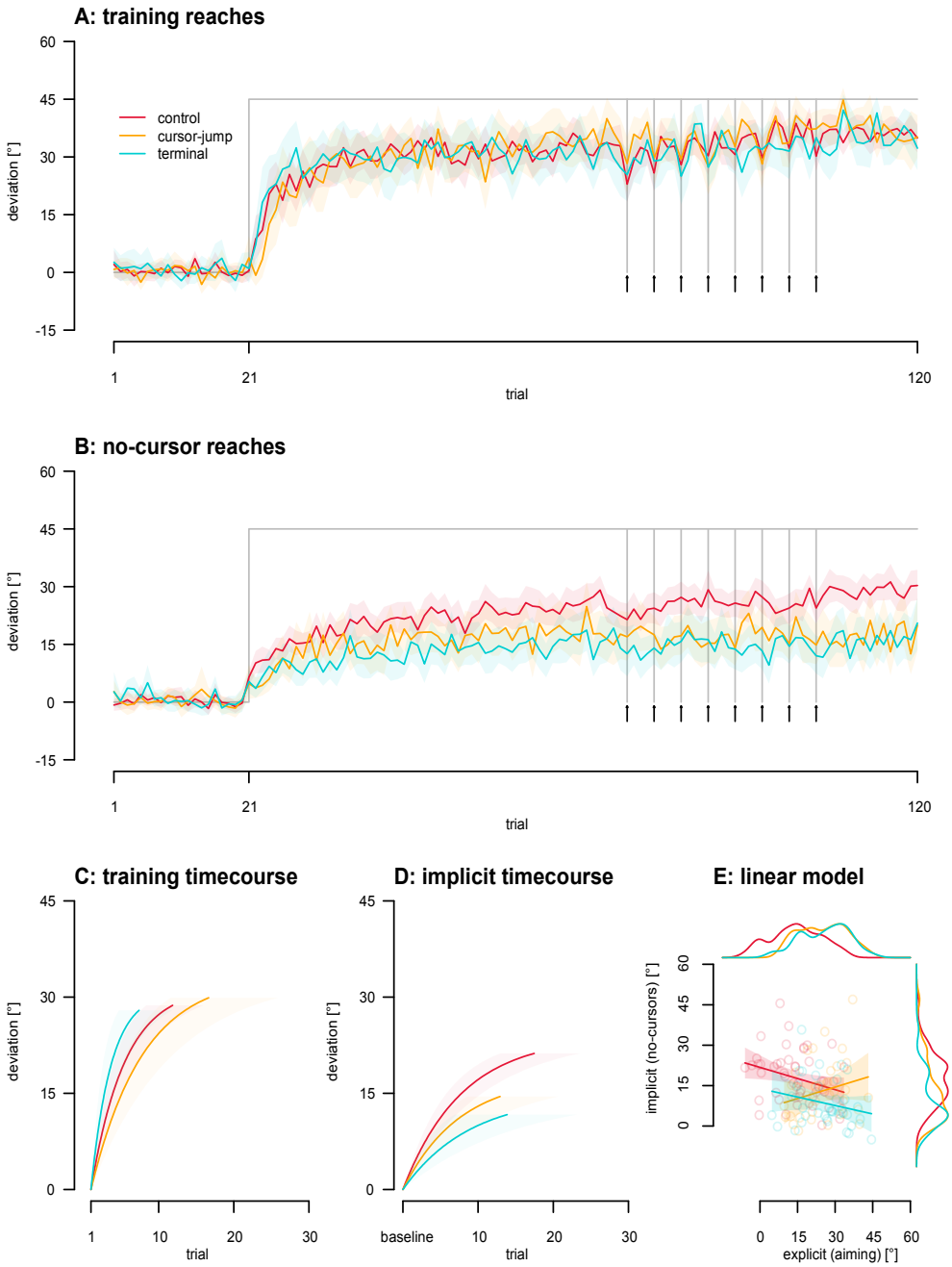

Fig 5. *Feedback Type. Shaded regions indicate 95% confidence intervals.* ***A.*** *Reach adaptation across trials, with eight aiming trials near the end of the rotated phase (indicated by vertical lines and arrows).* ***B.*** *Implicit reach aftereffects across trials* ***C.*** *Fitted exponential functions for reach adaptation in the rotated phase.* ***D.*** *Fitted exponential functions for implicit reach aftereffects in the rotated phase.* ***E.*** *Scatter plot with regression lines depicting the relationship between implicit and explicit learning processes. Each dot represents a participant.*

terminal and cursor jump (both BF_{10} > 1000).

