Peer Community In
Health & Movement Sciences

Aging of upper-limb and whole-body movement efficiency

Matthieu Boisgontier *based on peer reviews by* ***Zack van Allen***, ***Florian Monjo***, ***Pierre Morel*** *and 1 anonymous reviewer*

Robin Mathieu, Florian Chambellant, Elizabeth Thomas, Charalambos Papaxanthis, Pauline Hilt, Patrick Manckoundia, France Mourey, Jeremie Gaveau (2024) Comparing arm to whole-body motor control disambiguates age-related deterioration from compensation. bioRxiv, ver. 5, peer-reviewed and recommended by Peer Community in Health and Movement Sciences. https://doi.org/10.1101/2024.02.16.576683

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This study by Mathieu et al. (2024) builds on previous computational research showing that human arm movements use gravity to save energy and be more efficient (Berret et al., 2008; Crevecoeur et al., 2009; Gaveau et al., 2014, 2021), as well as on experimental research showing that kinematic and electromyographic markers are directly related to this energetic efficiency (Gaveau et al., 2016).

The primary objective of this study by Mathieu et al. (2024) was to compare the effect of age on movement efficiency in an upper limb task and three whole-body tasks. These two types of tasks are often studied independently in the literature. Therefore, testing them in the same study allows the generalizability of the effect of age on movement efficiency to be examined. Electromyographic and kinematic patterns were compared in younger (n = 20) and older adults (n = 24), and movement efficiency was assessed using an index based on the activity of antigravity muscles. Results suggest that the effect of age is dependent on the type of movement. Specifically, older adults used gravity less than younger adults when performing whole-body movements, whereas no such age effect was evidenced when performing arm movements. The authors interpret this effect as an adaptation of whole-body movement strategies that compensates for age-related changes in body structures and functions to stabilize postural balance.

These findings contribute to the literature on postural control and how it differs from movement control that does not include the constraint of maintaining body balance, i.e., avoiding falls. Specifically, these results suggest that our brain implements a movement strategy specific to movements that require body balance, and

that the efficiency of this strategy is affected by age. Further research would help to determine whether this efficiency, although altered, remains optimal throughout the age-related decline of body systems, or whether priorities change across aging, with stability and fall avoidance becoming more valued than energetic efficiency.

References

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- Mathieu, R., Chambellant, F., Thomas, E., Papaxanthis, C., Hilt, P., Manckoundia, P., Mourey, F., & Gaveau J. (20024). Comparing arm to whole-body motor control disambiguates age-related deterioration from compensation. bioRxiv, version 5. Peer-reviewed and recommended by Peer Community in Health and Movement Sciences. https://doi.org/10.1101/2024.02.16.576683

Reviews

Evaluation round #2

DOI or URL of the preprint: https://doi.org/10.1101/2024.02.16.576683
Version of the preprint: 4

Authors' reply, 28 August 2024

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Decision by Matthieu Boisgontier, posted 22 August 2024, validated 23 August 2024

Revision requested

Dear authors,

The reviewers are satisfied with your answers, but still suggest some minor changes. Once you have taken these into account and the preprint has been updated on BioRxiv, I will write my recommendation.

Best regards,

Matthieu Boisgontier

Reviewed by Pierre Morel, 21 August 2024

I commend the authors for adressing the major concerns of reviewers, throughly answering the questions that were raised, and doing substantial changes to the paper. Notably, the authors included interesting new analyses on the correlation between balance control and movement efficiency changes that strengthen the

message of the paper. However whith these important modifications, I still feel that some additional edits in the manuscripts are needed for readability. I will present these below:

Despite its thorough modifications, I find that the abstract could still be better structured. The early framing of deterioration VS compensation is still not easy to understand in this short form, as does the double negation ”decreased [...] efficiency [...] negatively correlated with”. An abstract focused on movement efficiency and with less storytelling would be easier to parse, while compensation could be mentioned only with regards to the balance results.

The more focused and streamlined introduction is appreciated. Since the authors mention that they use ”effort” and ”efficiency” somewhat interchangably (more on that in a comment below), I find the sentence L93-95 difficult to parse. More importantly, the end of the introduction should be amended to prepare the reader for the new analysis/results: balance is not mentioned in the introduction.

