**Responses to Reviewers:**

Manuscript number: eN-RGR-0265-20

Manuscript title: How movement variability constrains locomotor use-dependent learning

Date:

*We thank both reviewers for their helpful comments. We have edited the manuscript accordingly. Below, please find our point-by-point responses to all the comments in italics. We also identify the location (lines) of all edits in the tracked-changes version of the manuscript.*

**Responses:**

**Main Request:**   
It would be helpful to see the learning and washout time series for the 2 subjects that they tested in the constant condition and the 3 in the high variability condition. The preliminary data that are shown only give the mean and std during of SAI during learning. This does not illustrate the time course of learning and the time course of washout trials, the latter of which is their main outcome measure based on model predictions.

*We have now expanded the pilot data figure (Figure 4) to include stride by stride data for all participants. This figure now provides a clear illustration of the time courses of the Learning and Washout phase as the reviewers rightly point out. We provide the all individual data and the mean for each condition across strides. We have truncated the data so that each phase is of equal length. Two participants completed both the high variability and the stable condition. One participant completed the high variability condition only; however, there was a technical error during the learning phase, and therefore we have included the data for this participant up to the point where the technical issue occurred.*

**Reviewer #1:**

1. Authors need to indicate more explicitly in the methods in what way their theory is distinct from the one proposed in Diedrichsen et al. 2010. In other words, is it the exact same theory but just adapted to locomotion?

*Please see our response to R2 comment #3. [Seems weird to reference comment to R2. Should be other way around, no?]*

1. Authors need to indicate more clearly what are the distinct predictions from these two models upon changes in the consistency of the task.

*We have now added a sentence in the introduction (lines 44-45, & 52) to make the specific model predictions relative to the current proposed study clear. To address R1’s comments #17 and #18, we added similar clarifications to the model-based methods section (lines 283-284) and the statistical analysis section (lines 290-292), respectively. These adjustments should help readers understand the distinct predictions of each model relative to the current study.*

1. This is not a good idea. The authors should overlay the kinetic and kinematic events to realize that they are not equivalent time points.

*We now plan to perform event detection with kinematic markers only using the velocity-based tracking algorithm described in Zeni et al (2008). We have removed references to kinetic data collection, post-processing, and analysis (lines 129-130 & 145-150) and modified the Proposed analysis pipeline section (lines 144-145).*

1. Authors should consider reporting the asymmetry in leading and trailing legs. This will help the reader gain an insight on their use-dependent learning task. Many people have done this decomposition. As an example see Sanchez et al. 2020 Using asymmetry to your advantage: learning to acquire and accept external assistance during prolonged split-belt walking. doi: https://doi.org/10.1101/2020.04.04.025619

*We thank the reviewer for this suggestion and we have now added this analysis to our Proposed analysis pipeline section (lines 150-153). Although, we will add markers for the bilateral greater trochanter and the bilateral lateral knees (lines 133-134) so we can accomplish this analysis when we perform the experiment. We have also added the specific analysis of leading and trailing leg asymmetry to the Proposed analysis pipeline section (lines 150-151). We now plan to report this analysis once the data collection is complete so the reader can gain insight on the use-dependent learning task either in a figure (lines 153-154).*

1. Consider defining SAI in terms of long and short step length to help the reader contrast the authors results to previous findings.

*This change has been made to equation 1. A sentence is added to the proposed analysis pipeline section (line 160) for clarification.*

1. Authors should indicate that by design, the asymmetry is always positive.

*We now add a statement to this effect in the proposed analysis pipeline section (lines 165-166).*

1. Authors should zoom in the early part of the learning period for the reviewers to appreciate this. Also, authors should present the learning results under the different conditions during the early period to evaluate the extent to which the predictions from the model equally match the learning and washout periods.

*We have now added insets to the simulations plot for the initial learning phase to demonstrate that the models do not make qualitatively distinct predictions for this phase. We have also added plots to the pilot data figure (Figure 4) to demonstrate that the model predictions accurately match the early Learning behavior in the pilot data. We describe this in the stimulation section (lines…).*

1. Authors should indicate explicitly that they are fitting one single set of parameters to the learning and washout periods.

*We have added this description to the Computational Models section of statistical analysis (lines 371-373).*

1. Aren't errors and motor output directly related according to eq. 3? It is unclear why the authors indicate that the use-dependent component is not updated as a function of the error signal.