Here we test explicit adaptation as a predictor of implicit adaptation and across participants in the control group there is a significant relationship, but the confidence interval for the slope does not include -1 [r=-0.299, p=0.033, slope: -0.275 CI (-0.527, -0.023)]. Thus, matching the pattern found before in the rotation size experiment. However, none of the other relations are significant, and the confidence intervals of the slopes include 0 but not -1 [terminal: r=-0.220, p=0.203, slope: -0.209 CI (-0.156, 0.725); cursor jump: r=0.234, p=0.198, slope: 0.284 CI (-0.536, 0.119)]. Thus, there is only a weak relationship between implicit and explicit components in the control, continous cursor, but none for the terminal and cursor jump conditions.

The Effect of Feedback Delay (n=65)

After testing the kind of feedback, we now wanted to check how manipulating the timing of feedback affected implicit learning (Fig 6). Studies have demonstrated that inserting a delay prior to terminal feedback can reduce the extent of implicit learning (Brudner et al., 2016, Tsay et al., 2023). However, the impact of this delay on the rate of implicit learning remains unknown. The following experiment seeks to shed light on this.

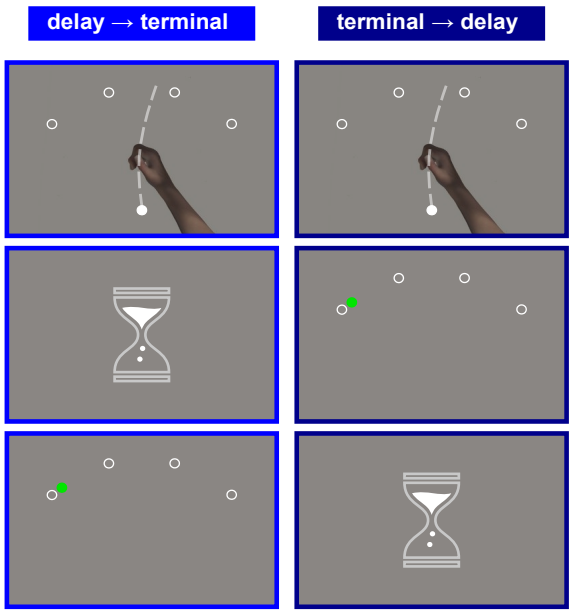

Fig 6. *Participants in the two new groups in this experiment experienced a delay as follows. The delay → terminal group would make the reach, wait during the 1.2 s delay, and then receive feedback about the end-point position for 0.6 s. The terminal → delay would receive feedback right away for 0.6 s, and then wait during a 1.2 s delay took place. The delay and feedback intervals combined took 1.8 s in each case, and the participant was to hold the stylus during that time, and only return to the home position after the 1.8 s hold. Each participant would perform a trial with one of the above types of feedback interlaced with a no cursor after every trial.*

delay → terminal group: 34 participants adapted to a 45° CCW visuomotor rotation with terminal cursor feedback. This group was like the Terminal Group in the feedback type experiment, except that we included a 1.2-second delay before the cursor was displayed for 600 ms following the 8.8 cm outward hand movement. Participants received end-point position feedback once their hand had moved 8.8 cm, and then the delay would begin. Participants were instructed to hold their end position for the full 1.8 seconds that took up the delay and the feedback period. After participants had held their end position for 1.8 seconds, the green circle guiding return movements would be shown, signalling that participants could move back to the home position. However, if participants moved more than 3.8 mm inward from the end position of their reach during the hold period, they would need to move back to the outer edge 8.8 cm away from home, and restart the hold period of 1.8 seconds. This was to ensure participants would indeed hold the stylus at the end position while feedback was shown. The terminal feedback was shown at the same time and for the same duration after the outward reach was finished, irrespective of whether or not the hold was maintained.

terminal → delay group: The 35 participants in this group served as a control for the above delay → terminal group in case the extra delay time would affect the overall time-course. For this group, much like the original Terminal group, they received a single position of cursor feedback for 600 ms immediately after the pen moved to the edge of the outer stencil where the targets were displayed. But we inserted a 1.2-second delay afterwards so that the trial length was similar to the delay → terminal group. Like the delay → terminal group, this group also had

		Control (N=51)	**Terminal (N=35)**	**Terminal → delay (N=39)**	**Delay → terminal (N=26)**
Reach training	RofC	19.8% (14.8%-27.9%) 6.2°	31.1% (19.7%-47.2%) 9.6°	19.5% (12.7%-29.8%) 6.2°	16.7% (9.9%-30.0%) 5.6°
	asymptote	31.5° (28.7°-34.5°)	30.7° (27.9°-33.4°)	31.8° (28.9°-34.7°)	33.4° (29.9°-37.0°)
No-cursors	RofC	12.0% (9.0%-15.8%) 2.8°	10.7% (6.5%-20.4%) 1.6°	18.9% (8.4%-66.9%) 2.7°	12.7% (8.8%-17.5%) 1.7°
	asymptote	23.7° (21.2°-26.5°)	14.7° (11.7°-18.3°)	14.4° (11.5°-18.1°)	13.2° (10.2°-16.3°)
Re-aiming	extent	14.4° (11.9°-17.1°)	25.8° (22.6°-28.9°)	18.2° (13.4°-23.1°)	22.9° (18.1°-27.2°)