Line 125 typo: whole [B]ody

Lines 122-123 and legend of figure 1: WBR tasks still mention ”a” target instead of two

Line 207: The use of the right shoulder marker for the detection of movement onset makes sense only for the whole body task, precise which marker was used for arm movements.

Lines 230 to 243: This additional paragraph about effort seems misplaced in the methods. Core concepts should be presented in the introduction (some already are), and/or recalled in the results for understandability. I’m puzzled by the sentence ”we interchangably use “energetic efficiency” and “effort minimization” in the present work”. If that is the case, I would strongly suggest that the authors consistently use one term only throughout the paper for clarity. Since ”effort” can have a subjective connotation, ”efficiency” would seem to be the most appropriate term given the framing of the paper.

Line 247: reference [Mat, 2021] seems wrongly formatted

Figure 3: indicate again what is TA, T and NA in the legend.

Line 276: The nomenclature ”Erector Spinae D7 (ESD7)” was not correctly updated here to EST7 and elsewhere, notably in figures.

The dismissal of SOL and EST7 as postural rather than focal would deserve more explanation / justification.

LInes 300-308: Given the major concerns in the first round of review I understand the justification here, but it would now read too ”defensive” for a first time reader. I feel that the main rationale should better be presented elsewhere than in the methods. Moreover, now that the authors confirmed that the machine learning was not used to select muscles of interest, I question its presentation before the main results. Could this analysis be better framed as a confirmation of the main results: antigravity muscles in whole body tasks are indeed the ones that separate subjects by age ?

Figure 6: Typo ”height” instead of ”eighth”. On panel B I would suggest adding a line representing chance level as well as ordering the muscles by accuracy to show that antigravity muscles have the highest. The relatively high score of some non antigravity muscles (like DP) could be discussed.

Reviewed by Florian Monjo, 18 August 2024

I appreciate the authors’ significant efforts in revising the manuscript.

Overall, they have satisfactorily addressed my comments.

However, a few minor issues remain, particularly some errors in the text.

Line 216: Please replace “analyse” with “analysis.”

Line 219: Remove the parenthesis before ”Winter” and place it before the year.

Line 222: Replace “ofsset” with “offset.”

Line 230: Replace “rational” with “rationale.”

Line 234: Enclose the year in parentheses.

Line 301: Consider revising the sentence to: ”Scientific literature has reported that the control of whole-body movements changes with age, while the control of arm movements does not.”

Line 483: Remove the ”s” from ”harvests.”

Regarding your response to the comment on oral consent ("The French National Ethics Committee (2019-A01558-49) approved the experiment to be conducted with oral informed consent only. Nonetheless, each participant was included in the study by a medical doctor."), it might be worth incorporating this information into the text.

Reviewed by Zack van Allen, 24 July 2024

The authors have addressed all my comments adequately and I have no further comments to add.

Evaluation round #1

DOI or URL of the preprint: https://doi.org/10.1101/2024.02.16.576683
Version of the preprint: 1

Authors' reply, 09 July 2024

Download author's reply

Decision by Matthieu Boisgontier, posted 06 April 2024, validated 08 April 2024

Age and movement efficiency in upper-limb and whole-body tasks - Revision requested

