Review of the article "Not fleeting but lasting: Limited influence of aging on implicit adaptative motor learning and its short-term retention" by Pauline Hermans, Koen Vandevoorde, and Jean-Jacques Orban de Xivry, submitted to PCI Health and Movement Science.

<u>Summary</u>
The authors present an article aimed at evaluating the effect of aging on implicit learning processes involved in sensorimotor adaptation, with an attempt to "conceptually replicate" a finding from a previous study (Trewartha et al., 2014). Trewartha and colleagues observed an age difference in the spontaneous recovery of a previously learned movement adaptation after it is extinguished through a short period of de-adaptation. This finding is at odds with more recent evidence suggesting that implicit learning and retention is preserved in aging. A key novel contribution of this paper is a direct measurement of implicit adaptation in a force field paradigm, and an assessment of the relationship between implicit learning and spontaneous recovery in older adults. A second aim was a conceptual replication of the previously observed age difference in implicit short-term retention in the form of spontaneous recovery. The results are interesting, and the some of the conclusions drawn are supported by the analyses presented in the paper. The observations that A) implicit learning levels measured during adaptation are similar between younger and older adults, and B) that implicit learning levels correlated with spontaneous recovery, were especially interesting and novel findings. The authors also did not observe a reduced spontaneous recovery, in contrast to the previously reported finding. The paper would generally make a nice contribution to the literature on age-related changes in sensorimotor adaptation. However, I have several comments and suggestions that could improve the paper. Specific comments/questions are listed below:

<u>Specific Comments</u>

1. My primary concern is that framing this paper as a replication of Trewartha et al., 2014 is inaccurate. The authors made several methodological choices in designing the experiment that are inconsistent with the procedures used in the Trewartha et al. paper. Indeed, the authors list several methodological differences in the discussion section to consider as reasons for the discrepancy between the results of the two papers. Several additional methodological differences are mentioned below. I would argue that it would be better to lead this paper with a focus on the more novel questions of A) measuring implicit adaptation in older adults in a force-field adaptation task, and B) evaluating the relationship between implicit adaptation levels and spontaneous recovery. That reframing does not take away from the observation that the spontaneous rebound did not differ in younger and older adults, in contrast to Trewartha et al. That discrepancy warrants

further investigation.

2. It is unclear from the current analyses whether the groups performed similarly during the de-adaptation or washout phase. I did not see a statistical comparison of this phase between groups, although in Figure 2, it looks similar between groups. Given that it is well established that older adults are more susceptible to interference in a variety of learning and memory contexts it would be helpful to show that lateral deviation changed similarly over the de-adaptation trials in the younger and older adults. This would confirm that the washout of prior learning was similar in the two groups.

3. The authors used the last 48 trials of the error-clamp phase to evaluate the spontaneous rebound in the current study. It was not clear to me why they chose not to include the first 16 trials. It is important to note that the largest age differences in spontaneous rebound in the Trewartha et al., paper was from trial 5 through 17 of the error-clamp phase, with several trials after that being statistically similar between groups. This does not seem like an apples-to-apples comparison. The current paper also used a longer error-clamp phase. This would provide more time for the younger adults' spontaneous rebound to return towards zero, and potentially, towards the older adults' level.

4. Related to the previous comment, it is not clear why the authors did not provide a plot of the adaptation index data for the error-clamp trials to show the spontaneous rebound of the two groups in a way that aligns with the Trewartha paper. Instead, the exerted force is plotted. Although the exerted force appears consistent with the conclusion that older adults' spontaneous recovery was similar to that of younger adults in the current data, it is not directly comparable to the previous paper. It is also worth noting that the variability within the older adults appears to be much larger than the younger adults. Perhaps there are some older adults with comparable rebound, and some without. There are clearly a handful of older adults exerting even more force than younger adults (Figure 3B). Even looking at Figure 3C, there are several older adults with an adaptation index (albeit in the last 48 trials) below zero, suggesting no rebound, whereas there is only 1 younger adult in the same category. The reason for these individual differences may be unclear but are worth discussing.

5. In Figure 4 also, the relatively large forces exerted by 3 of the older adults may contribute to the strength of the correlation. Would the correlation change if those somewhat unusual participants were not included?

6. I was also curious if there was a correlation between force exerted into the channel and velocity. There appear to be several younger adults with lower velocity relative to the older group. If correlated, it might show that the

rebound in younger adults was somewhat lower in this study. Again, it is difficult to evaluate by comparison with the Trewartha paper without a plot of the adaptation index scores across the entire error-clamp phase.

7. While the authors identify several potential reasons for the discrepancy between their findings and the previous study, the discussion is somewhat dismissive of those potential explanations. Although ample behavioral and statistical evidence is provided from the current sample in favor of the conclusion that older adults' implicit learning is similar to younger adults, the discussion should emphasize the need for additional work to identify the conditions under which older adults might exhibit a smaller rebound than younger adults. This is important in establishing the nature of age-related changes in motor learning more generally. It is worth noting, that some of the factors acknowledged in this paper are not trivial. For example, the length of the adaptation phase may improve the overall memory of the load in older adults. The cognitive aging literature has frequently discussed the role of providing more time for learning in older adults as a way to improve memory performance in various contexts. Individual differences in cognitive abilities, and the potential differences between the respective samples in these studies, should also not be dismissed. It is well known that aging comes with increased interindividual performance variability in many cognitive and motor tasks.

8. There appear to be several other methodological differences between the studies that could contribute to the discrepancy in findings. Again, these are not trivial differences. They include the following:

   a. The current older adult sample appears to be younger, and with a tighter range than Trewartha et al. Recent studies in the aging literature have tended towards older age ranges, with mean age often over 70 years old. It is possible that change in the rebound occur after the 60s. Perhaps an exploration of a correlation between chronological age and the magnitude of the rebound would be informative here.

   b. The experimental design of the forcefield task here used 8 radial targets from a central start position. The Trewartha study used alternating movements between two targets (akin to Smith et al., 2006) with forces only applied to movements in one direction. Is it possible that implicit learning is strengthened by learning to apply compensatory forces in multiple directions in older adults? To my knowledge, there have not been any studies to directly compare these conditions in older adults. If increasing the number of targets increases the complexity of the task for older adults, it may impact how they perform, an effect commonly observed in a variety of motor

tasks.

    c. While the cuing method used to quantify implicit learning during the adaptation phase is an important part of this study, it is also a methodological difference from Trewartha et al. that may have impacted how older adults performed. Do those trials provide older adults with a cue about how their movements are adapting over time, and could that impact their level of implicit adaptation and rebound?

    d. In the current study the deadaptation phase was longer (more trials) than the previous study. This could have an impact if the age groups are differentially impacted by interference (related to my previous comment/question).

    e. The task instructions for participants in the current study differed from the Trewartha et al. paper. Here, as is common in these experiments, participants were asked to make "slicing" movements through the target and avoid movement corrections. In the Trewartha paper they were asked to stop on the target as the target would become the start position for the next movement. This could have made a critical difference in how the older adults produced the movements in the channel trials.

9. The methodological differences noted above are important, especially if it is claimed that this study is an attempted replication of Trewartha et al (2014). Critically, whether this study is a replication or not does not take away from the importance of the current observations or the discrepancy from those previous observations. It is important to highlight those differences to motivate future studies that can explain conditions under which these different findings might be observed. In some ways, focusing so much on framing the paper as a replication, without actually replicating the methods of that previous work, detracts from the potential impact of this paper.