Table 3. Feedback delay group comparison of rates of change and asymptotes for the rotated phase. Rates of Change (RofC) and asymptote values shown for reach training trials with cursor feedback, and implicit no-cursor trials. Average aiming responses across the eight aiming trials in the second half of the rotated phase are also shown above.

to maintain a 1.8-second hold at the end of the reach, and had to restart the hold if it was broken before 1.8 seconds had elapsed. These two groups had equivalent durations of trials and the whole experiment, with the only difference being that the delay → terminal group had to wait after reach completion before seeing the terminal feedback. According to previous studies (Brudner et al., 2016; Song et al., 2020; Tsay et al., 2023), this should lead to more explicit adaptation, and hence perhaps would also lead to less, or slower, implicit adaptation. We compare these two experimental groups with the previous terminal group that had no delays, as well as with the overall control group.

Delays in feedback have been proposed to elicit only strategic compensation, and not engage in implicit learning. Here, we test this hypothesis by again measuring implicit aftereffects after every reach training trial where the cursor only appears 1.2-seconds after the reach is completed. Again from our bootstrapped parameters, we obtained 5,000 values for each group and computed their confidence intervals (Table 3). We then calculated 25 million difference scores to compare the groups and derived a 95% confidence interval for the difference between them. If this interval includes 0, the difference is not considered significant in this dataset. Seeing that the feedback type experiment found an effect of terminal feedback on the asymptote of implicit reach aftereffects, this experiment asks if there are any additional effects of delays when combined with terminal feedback. Therefore, we compare the two new groups to the previous terminal group, and find no effect on overall adaptation for both RofC [95% CI for terminal and delay → terminal (-0.027, 0.318); terminal and terminal → delay group (-0.036, 0.289)], and asymptotes [95% CI for terminal and delay → terminal (-7.231, 1.733); terminal and terminal → delay group (-0.036, 0.289)] (Fig 9A, Table 3). As illustrated in Fig 9B, we find a similar absense of a difference for the implicit no-cursors across the different terminal conditions (blue curves), for both RofC [95% CI for terminal and delay → terminal (-0.085, 0.0802); terminal and terminal → delay group (-0.557, 0.062)] and asymptote [95% CI for terminal and delay → terminal (-2.852, 6.241); terminal and terminal → delay group (-4.432, 4.933)], as shown in Fig 9C-D and Table 3. In summary, adding a delay before the cursor feedback did not reduce the implicit reach aftereffect, nor did it slow the rate at which these implicit changes emerged.

Then, we investigated if there was an effect of delays on aiming, and interestingly we do see one (BF_{10} = 167.4, as seen in Fig 9). Follow up tests reveal that with terminal feedback without any kind of delay, the re-aiming responses are larger than in the terminal → delay group (BF_{10} = 3.520), whereas there is little evidence for a difference or equivalence between the terminal group and the delay → terminal group (BF_{10} = 0.417). That is: adding a delay after the terminal feedback seems to make adaptation *less* explicit (Fig 9).

Finally, we test explicit adaptation as a predictor of implicit adaptation. The two groups with delays again show no linear relationship between implicit and explicit adaptation [terminal → delay: r=-0.134, p=0.418, slope: -0.093 CI (-0.324, 0.137); delay → terminal: r=0.090, p=0.663, slope: 0.060 CI (-0.222, 0.343)].