Dear Authors, Thank you for submitting your work to PCI Health & Movement Sciences. I would like to begin this decision letter by commending your commitment to good research practices, including open data and open code. The primary objective of your study is to assess the effect of age on movement efficiency in one upper limb task and three whole-body tasks. Movement efficiency was assessed using an index based on the activity of antigravity muscles. The results suggest that this effect of age is dependent on the type of movement. I find the study interesting because it assesses movement efficiency in two types of tasks that are often studied independently in the literature, providing an opportunity to test the generalizability of the effect of age on movement efficiency. However, a number of concerns were raised by three experts in the field of your study and one expert in machine learning. Their comments appear below and mainly relate to readability, the clarity of the hypotheses and key concepts, the thoroughness of the EMG analysis, and the use of machine learning and ANCOVA analyses on the same dataset. I share these concerns of the reviewers. Regarding the latter concern, it is my understanding that the authors used the same dataset for both the selection of variables and the subsequent analysis of the differences in those variables, which could potentially have biased the results. Using the same dataset that was used to select the variables that differentiate between younger and older adults' strategies introduces the risk of overfitting the model to the dataset. A consequence of this overfitting is the possibility that the results will not be replicable in future studies. One of the options that might address this concern would be to remove the machine learning approach and to focus on *all* the antigravity muscles in the analyses (e.g., adding the T7 erector spinae and the posterior deltoid), as it makes sense from a theoretical standpoint. If the authors decide to keep the machine learning analysis, it should at least be described in more detail, the table of the results should not be in supplemental material, and the limitations should be acknowledged, as suggested by the fourth reviewer. In my own reading, the introduction and discussion put a lot of emphasis on the concept of compensation. However, I wonder whether the study is really about compensation, since it does not examine the relationship between the effect of age on movement efficiency and any age-related processes that this effect might compensate for. To me, the study is about the effect of age on movement efficiency. The variables collected in the study do not allow for the investigation of a possible compensatory effect. Following on from this comment, I think there are several cases of overstatement where the conclusions are not based on the data and should rather be presented as potential explanations or interpretations of the results that require further research to be confirmed or

rejected. I think it is essential for science to avoid drawing conclusions that are not supported by evidence. Such invalid conclusions cascade through the literature, because, unfortunately, articles are most often cited for the conclusions drawn by the authors, not for the evidence supported by the data. In the main results, the authors pooled the data from the three whole-body tasks. I think that an analysis testing the 4 tasks x 2 age groups interaction would be important: the absence of a significant interaction in each of the 3 whole-body tasks would further support the main results, whereas a significant interaction in one of the whole-body tasks would mitigate them. In any case, this analysis would provide useful information to the reader. In addition to these general comments, I would like to provide specific comments that may improve the quality of the manuscript, should the authors succeed in addressing the major concerns raised at this stage of the evaluation process. Best regards,
Matthieu Boisgontier

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Specific Comments:

- Line 137: Please provide the rationale for the sample size used in this study (e.g., Lakens, 2022).- Line 143 (experimental tasks): Could the authors clarify why they chose to use a single task for upper-limb movements and 3 tasks for whole-body movements? If this study and sample population were used for another article, please mention this in the manuscript.
- Line 146 (Figure 1A): Perhaps put the down arrow under the arm of the avatar that is on the left of the picture.
- Line157: I would rather call it as shoulder flexion/extension. Shoulder elevation is when the shoulder moves toward the ear (involving contraction of the upper trapezius muscle).
- Line 158: The STS/BTS movement is not described.
- Line 168: “her or his” can be replaced by “their”.
- Line 187-193 (Kinematics): Please remove the capital letter in “Clavicle”; “sternum” - where exactly on the sternum?; “backs of the head” - on which bone structure?; “on the scapula” - where exactly on the scapula?; “shoulders (acromion)” - acromion is still the scapula; “forearms (lower lateral 1/3 left, 2/3 right)” - on which bone (ulna or radius)?; “fingers (second metacarp)” - metacarpals (not metacarps) are not part of fingers; “knees (flexion/extension axis)” - on the medial or lateral side of the knee?; “thighs (upper lateral 1/3 left, 2/3 right)” - please clarify, is the position different for the right and left thigh?; “calves (upper lateral 1/3 left, 2/3 right)” - please clarify.
- Line 199: « vastus lateralis (VL) biceps femoris (BF)”: please add a comma.
- Line 200: T7 (for thoracic) instead of D7 (old nomenclature)
- Line 200: “anterior tibialis (TA)” should read “tibialis anterior (TA)”
- Lines 236-248: “We defined negative epochs as an interval where the phasic EMG signal was inferior to zero minus three times the standard deviation of the stable phase preceding the movement, and this for at least 40ms.” On what basis were these decisions made? Why not 2SD? Why not 35 ms? Please clarify.
- Line 251: Please clarify how the negativity index is to be interpreted. Do higher values indicate better efficiency?
- Lines 250-252: “[…] negativity index, defined as T x NA / TA, with NA the Negative Area integrated on the phasic signal between negativity onset and offset, TA the Tonic Area integrated on the tonic signal between the negativity onset and offset, and T the duration of the negative epoch normalized by movement duration”. Since the negativity index is central to this study, please illustrate T, NA, and TA in a figure to provide the reader with a visual explanation of what variables are included in this index.
- Line 261 (Statistics): Please clarify which tests were conducted and which variables (and covariates) were included.
- Line 264 (Machine learning): Since the machine learning results appear to inform the ANCOVAs conducted, should this section not be placed before the “Statistics” section. Also, is machine learning not also a statistical analysis?
- Line 284: “Movement duration […] was slightly reduced in older compared to younger participants”. Was movement duration shorter in older adults? How do the authors define “slightly”?