*The reviewer is correct with regard to how use-dependent learning is impacted by the error signal in the context of this study. In the Strategy + Use-dependent model, use-dependent learning depends on previous motor output and occurs in parallel to updates based on an error signal (Diedrichsen et al., 2010). The error signal directly drives strategic learning, and due to the interactions between strategic and use-dependent learning, impacts the use-dependent process. However, in the absence of an error signal (e.g., an individual chooses to walk asymmetrically in the without a specific goal or external target), the use-dependent learning process would still be active, given that it learns from previous motor output, regardless of whether the motor output changed due to an error signal or not.*

*As the phenomenon we are trying to capture in the use-dependent process is the pure repetition effect absent any error, we chose to emphasize that component in the text. However, we see how the way we stated the unique features of the model was not clear andadjusted the Model Based Methods section (lines 233-234) to address this comment. It now reads...*

1. Unclear why authors chose a fixed sensitivity (learning rate F) to the update rule, given literature indicating that the update of motor memories depends on the consistency of the task. For example, see the work of Maurice Smith (Gonzalez-Castro LN\*, Hadjiosif AM\*, Hemphill MA & Smith MA (2014). Environmental Consistency Determines the Rate of Motor Adaptation. Current Biology 24, 1050-1061.) or Reza Shadmehr (Herzfeld and Shadmehr. A memory of Errors in Sensorimotr Learning 2014). While this literature focuses on adaptation processes, it is unclear why use-dependent plasticity won't be also affected by consistency in the "teaching" signal, in this case motor output.

*The reviewer raises an interesting point, one which we have addressed in the main text now…With regard to the Strategy plus Use-Dependent model, we believe the lack of an extra sensitivity term in the use-dependent process is a core feature of the model and is why we have pit this model directly against the Adaptive Bayesian model, which is sensitive to consistency. The basis for a fixed learning rate comes directly from Diedrichsen et al 2010, and specifically, data from experiment 3 of their paper is particularly instructive. There, according to their model, participants demonstrated a robust use-dependent bias in response to a velocity-dependent force field (Fig. 3H), even during the early trials in which the movement angles are highly variable. Interestingly, the use-dependent learning rate was not lower during this experiment than in the other two experiments from the paper, even though the other experiments induced use-dependent learning through more consistent movement patterns. While this evidence is indirect, this suggests that under certain conditions, use-dependent learning may not be sensitive to consistency. We believe that this controversy over how sensitive use-dependent learning is to the consistency of movement is best tackled directly and forms the primary motivation of our study and our choice of models.*

1. Unclear stability of the model

*To obtain stable model parameters we bootstrapped parameter values from the acquired dataset 1000 times. We explain this fitting process, in the simulations section (lines 459-461).*

1. This seems arbitrary. Authors should provide a better justification or sensitivity analysis.

*We appreciate the reviewer’s comment and have provided a clearer justification in the Model Based Methods section (lines 240-244). There we provide a rationale for why we chose this constraint and the empirical evidence for a slower use-dependent learning rate than strategic learning (F and C, respectively). Briefly, Taylor and Ivry (2011) showed that humans can quickly adjust strategic aiming, and can reach learning rates that are even close to 1. Given that strategic aiming is much faster than implicit adaptation, which typically has estimated learning rates between 0.10-0.30, and that implicit adaptation is much faster than use-dependent learning (somewhere on the order of 0.05, as shown in Diedrichsen et al. 2010), we reasoned that strategy must be many times faster than use-dependent learning. We also note that removing this constraint does not qualitatively change the quality of fits or relationships between the learning rates, in terms of strategy being much faster than use-dependent learning.*

1. Authors should reformulate this equation such that it only depends on the prior and likelihood variances. This will make it easier for the reader to relate to prior maximum likelihood frameworks such as the paper by Ernst and Banks (for example). More importantly, how is target location and motor output related?

*We now reformulate equation 6 as noted by the reviewer (line 256). We also remove the equation for the posterior variance as this is now incorporated into equation 6.*

*In the Adaptive Bayesian model, we assume that the maximum a posteriori (MAP) estimate represents the brain’s estimate of the target location. We assume that the motor output is a direct readout of this estimate as in Verstynen and Sabes, 2011. We describe this assumption in the Model Based Methods section (lines 253-255).*

1. This is unclear.

*Please see the first part of our response to R1 comment #13.*

1. Is the likelihood variance the same during the learning and washout period? If so, authors need to justify why given that the sensory information is quite distinct during these two experimental periods.