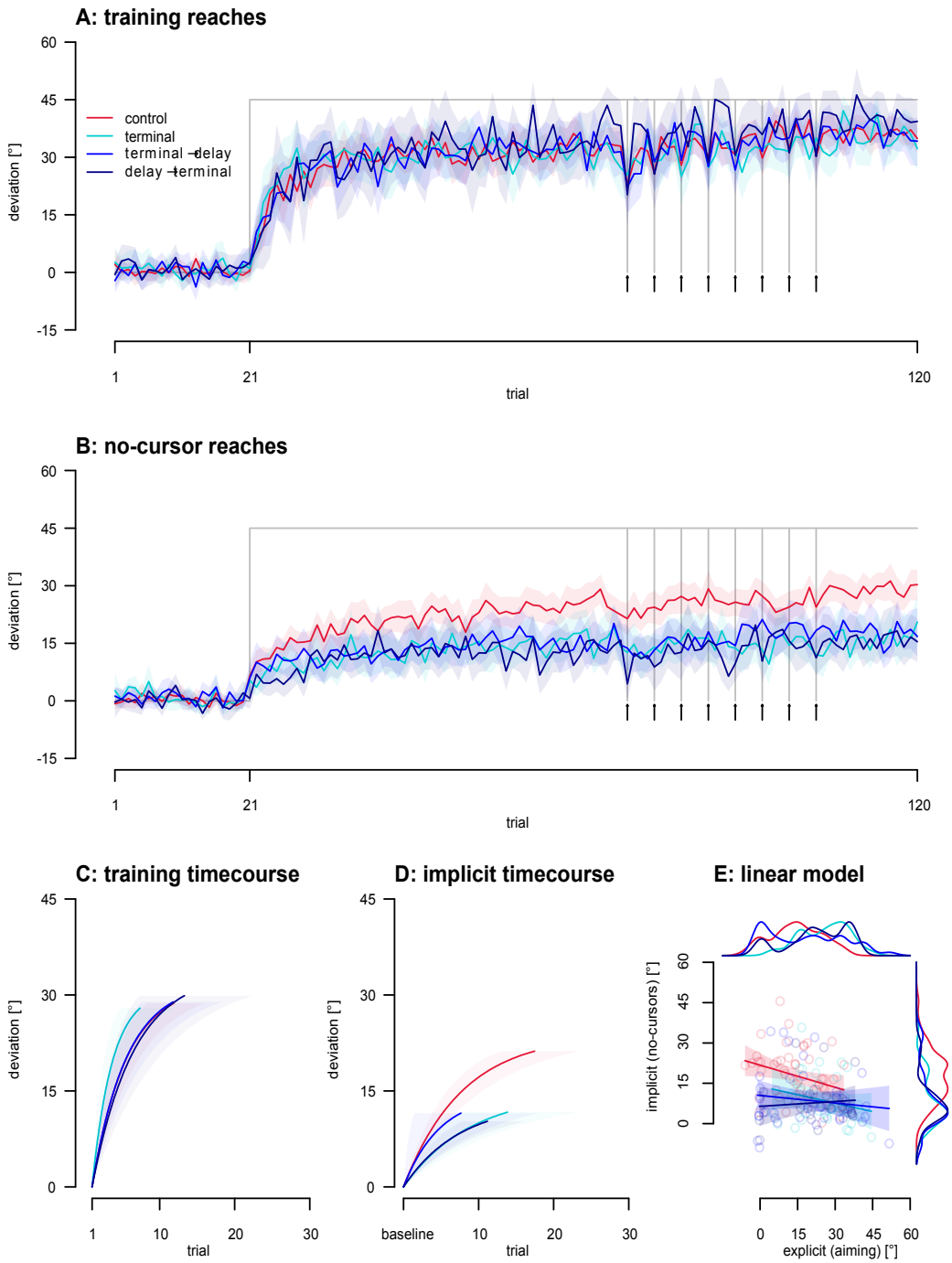

Fig 7. *Feedback Delay. Shaded regions indicate 95% confidence intervals.* ***A.*** *Reach adaptation across trials, with eight aiming trials in the rotated phase indicated by arrows and vertical lines.* ***B.*** *Implicit reach aftereffects across trials* ***C.*** *Fitted exponential functions for reach adaptation in the rotated phase.* ***D.*** *Fitted exponential functions for implicit reach aftereffects in the rotated phase.* ***E.*** *Scatter plot with regression lines depicting the relationship between implicit and explicit learning processes. Each dot represents a participant.*

The Effect of Continuous Aiming (n=37)

Previous work from our lab showed that aiming trials throughout a visuomotor adaptation paradigm can lead to more explicit adaptation ('t Hart et al., 2022). This is why we avoided using aiming trials until after adaptation was close to saturation. However, to test if this assumption is true and to see if explicit adaptation is indeed faster than implicit adaptation, we also included a continuous aiming condition. Instead of participants conducting 8 aiming trials late into the rotated phase like in all previous experiments to measure explicit strategy, this condition introduced consistent aiming trials throughout the rotated phase (Fig 1E). Participants performed an aiming trial, followed by a reach-training trial and a no-cursor trial in a repeated pattern. Thus, we had a single experimental group in the continuous aiming version of the experiment that adapted to a 45° rotation to examine the time-course of both explicit strategy use and development, and implicit adaptation.

Given that we find no relationship between the extent of implicit aftereffect and the magnitude of explicit aiming in the later half of training, we want to next compare the time courses of these two components. Thus, we ran a group where we measured both aiming and

		Control (N=51)	**Aiming (N=37)**
Reach training	RofC	19.8% (14.8%-27.9%) 6.2°	19.0% (15.2%-24.2%) 6.9°
	asymptote	31.5° (28.7°-34.5°)	36.3° (34.0°-38.6°)
No-cursors	RofC	12.0% (9.0%-15.8%) 2.8°	15.8% (10.7%-24.0%) 3.3°
	asymptote	23.7° (21.2°-26.5°)	20.8° (17.7°-24.0°)
Re-aiming	extent	14.4° (11.7°-17.0°)	24.3° (19.9°-28.2°)
	RofC		13.0% (11.0%-25.1%) 3.3°
	asymptote		25.1° (20.9°-29.5°)

Table 4. Continuous aiming group comparison of rates of change and asymptotes for the rotated phase. RofC and asymptote values shown for reach training trials with cursor feedback, and implicit no-cursor trials. These values are also shown for the rate and asymptote of aiming responses themself. Average aiming responses across the same eight aiming trials in the second half of the rotated phase for both groups are also shown above under extent.