- Line 318: Perhaps visually indicate what is green and what is blue, as in the other figures using this color code?
- Line 319: Please also test the Age x Task interaction without combining the 3 whole-body tasks, similar to what is done in the exploratory analyses.
- Lines 3043-312: This part might be better suited for the Methods section.
- Line 314: Please include Supplementary Figure 1 in the manuscript. Please further describe the results of this table in the text.
- Lines 349-350: This information should be mentioned in the Methods section.
- Line 321: I know it's already mentioned above, but please clarify again that the muscles that were used to compute this average muscle pattern were those selected according to the machine learning analysis.
- Lines 316-318: "in the following we focus the analysis on antigravity muscles. Those muscles are the Anterior Deltoid for the arm task and the Vastus lateralis and Erector Spinae at L1 level for the tasks involving movements of the entire body (STS/BTS and WBR)." Why not the T7 erector spinae and the posterior deltoid, which also have an antigravity action?
- Line 365: "The results revealed an age-related alteration of muscle commands that differ between tasks." Since there is no evidence of a change in the whole-body task, it might be more accurate to write that the alteration is dependent on the type of task.
- Lines 366-367: "we found that a muscle marker of effort minimization was reduced during whole-body movements but not during arm movements". The results showed no evidence that effort minimization was reduced during arm movement, not that it was absent. "Absence of evidence is not evidence of absence" (Alderson, 2004).
- Lines 372-373: "The present results reveal that effort-minimization was downregulated in older adults compared to younger adults". I suggest the authors clarify that this statement reflects their interpretation rather than facts. The pooled data from whole-body movement suggests that there is no such age-related downregulation.
- Lines 373-374: "Overall, the present results suggest a compensation process that modulates planning strategies to maximize equilibrium in older adults." How can the results suggest this when there was no assessment of balance in the study? To me, this conclusion is not supported by the results of the study.
- Line 432-434: "In conclusion, probing a specific motor control process, the present study provides a set of behavioral results that support the interpretation of a compensatory process that counterbalances other deteriorated processes in older adults." How can the study support the fact that the results of the study support a compensatory process that counterbalances other deteriorated processes in older adults, when the deteriorated processes were not assessed, and their relationship to the Negativity Index not tested? The results suggest that the effect of age on movement efficiency is moderated by movement type. Whether this moderation reflects a compensatory mechanism requires further research. References:
- Alderson P. Absence of evidence is not evidence of absence. BMJ. 2004;328(7438):476-477.
- Lakens D. Sample size justification. Collabra: Psychology. 2022;8(1):33267.

Reviewed by Pierre Morel, 21 March 2024

This manuscript provides a valuable contribution by addressing the discrepancies found in prior research regarding movement efficiency in younger versus older adults. Previous studies on arm movements find that efficient control of movements is maintained in older adults, while studies on whole-body movements show degradations. However these results came from different studies, using different samples and different tasks and measurements.

The strength of the current study is to compare both types of movements within the same sample of young and older adults and using the same type of measurement and analysis. For this, the authors adapted to whole-body movements a measure of the optimisation of movements against gravity using phasic EMG, previously used for arm movements. The study confirms the earlier observed differences between the control of arm-only movements and whole body movements in older adults.