*We thank the reviewer for this comment regarding the likelihood function, and we have now added a justification in the main text. To summarize, as the likelihood function represents the sensory estimate of where to step, this is based on the visual target information provided during Learning. During washout, there is no visual target provided, instead, the “target” step length is the return to normal walking (i.e., baseline (a)symmetry). While it would be possible to fit two separate likelihoods to the different conditions, parsimony dictates that only one likelihood function is necessary if we assume that sensory uncertainty around target step lengths is the same during both conditions. (We note here that if we had asked participants to do anything other than return to normal walking during the washout, we would want to fit separate likelihoods). Given that the target step length is their usual walking pattern, we believe this assumption is justified. Put another way, see the baseline target linesas compared to a condition in which thevisible, nor would we expect the level of uncertainty about where to step to change*

1. Authors need to provide a rational for their proposed update rules for the prior distribution of the target step length

*have added a rationale for the adaptive priorsWe now make clear that the adaptive priors express one way that the brain may adjust its belief about the consistency of the environment as more data (evidence) arrives—in other words, how the brain learns new priors. As empirical support for this view, we cite Verstynen and Sabes 2011, where they show that use-dependent learning is more accurately modeled using adaptive priors versus their normative Bayesian model in which prior variances were “hand-tuned” to match the target variance, an assumption the authors explicitly state as likely not being correct.*

1. While authors explain in here the computational differences between the two hypothesis, the distinct predictions from each of these models need to be explained more explicitly.

*Yes, we agree and have made these distinctions more explicit in the text. Please see our response to R1 comment #2.*

1. Authors need to indicate more clearly, what are the distinct predictions from the two models in this section.

*Please see our response to R1 comment #2.*

1. Authors should validate their models by contrasting the distinct predictions from each against empirical data. This will be more convincing than AIC.

*We agree. We plan to compare the model predictions with behavioral data as the primary validation of the correct model once data are collected. As the reviewer states, this is the best way to determine which hypothesis is correct and will be especially convincing if we see strong qualitative differences between the congruence of each models’ predictions relative to the data. We describe additional analyses to test our hypotheses in the Behavior subsection of Statistical Analysis section. We have now changed the order of the Computational Models and Behavior subsections as well as added clarifying language to the Statistical Analysis section to ensure that we are not over emphasizing our model fitting analyses (lines 297-303). However, we do still plan to perform objective model comparison using AIC as this will allow us to test how the relative quality of each model’s fits to behavioral data. This analysis should complement the analysis of behavior and we expect the results of this analysis will align with the behavioral analysis. We offer further statistical analysis of the AIC values as confirmation for the correct hypothesis/model.*

1. While this is ok for quantifying the fit of the data, authors should consider a different approach if they are truly interested in contrasting the two hypothesis that they present. In principle they have two contrasting theories that provide distinct predictions. Authors will presumable test these predictions experimentally. The results will match one theory better than the other. This will be more convincing for selecting the model that underlies use-dependent plasticity in locomotion, as opposed to AIC.

*Please see our response to R1 comment #19 above.*

1. I might have missed this, but I did not see the rational for this expectation.

*We have bolstered our rationale There, we state that the variability of SAI behavior during the Learning phase should change as a function of the target variability. Put more simply, we expect behavior to follow the on-screen targets during Learning. If this is true, the mean SAI behavior for the entire Learning phase should be similar across all conditions (as the mean target location is the same across conditions), but the standard deviation of the SAI behavior measured for the entire Learning phase should be different across phases. Participants should demonstrate the least amount of SAI standard deviation during the Constant condition, the second highest amount of SAI standard deviation during the Low Variability condition and the highest amount of SAI standard deviation during the High Variability condition. Validating that there are indeed different amounts of training consistency (centered around similar means) will allow us to confidently say that aftereffects either do or do not depend on that training consistency.*

1. Revise. As of now it is unclear if Authors have done (or will) correct for multiple comparisons.

*We have revised to indicate that we plan on correcting for multiple comparisons (lines 350-351).*

1. Authors should submit their paper once they can validate their models. Even if this is a Stage 1 Registered Report, as of now, the study is not complete and does not add to the current theories of processes underlying use-dependent learning.

\*\*\*clarification\*\*\*

1. This section is appropriate for a grant, not for a journal paper! Please revise.

\*\*\*clarification\*\*\*

1. Authors should consider removing this analysis. It is more convincing to observe distinct predictions from each model.