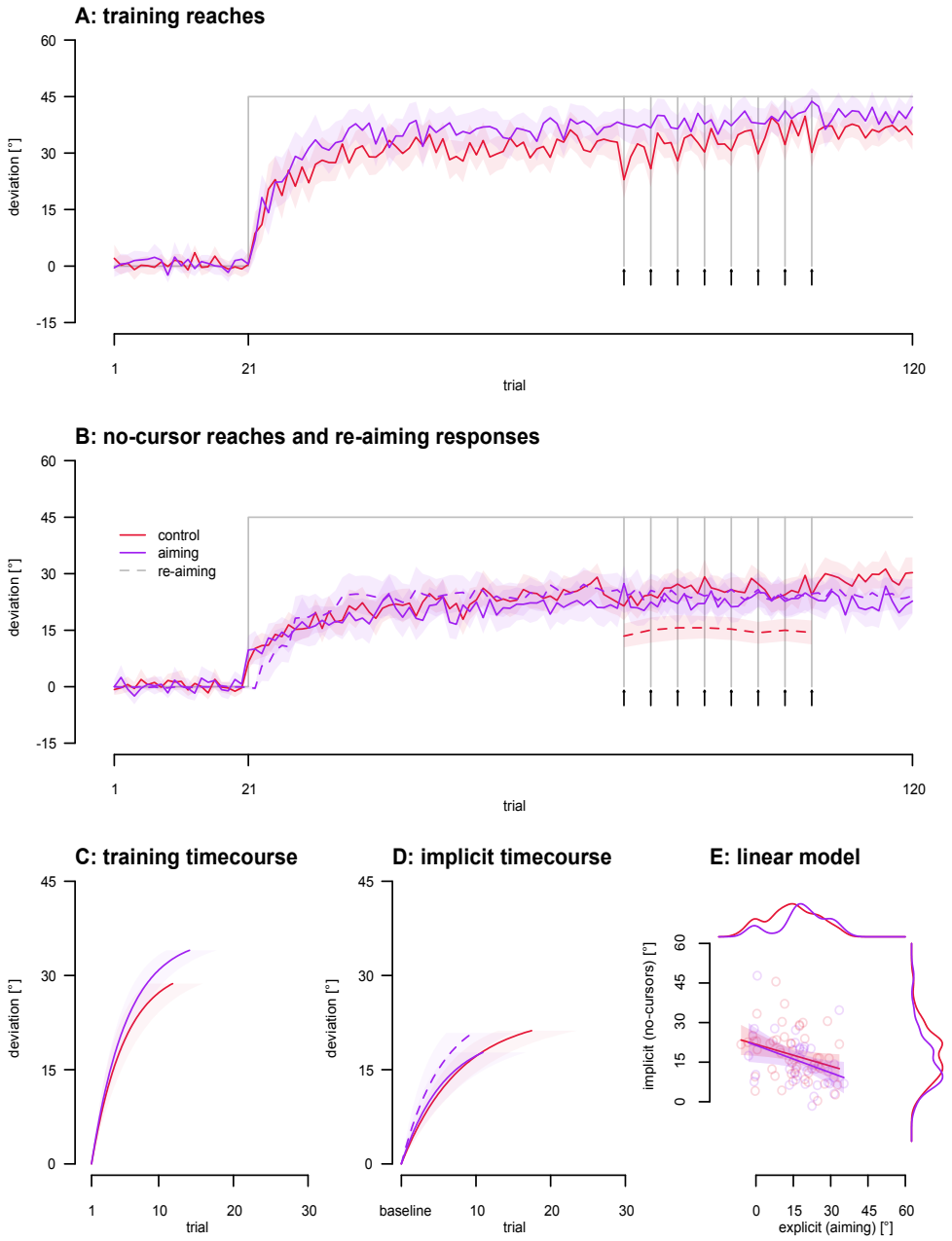

Fig 8. *Continuous Aiming. Shaded regions denote 95% confidence intervals.* ***A.*** *Reach adaptation across trials, with eight aiming trials indicated by vertical lines and arrows.* ***B.*** *Implicit reach aftereffects across trials (solid lines) and explicit re-aiming for the continuous group (purple dashed lines) and the control condition (red dashed line).* ***C.*** *Rate of Change for rotated phase of Reach adaptation* ***D.*** *Fitted exponential curve for implicit reach aftereffects in the rotated phase, and for the explicit re-aiming in the continuous group (purple dashed curve).* ***E.*** *Scatter plot with regression lines depicting the relationship between implicit and explicit learning processes. Each dot represents a participant.*