I however have three main concerns with the manuscript. First, since it is so central to the argument, the manuscript would benefit from a more detailed exploration of the adaptation of phasic EMG analysis to whole-body movements, taking into account potential limitations and confounds that could enrich the discussion (see point 9 below). Second, further clarification on the selection of muscles for analysis and the use of the machine learning approach is crucial to strengthen the rigor of the study (see point 10 below). Last, streamlining certain sections could improve readability and argument flow. I will provide more detailed comments and questions below, which I hope will be helpful.**Abstract / Introduction**

1. The abstract could be revised for clarity and conciseness. Specifically, it should more clearly define what is meant by ”behavioral compensation” and articulate the relationship between compensation and energy efficiency in the gravitational environment

2. The hypothesis presented in the abstract and introduction —that age-related compensatory processes may correspond to an adaptation process altering movement strategy— lacks clear differentiation among the terms ”compensatory processes” ”adaptation processes” and ”changes in movement strategy”. This ambiguity makes it challenging to conceive an alternative hypothesis.

3. The introduction's first two paragraphs makes a good point in explaining what compensation is, why it is important to study it and why it is difficult. However the rest of the intro could be refined, notably to present more clearly the literature on movement efficiency. In addition to point 2 above:

- The paragraph on neural mechanisms (L.72-86) could be more concise if its only point is to show that previous studies used broad measures as written L. 90.
- L. 95 ”There is no denying”: informal
- The sentence L.108-110 makes the point that previously cited studies did not really study compensations and hints that studies cited in the following paragraph do. This could be be better explained and demonstrated in both paragraphs.
- The paragraph L.111-134 could benefit from a more coherent structuring to clearly present conflicts in the literature and show how the current study aims to resolve them.

Methods

4. The Methods section would benefit from additional detail to address the following points:

- The description of target placement (L. 154) should specify if it is only in front of the right shoulder, as only the right arm is used
- The spacing of targets (L. 166) needs clarification on whether two targets are used and if subjects reach with both hands.
- The methods did not specify which motion-tracking markers are used for movement segmentation for each type of movement
- L. 220: ”signals were integrated...”. More precisions could be added: do the authors refer to sliding-window averaging, RMS ? How does it compare/mesh with the envelope generation using low-pass filtering mentioned L. 228 ?
- L. 227-239: All steps seem to require resampled EMG, but details are missing (resampling of slow movements, how the additional delays mentioned L221-221 are taken in account for the resampling)
- L. 239: should ”from the EMG trace of each fast movement” be ”from the EMG trace of each pair of fast movements” since averages are made in pairs ?
- L. 264 The stated goal of the machine learning approach as described in the methods section is not as clear as in the results sections.

5. The figure 2 shows a marked decrease of the shoulder vertical position before the onset of the upward movement and after the offset of the downward movement. This raises several questions:

- What causes it ? Is it the subject bending forward before sitting up ?
- Why were onset/offset point definitions chosen so that these were ignored ?

6. In figure 3 There are identifiable bumps in the fast EMG signals visible in panel A that are not visible in panel C, notably a period at around 2/3 of the movement where there is a high variability between fast traces that is not present on the phasic traces. What causes this discrepancy ? Moreover it could be useful for comprehension to represent extracted / computed values (NA, TA, etc) in figure 3.Results

7. The results section should specify that Figure 4 and lines 284-289 pertain to the fast movements only, if that is the case.

8. The authors indicate that movement duration was used as a covariate in further analyses. The manuscript would benefit from an explanation of how this was done what were the outcomes of this.

9. The fact that negativities reflect the exploitation of gravity to reduce muscle efforts in arm movements is sound and well established in itself, but the jump from this to the comparison of two populations and whole body and multi-joint movements seems quite steep and doesn't explore limits and confounds. Here are a few points:

- The analysis of EMG alone seems limiting, as a difference in EMG would not be interpreted in the same way when it is accompanied by a difference in kinematics or when kinematics are the same. For example in Figure 5 for ESL1 and VL in WBR downward movements some differences in the EMG phasic traces appear to be due to timing differences rather than amplitude differences. Could those be due to different timings in the movements of different segments of the body between the groups ? How would the interpretation change if this was the case ?
- The above comment is also linked to the observation #5 about figure 2. If this is indeed the subject bending forward, could there be differences between the groups on this ?
- I imagine that the way gravity acts on movements would depend on the build of the person and the way muscle and fat mass are distributed in the body and limbs. The two groups could notably differ in that respect, which could be discussed.
- Negativity indices for different tasks are compared in the same statistical model. It is not immediately clear whether the normalisation process is sufficient to compare directly an index computed from two muscles in a multi-joint complex movement to an index computed from a single muscle in a single-joint simple movement.