*We appreciate the reviewer’s comment and hope we have addressed the overall concern regarding testing distinct predictions in our responses to comment #’s XX. Our rationale for including the model recovery analysis, in addition to the more direct analyses of our models’ predictions, is to ensure that the models can indeed be differentiated under ideal circumstances (i.e. when the models themselves generated the data). It can also help determine which method of objective model comparison is best to use in a given circumstance (i.e. with these specific models in this specific experimental paradigm). The model recovery process starts with generating ‘fake’ datasets for each model using random parameter values. Next, each dataset is fit by both models and some measure of objective model selection criteria is generated (AIC for example). The model that is fit better for that iteration of fake data according to the objective model selection criteria is recorded. After this analysis is complete, the confusion matrix provides a summary of this process with the probabilities that the model which generated the data, fits the model best. We performed this process for both AIC and BIC, and, in this specific case, AIC demonstrated better ability to distinguish the models compared to BIC. We feel that it is necessary to include this analysis because we are proposing to compare two models. We have changed the name of this section to “model recovery” which is the more accurate term. We have also made significant adjustments to our explanation of what this analysis is and what it entails. We believe these changes also help address comments #27-29. (lines 409-440).*

1. Model fits implies that the parameters were fit to data. It is unclear if this was the case.

*Here we are fitting simulated data from both models as described in Wilson and Collins (2019) who use similar terminology. We adjusted the phrasing in this section (lines 417 &422) to make sure this is clear.*

1. What "objective model comparisons"?

*We are now more specific about what objective model comparisons we are using throughout this section.*

1. Typo

*This typo has been removed.*

1. Not always true. This statement is not substantiated.

*Please refer to our response to R1 comment #25.*

1. This seems incorrect. While the authors have made the point regarding the slow dynamics of the use-dependent process in their S+U model, they fail to explain why the sensitivity to previous motor output (F parameter in eq. 5) will not be affected by variable targets.

*Please refer to our response to R1 comment #10.*

1. Authors need to expand this explanation. What kind of data, how many samples, same protocol as this one or not, etc?

*Data were obtained with permission from experiment 2 of Wood et al. 2020 (n=16). These data provide a similar magnitude of asymmetry learned with both abrupt and gradual perturbations and a similar washout phase. These details have been added to the Simulations section (lines…).*

\*\*\*ask for clarification\*\*\*

1. What about the aftereffects for these two conditions?

*Please refer to our response to the main request.*

1. While this is a stage 1 submission for a registered report in eNeuro, the manuscript is not ready for publication. I suggest that authors include preliminary data of the aftereffects. Since as of now, it is quite challenging to evaluate the merit of the proposed theories.

\*\*\*ask for clarification\*\*\*

*Please refer to our response to the main request.*

**Reviewer #2:**

This is a well written stage 1 registered report that proposes a design to test whether and how movement variability (here variability in step asymmetry) affects a form of use dependent locomotor learning. The paper largely relies on a behavioral paradigm that was described in a recent article by Wood et al. 2020 and two different computational models. The two models are shown to respond differently to increased variability-one is a use dependent model with a strategic component added on, and the other is a Bayesian model.  
  
I have no major concerns about the hypothesis being tested-- it is interesting and timely. However, there are a few things that would be worth thinking through a bit more, or justifying a bit better, within the design.

We thank the reviewer for their encouraging words regarding our study…

1. The Wood et al. 2020 paradigm used a gradual introduction of a perturbation during learning and the experimental design proposed here uses an abrupt change during learning. These can result in different after-effects in other types of motor learning paradigms for walking (e.g. adaptation). Are there reasons to think that these types of perturbations would be equivalent in this use-dependent experiment? This may influence the power analysis since it is being done using gradual learning data and applied to abrupt conditions. It might not be a problem, but it seems worth thinking about.

*The reviewer brings up an important point. We failed to mention in our original submission that, for the power analysis, the aftereffect magnitudes are based on the Washout phase from Wood et al. (2020), which was performed after a 5-minute abrupt (not gradual) learning phase. Therefore, we do not believe that the fact the first learning phase during Wood et al. was initiated gradually would affect the power analysis in the current proposed behavioral experiment.*

*However, the question of a gradual learning paradigm possibly influencing the use-dependent process is interesting, especially since gradual learning has been used as a proxy for less repetition (i.e. less opportunity for use-dependent process) in previous studies (Leow et al., 2016; Orban de Xivry et al., 2011; Orban de Xivry and Lefèvre, 2015). We simulated a gradual paradigm instead of the abrupt paradigm for the repeated condition and there is little change in the model predictions, the aftereffects for a gradual condition (as long as it is consistently gradual) remains the greatest compared to the low and high variability conditions. This is interesting especially when considered in the context of the Adaptive Bayesian model. It appears that if the targets change gradually but with low amounts of variability the Adaptive Bayesian model still considers this consistent. This may be a theory to test in a future experiment.*