reach aftereffects continuously through training. Bootstrapping our parameters, we derived confidence intervals for each group (Table 4). Then, by calculating difference scores, we obtained a 95% confidence interval to compare the groups. If this interval includes 0, the difference is not considered significant in this dataset. This group, continuous aiming, seems to increase the extent of overall adaptation (Fig 8A&C, Fig 9B and Table 4) with an asymptote of 36.3° (34.0°-38.6°) compared to the control of 31.5° (28.7°-34.5°), yet it does not affect the speed (Fig 9A). Moreover, it does not affect the time-course of implicit reach aftereffects, as shown by the solid lines in Fig 8B&D and in 9C and Table 4. Additionally, in this group, we were interested in the time course of aiming responses themselves. The time course of reported aiming directions before each trial is depicted by the purple dashed line in Fig 8B&D. We find no differences in the RofC of explicit cognitive strategy (dashed purple lines in Fig 8B&D) compared to implicit no cursor RofC in the control or continuous aiming group (solid lines). Specifically, as listed in Table 4, for the continuous aiming group, the RofC for explicit re-aiming was 15.8% (10.7-24.0%) and 13% (11.0%-25.1%) for the implicit reach aftereffects; this led to a fitted change in deviation after the first training trial of 3.3° for both implicit and explicit measures. Thus, with the current data set and approach, we can not detect a difference in how quickly implicit or explicit adaptation emerge, so it is possible they might be equally fast in this group. That is, implicit and explicit contributions to adaptation emerged simultaneously and at the same rate.

For our next analysis, we wanted to see if there is a difference in the reported aiming direction between the continuous aiming group and the control group. Analyzing the 8 aiming trials during the latter portion of the rotated phase, we compare the control group of participants performing aiming trials only 8 times (red dashed line in Fig 8B) with those that do aiming trials throughout the whole experiment (purple dashed line in Fig 8B, see Fig 9E too). Our findings indicate no difference in explicit strategy between these participant groups (BF_{10} = 1.242). This contrasts with the 95% confidence intervals in Table 4, which do not even overlap. Notice however that strategies are not really normally distributed, but tend to cluster at specific magnitudes (figure 3E, 5E, 7E and 8E). Moreover, while adding continuous aiming may evoke strategies in some participants who would not have discovered one themselves, a majority of participants in the control group did have a strategy already.

Lastly, we test explicit adaptation as a predictor of implicit adaptation for the aiming group. There seems to be a significant, but non-additive relationship between measures of implicit and measures of explicit adaptation (r=-0.361, p=0.028, slope: -0.347 CI (-0.656, -0.039)] as illustrated in Fig 8E In other words, consistent with the results above, the implicit reach aftereffect did not consistently vary with the magnitude of explicit strategies, as would be expected if the implicit component were merely the residual difference between overall adaptation and the strategy used.

Discussion

Our study sought to investigate the time-course of implicit adaptation during classical visuomotor adaptation using interleaved no-cursor trials to gauge if implicit adaptation is 1) a

slow process in adaptation, 2) affected by rotation size, 3) modulated by conditions that (mostly) increase explicit adaptation, and 4) linearly additive with explicit adaptation. Using no-cursor reach aftereffects after every reach training trial, we can map out the speed of implicit adaptation with high temporal precision. All the non-control conditions in experiments 2 through 4 were used to evoke more explicit strategies. Our results challenge the traditional notion that implicit adaptation is a slow and gradual process, as we found that implicit learning processes emerge much faster than previously assumed. Using interlaced no-cursor trials, we validate the efficacy of this method through the expected effects of increasing rotation size. We observed that cursor-jump feedback and terminal feedback primarily enhance explicit adaptation while having minimal impact on the speed and asymptote of implicit adaptation. For the group that did continuous aiming reports, we find no discernible effect on implicit adaptation, highlighting a parallel development of implicit and explicit adaptation at comparable speeds. The rapid emergence of aftereffects we found was robust to our various feedback types and the aftereffects of implicit learning were observed to develop within the first few training trials, and reaching asymptote within 20 training trials for all conditions and as few as 10 trials for half of our conditions. This indicates that unconscious adaptation can occur very rapidly.