10. L.304-318: The machine learning approach is explained more clearly here. Its goal is to confirm that that the antigravity muscles are the ones that allow the best discrimination between groups, reflecting the differences in motor strategies between groups. I find the argument and the data not convincing here, and the results raise a concern of bias in the selection of muscles:

- Some non-antigravity muscles in whole body tasks, such as DP, show classification performances that are not far off the performance of the chosen muscles (VL and ESL1)
- Some known antigravity muscles such as the soleus muscle yield comparatively poor scores here and are not selected for the main analysis. Was the machine learning approach then used for selection and not just for confirmation ? This raises a concern of potential "double dipping" if that was the case.

11. The reporting of statistical results seems incomplete. Additionally to my comment #8:

- L. 320-335: Main effects of age and tasks are not reported

- L. 349-360: Here the mentioned models include task direction as a covariate. Was this explored in the main analysis ?

12. Fig 7 is not referred to in the text. It also does not present data from the arm task. The legend mentions different panels for data from the ESL1 and VL but this does not appear in the figure.Discussion

13. L. 373-374: The authors mention that results suggest that the planning strategies maximise balance but this was not shown in the results L. 344

14. The similarity between paragraph L. 393-404 and the previous paragraph (same citations) should be addressed to avoid redundancy.

15. L. 426: About the non reproduction of previous results, the authors mention that the EMG-based analysis of the current paper might yield different results from the kinematic analysis from the previous study. Since motion was recorded here, the same analyses could be repeated to check this. This is linked to my comment #9.

16. The tasks proposed here differ not only in terms of arm versus whole-body involvement but also in their intrinsic nature, which could affect the control strategies employed. This could be discussed. Notably:

- Some tasks have a precision component with pointing (ARM, WBR downward D1 and D2), while others do not (STS/BTS, and WBR upward D1 and D2).

- If the WBR tasks are bimanual (see #4) this makes them also different conceptually from the ARM task.

Reviewed by anonymous reviewer 1, 15 March 2024

Major comments:

- The abstract needs a complete re-organization and also needs to include more information about the methods, results, and conclusions.
- I think the Introduction can be shortened and focus more on the specific gap the study is investigating - compensatory mechanisms in fine movements in older adults.
- It is not clear what type of adaptations are required in these simple tasks. In other words, why these tasks were chosen is not clear. This needs to be justified in the Introduction or briefly in the Methods.
- The authors mentioned in the Introduction that they are investigating compensatory mechanisms at the behavioural level but they are doing the analysis based on EMG signals. The EMG signals are at the cellular level (motor units). I am not sure this is considered behaviour.
- It is not clear why an ML analysis was needed when they could statistically compare the EMG metrics between the groups.- If the simple arm movement tasks did not show compensatory mechanism differences, then the authors' claim in the Introduction that simple task are needed to study this mechanism is not valid.- Overall, I am not convinced about this conclusion: "Overall, the present results suggest a compensation process that modulates planning strategies to maximize equilibrium in older adults." **Minor comments:**- The first four lines of the abstract can be shortened to focus more on the specific problem the study is targeting.
- Lines 28-31: Belongs to the intro of the abstract.
- Only oral consent, not written?

Reviewed by Florian Monjo, 18 March 2024

Summary:

The paper presents a thorough investigation into age-related compensation in motor control processes, focusing on how older adults adapt their movements to maintain efficiency and functionality. The study involved twenty younger adults and twenty-four older adults, comparing their muscle activation patterns during tasks involving arm and whole-body movements. By utilizing a specific analysis method, the researchers

aimed to uncover whether age-related alterations in movement efficiency reflect an adaptation process or dysfunction. The findings suggest that age-related changes in whole-body movements may be interpreted as a form of compensation for deteriorated sensorimotor components, rather than simply dysfunction. The study provides valuable insights into the intricate relationship between aging and motor control, highlighting the importance of understanding compensatory mechanisms in maintaining optimal movement patterns. However, certain aspects of the paper require further clarification, particularly concerning the definition of its central concepts and certain methodological elements.