1. I also assume that you switched to an abrupt change so that the model fitting would include both adaptation and de-adaptation. You state that you will model individual subject data, which is appropriate, but I am not clear why you will model all three conditions combined? Wouldn't it be a stronger test if you model one condition for each subject (e.g. the high variability condition where you expect the greatest differences) and then see how those model parameters apply to the other conditions? Can you clarify?

*This thought had crossed our mind when considering the best way to fit the models to behavioral data. We decided to model all three conditions combined for two reasons: First, we want to obtain a single set of parameter values for each individual participant. However, this can also be accomplished by the method the reviewer suggests. More importantly, we felt that choosing a condition as our ‘reference’ condition to fit then compare those parameters to the other conditions, would unduly favor that condition. Using one specific condition as our reference condition means we believe that the model fit to condition acts as our ‘gold standard’ for the use-dependent process. We do not want to assume a gold standard model fit and condition, so we decided to take an unbiased approach and determine which model fits all three conditions the best.*

1. The use-dependent + strategic model seems to be based on the use-dependent model from the Diedrichsen et al. 2010 paper. Correct me if I am wrong, but it adds in an assumption about a strategic component that was not in the Diedrichsen paper-namely that there is a retention factor for the strategy that is assumed to be used from one stride to the next. I would like some more intuitive justification for the need for the strategy component and for fitting the A parameter. In walking, subjects may easily have time to modify the gait pattern online and hit a target, thus they might only need the C\*en part of that equation. It would be nice to understand the basis for the A\*sn component. It is not entirely intuitive. Perhaps it just biases the model in the direction of the abrupt perturbation? More explanation would be useful. Perhaps even a plot showing how the different components of the model change as a function of stride, which might help the reader intuit.

*The reviewer is correct about the changes we made to the model from Diedrichsen et al., 2010. The original model from that paper combines two processes: use-dependent learning and error-based learning. The error-based learning component is based on a force field adaptation task. The force field adaptation task in Diedrichsen et al. is quite different from the one we plan to use in the current study. Previous work has demonstrated that participants learn this walking task through primarily explicit or strategic means and that this task does not provide a robust sensory prediction error to elicit adaptation even when the bars are distorted (French et al., 2018; Wood et al., 2020). For these reasons, we replaced the error-based learning component with a strategic component. We have added more detail about this explanation in the Model Based Methods section (lines 199-202).*

*The strategic component learns from an error signal which is the difference between the target position and the motor output (this is a different error signal than in the Diedrichsen model). The strategic component learns a portion of this error and corrects for it on the subsequent trial which, as the reviewer correctly states is the C\*en term. The A\*sn term represents the ability for the brain to remember or retain prior strategies, not just correct for errors. This is added because when a participant aims for a target, they would remember the general area where they aimed previously. This memory of the previous strategy is not perfect, so they remember only a portion of the previous strategy. The A parameter, specifically, retains a portion of the strategy from one trial to the next. This term has also been added to previous studies of upper extremity reaching studies to model strategy* (Taylor and Ivry, 2011)*. We have added clarification to these points in the Model Based Analysis section (lines 226-228). We also added a panel in the simulations figure to visualize the different components of the Strategic plus Use-Dependent model as a function of stride as the reviewer suggests.*

1. The simulations that you show have a high SD for the learning phase in the groups where variability was added. It makes me wonder what the individual fits might end up looking like? The fit relies so heavily on the learning portion of the data since it is nearly half the data for each condition. Do you have individual subject examples? Perhaps I am missing something?

*The variability in the simulations figure (Figure 3) likely represents the variation of targets performed on each iteration of the simulation. On each iteration of the simulation, there is a new set of parameters drawn from a bootstrapped sample and random samples of targets drawn from the distributions for each condition. That set of targets is used to simulate both models for that iteration. The models then simulate data based on those targets which is reflected in the high amounts of variability the simulation plot. It is less likely that variation in the parameter values is causing the variability because 1) there is very little variability around the Constant condition simulation and 2) the parameters were based on bootstrapped samples from a previously collected dataset. We have also added individual examples of the model fits to demonstrate that the models do indeed follow the behavior (Figure 4).*