Our experimental approach represents an advancement in the study of implicit learning during *classical* adaptation by introducing key improvements in measurement and analysis. Like in our previous three papers (Ruttle et al., 2016, 2018, 2021), we consistently measure aftereffects to capture the residual deviations in reaching movements even after the feedback is removed or returned to normal, providing a more comprehensive approach that ensures a robust assessment of the natural time course of implicit adaptation. Unlike traditional approaches of calculating implicit learning during classical visuomotor adaptation that relies on subtracting explicit contributions from the overall learning effect, we used independent and direct measurements, i.e. not relying on subtraction. By avoiding the subtraction method, which assumes additivity between the two processes, we can better elucidate their individual contributions and potential interactions. This refined approach accounts for the complex interplay between implicit and explicit processes, which is unlikely to be additive. Although our study did encounter challenges, such as the exclusion of certain participants due to lower performance, these limitations do not overshadow the strength of our experimental method. The inclusion of no-cursor trials may have introduced some interference with overall adaptation. Ruttle et al. (2021) found that interleaving no-cursor trials led to a lower asymptote at 77%, compared to 96% when there was no interleaved reach but just a gap in time. This suggests that interference from no-cursor trials, or even time between successive trials, likely had only minimal effect on overall adaptation, and cannot be the sole determining factor in our findings. However, in the previous study we observed higher rates of change, and that study used passive movements for the return to home in all trial types, whereas here we used active movements. Since the previous study showed that active interlaced movements reduced learning somewhat, this may explain some of the differences in findings between our two studies. Importantly, we did not include any groups without no-cursors, so that the effect of the interlaced no-cursor trials was present in all groups. Finally, this should be taken into account when making comparisons with other research.

In our study, we also investigated the relationship between rotation size and the time-

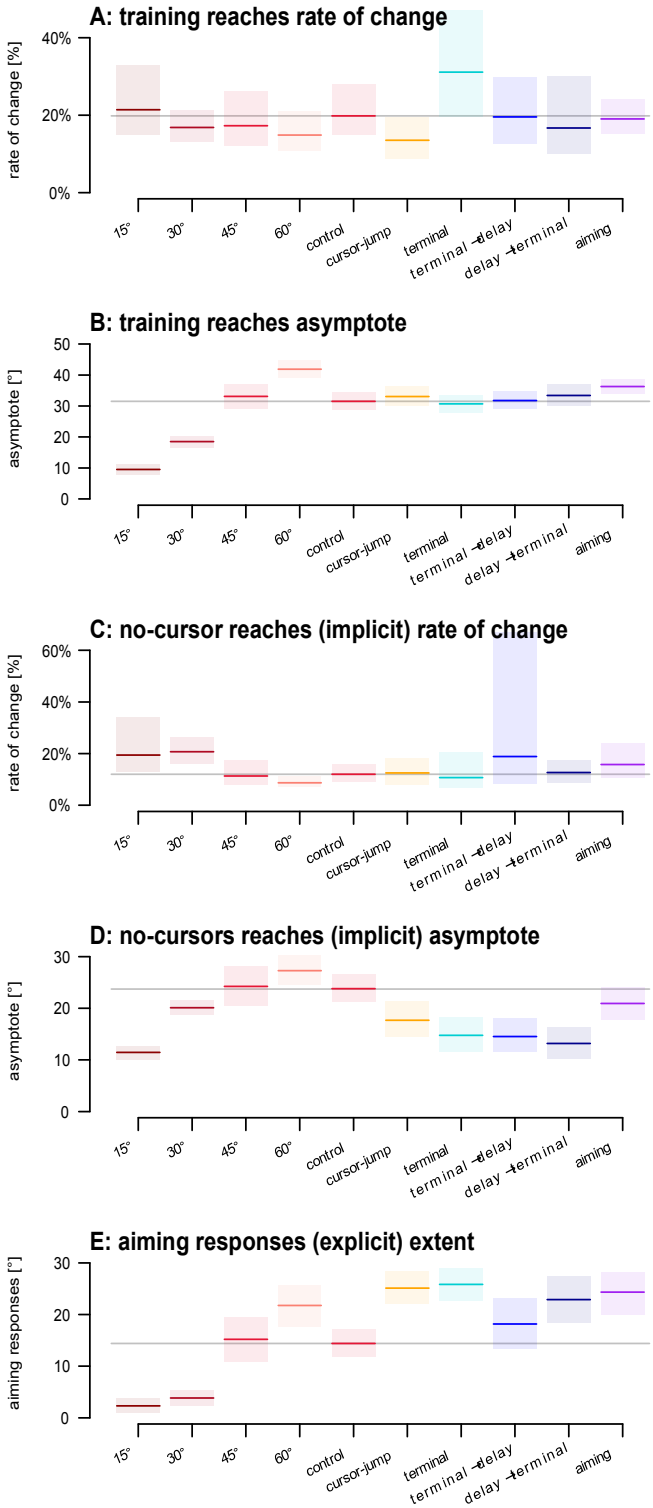

Fig 9. *Summary figure of all groups and adaptation indicators.* ***A.*** *Reach training rates of change.* ***B.*** *Reach training asymptotes.* ***C.*** *Implicit no-cursor rates of change.* ***D.*** *Implicit no-cursor asymptotes.* ***E.*** *Aiming extents from aiming trials within the groups.*

course of implicit learning. Not surprisingly, larger rotations led to a proportionally larger overall adaptation extent. However, in contradiction to Bond & Taylor (2015) and our own lab (Modchalingam et al., 2019), we found that the extent of reach aftereffects did vary a bit with rotation size during training, but the difference was only 7° for rotations between 30° and 60° rotations, as compared to 24° of difference for total adaptation. The rate of change or absolute time-course in degrees, both during rotated-reach training, and during subsequent washout with a veridical cursor, did not clearly vary with the size of rotations between 30° and 60°, as illustrated in Fig 3B and D. Only training with a small rotation like 15° led to any differences, which is not surprising.