Overall Comments:

Central Concepts of the Study: The paper could benefit from defining and clarifying central concepts such as movement efficiency, effort, effort minimization, effort minimization downregulation or upregulation and their relationship throughout the text. For instance, the authors introduce the concept of energy efficiency in the abstract; nevertheless, it remains undefined and is not revisited in the main text. It would also enhance clarity to illustrate how the various variables measured and analyzed relate to these concepts.

Discussion on Potential Confounding Factors: Addressing potential confounding variables such as physical fitness levels and cognitive function would enhance the interpretation of the results.

Specific Comments:

Introduction:

Line 54: Consider clarifying the statement regarding the elementary concept of health.

Line 55: "despite normal age-related deterioration, compensatory processes enable older adults to remain in good health and continue to perform their daily activities comfortably." It's important to specify that this pertains to successful aging.

Line 75: "In the sensorimotor field, following the consens that aging is associated with increased activation and increased spatial recruitment, numerous studies have attempted to establish a correlation between neural activation and behavioral performance in older adults". Do you mean the consensus? Also, could you clarify what you mean by 'neural activation' and 'spatial recruitment'? It seems vague; are you referring to the spatial recruitment of motor units and to the neural activation of muscles?

Line 79: "This literature has not reached a consensus on the neural changes underlying compensatory mechanisms in older adults. Indeed, several studies reported a positive correlation, and as many reported no correlation or even a negative correlation". The intended meaning is ambiguous regarding which correlation is being referenced.

Line 85: "Several reasons may explain these discrepancies". If these reasons aren't elaborated upon, consider removing this sentence.

- Line 88: not sure 'neuronal alterations' is the appropriate term. It sounds like you are investigating the nervous system directly. I suppose it would be better to refer to sensorimotor alterations.
- Line 92: In the same line, not sure it's appropriate to talk about measuring neural mechanisms, given that, as you mentioned, these studies used 'broad measures'. Rather, these studies investigate behaviors through experimental paradigms that allow inference of some neural mechanisms.
- Line 96: Still in the same vein, you write 'linking the brain to behavior.' It still sounds as if you are conducting an imagery study."
- Line 95: Avoid 'There's' and prefer 'There is'

Line 106: "older adults favor movement efficiency over precision to compensate for their increased energetic cost". It's important to clarify what you mean by 'movement efficiency' (In my understanding, a movement is more efficient when achieving similar mechanical output with lower effort or neuromuscular activation). Additionally, I don't understand why you oppose movement efficiency and precision here because, in my view, these are not contradictory concepts; a precise movement can indeed be efficient. As mentioned in the overall comments, the concept of movement efficiency is central to the study and requires clearer definition and its relationship with concepts such as effort minimization and neural activation needs to be established.

Line 111: Please clarify the concepts of upregulation and downregulation of effort minimization.

Methods:

- Line 138: Why did participants only provide oral consent instead of written consent?
- Line 151: ”vertical arm movements around the shoulder joint.” Would it be more accurate to refer to shoulder flexion?
- Line 157: Is ”shoulder elevation” the appropriate term here?
- STS/BTS task: If I am not mistaken, the rationale for using slow and fast movements is not presented.
- Line 171: Please use ”×” instead of ”*”.
- Trial organization: It is not stated whether slow/fast trials and tasks were randomized.

The variables analyzed are not clearly identified, making it difficult for the reader to understand the rationale behind performing ANCOVAs in the statistics section. Further development is necessary to clarify this aspect.

Results:

Some parts appear to be more suitable for inclusion in the method section, particularly the paragraph beginning at line 304.

Line 348: I am unclear why these are categorized as exploratory analyses, as they seem to be testing your hypothesis.

Discussion:

- Line 364: ”muscle patterns” does not appear to be the appropriate term; perhaps you mean ”muscle activation pattern.”
- Line 365: ”Muscle command” is rather unusual and seems inappropriate given that the command is generated centrally.
- Line 366: It could be helpful to describe this muscle marker and to rather refer to muscle activation marker of effort minimization.
- Line 386: ”Thus, arm movements equally optimized gravity effects in younger and older adults.” I do not understand the intended meaning here. Do you mean that both younger and older adults optimize gravity effects to control arm movements?
- Line 397: I would suggest placing the references at the end of the sentence.
- Line 399: ”Muscular patterns” – please refer to previous comments.

Reviewed by Zack van Allen, 05 April 2024

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