Comparing the time-course of aftereffects *during* training for different types of visual feedback offers valuable insights into the factors influencing the progression of implicit learning. Our findings support and extend the work of Ruttle et al. (2021) who also examined aftereffects throughout early reach training. Our study demonstrated continuous reach aftereffects at a rate of change of 20.7% (CI 16.1-26.4%), in contrast to the 56.9% (CI 27.4–58.5%) reported by Ruttle et al. (2021). Despite this variability, both of our works challenge the traditional notion of slow implicit adaptation, indicating that it *still* occurs at a notably faster pace. Furthermore, we delved into the influence of feedback type, revealing that terminal and cursor jump feedback both led to smaller implicit reach aftereffects than continuous feedback, suggesting potential competition between explicit strategy engagement and implicit adaptation (Albert et al., 2022). Like others, we found that terminal feedback lowered the extent of implicit adaptation, perhaps due to its limited visual cues (Taylor et al., 2014, Barkley et al., 2014, Hinder et al., 2008, Brudner et al., 2016, Schween & Hegele, 2017). However, our observations highlight that implicit adaptation can still rapidly emerge within this context. While we did not find a slower rate for cursor jump, we did replicate the finding in Gastrock et al. (2020) of reduced aftereffects. Additionally, our investigation into feedback delays indicated that the timing of visual information does not substantially affect the extent of implicit adaptation or the rate of its emergence, suggesting a degree of robustness to timing variations. However, our observation regarding the extent of implicit aftereffects contradicts prior research (Brudner et al., 2016; Tsay et al., 2023, Song et al., 2020), in that delayed feedback leads to similar magnitudes of implicit learning. Imperatively, terminal (with and without a delay) and cursor jump feedback do not influence the rate at which implicit adaptation unfolds.

While our study primarily focused on implicit components of adaptation, we also examined the extent of explicit adaptation. We used an aiming task to determine the explicit contribution to adaptation across our feedback types, and our results suggest that error feedback type (terminal & cursor jump) can increase the amount of explicit control over the task, aligning with previous research (Taylor et al., 2014, Gastrock et al., 2020). Taken together with our measure of aftereffects, we now had direct measurements of both implicit and explicit processes. After performing linear regressions on this data we consistently found a non-additive relationship between implicit and explicit, in line with recent work from our lab ('t Hart et al., 2022). Expanding on aiming, our experiment also explored how taking frequent explicit measurements can affect the progression of implicit learning. Exploring this continuous aiming, we find that explicit aiming judgments do not impact the rate of implicit learning, while it slightly increases the overall extent of adaptation without affecting its speed. The rapid use of cognitive

strategy suggests that consistently reporting an aimed location can facilitate similar rates of implicit and explicit learning. This phenomenon should prompt us to be mindful of their interaction when exploring new avenues of work in motor adaptation. The speed of explicit processes has been extensively explored in the field and is generally agreed to be remarkably fast (McDougle et al., 2015, Taylor et al., 2014, Smith et al., 2006, Huberdeau et al., 2015). Consequently, our study highlights the importance of considering the speed of implicit adaptation, and that further exploration of implicit learning is warranted.

Additionally, we see in all four experiments that the distribution of the amount of implicit adaptation seems predominantly uni-modal, whereas the level of explicit adaptation may follow a multi-modal distribution. A portion of participants seems not to develop any strategy, whereas others have strategies that fall in clusters. We observed something related previously ('t Hart et al., 2022) so this is not wholly unexpected. However, instead of speculating on it, we will leave this phenomenon for future investigation.

In conclusion, our study challenges conventional assumptions about the time-course of implicit adaptation during visuomotor tasks. We provide evidence that implicit learning can occur rapidly within the initial stages of training, across different feedback conditions, rotation sizes, and feedback delay timings. We also find that the speed of implicit adaptation was indistinguishable from the speed of explicit adaptation. This has important implications for our understanding of how motor learning processes unfold and interact, and the complex synergy between implicit and explicit components of adaptation. Further research in this direction could offer insights into optimizing motor learning interventions and training strategies.

Data Availability

Data and analysis are available on Open Science Framework (https://osf.io/ajwyr/).

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Contributions

B.M.tH. and D.Y.P.H. designed the research. S.D. collected the data. S.D., J.E.R., and B.M.tH analyzed the data. S.D. wrote the manuscript, S.D, B.M.tH. and D.Y.P.H edited and revised the manuscript. All authors reviewed and approved the manuscript.

Competing interests

The author(s) declare no competing interests.

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