Peer Community In
Health & Movement Sciences

An inverse gravity experiment supports the theory of an internal gravity model in the central nervous system

Anne Koelewijn *based on peer reviews by* ***Jan Hondzinski*** *and 3 anonymous reviewers*

Denis Barbusse, Sarah Amoura, Jérémie Gaveau, Olivier White (2024) Feedback-driven adaptation of gravity-related sensorimotor control to an upside-down posture. OSF preprints, ver. 3, peer-reviewed and recommended by Peer Community in Health and Movement Sciences. https://doi.org/10.17605/OSF.IO/D9JPF

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The study by Barbusse et al. (2024) investigated how motor control of arm movements is affected by reversed gravity. It is commonly assumed that the central nervous system contains an internal gravity model, and that this model is used to optimize movements to minimize effort under the influence of gravity (e.g., Berret et al., 2008). Previously, the effect of decreased and increased gravity was investigated, and it was shown that people were able to adapt to this novel environment in a matter of minutes or days (e.g., Gaveau et al., 2011). Therefore, the authors investigated the effect of inverse gravity on motor control of arm movements.

In this study, an experiment was performed in which participants were placed in an inversion table and asked to perform as many pointing movements with their shoulder as possible in 12 blocks. In each block, the inversion table was placed either in the head-up or head-down position, and the position was switched every 35 seconds, starting from the head-up position. After 4 blocks, a 90 second break was taken. It was found that movement duration and amplitude did not significantly differ between both orientations. An analysis of the difference in time to peak acceleration, time to peak velocity, and time to peak deceleration between upward and downward movements revealed no significant difference for the peak acceleration, while for the peak velocity, the time difference was significantly smaller in the head-down than the head-up position, and for the peak deceleration, the time difference changed in the head-down position with the number of blocks, reaching a value more similar to the head-up (baseline) position.

The time to peak acceleration did not reverse for the head-down position, which showed that the central nervous system is not able to take advantage of gravity when it is placed in a head-down position, since it does

not take advantage of the “free” acceleration provided by gravity. A longer exposure to inverse gravity might allow the body to adapt and re-optimize its internal gravity model to the new situation. The time difference was significantly different for the deceleration, but not for acceleration, which indicates that the movement was adapted mainly by feedback control, but that feedforward control remained largely the same. This further supports the conclusion that the central nervous system had not yet adapted its internal gravity model, and that re-optimization starts with adapting feedback control (Izawa et al., 2008). An important limitation is the discomfort that is experienced in the head-down position, which not only changes gravity, but also created negative physiological responses.

References:

Denis Barbusse, Sarah Amoura, Jérémie Gaveau, Olivier White (2024) Feedback-driven adaptation of gravity-related sensorimotor control to an upside-down posture. OSF preprints, ver.3 peer-reviewed and recommended by PCI Health & Movement Sciences. https://doi.org/10.17605/OSF.IO/D9JPF.

Berret B, Darlot C, Jean F, Pozzo T, Papaxanthis C, Gauthier JP (2008) The inactivation principle: mathematical solutions minimizing the absolute work and biological implications for the planning of arm movements. PLoS computational biology, 4, e1000194. https://doi.org/10.1371/journal.pcbi.1000194

Gaveau J, Paizis C, Berret B, Pozzo T, Papaxanthis C (2011) Sensorimotor adaptation of point-to-point arm movements after spaceflight: the role of internal representation of gravity force in trajectory planning. Journal of Neurophysiology, 106, 620–629. https://doi.org/10.1152/jn.00081.2011

Izawa J, Rane T, Donchin O, Shadmehr R (2008) Motor adaptation as a process of reoptimization. The Journal of Neuroscience: The Official Journal of the Society for Neuroscience, 28, 2883–2891. https://doi.org/10.1523/JNEUROSCI.5359-07.2008

Reviews

Evaluation round #3

Reviewed by anonymous reviewer 2, 07 October 2024

Dear Editor,
I think the manuscript can be published as it is.
Best Regards,

Reviewed by anonymous reviewer 3, 18 October 2024

Authors addressed most concerns, especially those that needed it for future readers. My only comment that I still saw was in the Figure 3 caption–it should include information about the meaning of two asterisks also.

Evaluation round #2

DOI or URL of the preprint: https://doi.org/10.17605/OSF.IO/D9JPF
Version of the preprint: 2

Authors' reply, 13 September 2024

Download author's reply

Decision by Anne Koelewijn, posted 02 September 2024, validated 02 September 2024

Dear authors,

Thank you for the revisions! Most of the comments have been addressed, but some important questions have been raised about the statistics that still require some further explanation. Therefore, I would like to ask the authors to clarify the motivation for the statistical approach that was used. Other than that, there are some minor comments that could also still be incorporated.

Kind regards,

Anne Koelewijn

Reviewed by anonymous reviewer 2, 30 August 2024

I'm a bit 'puzzled' by the reply of the authors regarding statistical analyses

Rechecking the normalities with the Shapiro-Wilk test revealed that normality was not confirmed for 9 out of 36 variables. However, since all the normalities were verified with KS tests and a qualitative observation of the data did not reveal any aberrant distributions, we preferred to rely on these results in order to carry out parametric tests, the results of which are more robust.

parametric tests certainly have more power, i.e., you're more likely to detect a significant difference when there is one, but they are less 'robust' than non-parametric tests. And I have difficulties to understand the logic of relying on a less reliable test for normality (namely the KS test with small sample sizes) when a more reliable test shows that a fourth of the tests were likely run on non-parametric data.

We also base our decision to use ANOVAs, on a set of articles that have shown their strong capacity for resilience and robustness with data that do not follow a normal distribution (Schmider et al., 2010, DOI : 10.1027/1614-2241/a000016; for review, see : Glass et al., 1972, DOI : 10.2307/1169991; Harwell et al., 1992, DOI : 10.2307/1165127).

so, are non-parametric tests useless?? If so, why having tested for normality (with the KS test) in the first place??

Finally, we would like to draw attention to the fact that the statistical tests carried out here have only been used to objectify phenomena that are already clearly visible in figures, simply by looking at the graphs in Figure 3. We were not looking for particularly low significance effects.

I guess this part is the most puzzling. Some results seem pretty clear indeed. But following your 'logic', why report so many statistical results (lines 190-227)??

Overall, we strongly believe that the conclusions reached in the present manuscript would remain the same with others types of statistics.

Data and proper tests are usually more convincing than 'strong beliefs', though overall I don't doubt your results.

For information, 'highly significant' (line 199) does not mean anything. You set a significance threshold (in your case at .05, line 177), so the test is either significant or non-significant. Also, Bonferroni is not a test (line 176), but a correction method to control the false alarm rate when running multiple comparisons.

Reviewed by anonymous reviewer 1, 09 July 2024

The authors have addressed all the points raised by the review.

One optional recommendation for the authors: I understand that the legend of figure 2 explains the color/arrow information, but I'd still recommend adding a text label (good for the figure to stand on its own at a glance, particularly if you want to show it in presentations in the future). But as I mention this is a stylistic preference that the authors can consider to be optional.

Reviewed by Jan Hondzinski , 17 July 2024

Responses to PCI HMS questions:

Does the title clearly reflect the content of the article? Yes
Does the abstract present the main findings of the study? Yes
Are the research questions/hypotheses/predictions clearly presented? Yes
Does the introduction build on relevant research in the field? Yes
Are the methods and analyses sufficiently detailed to allow replication by other researchers? Yes
Are the methods and statistical analyses appropriate and well described? Yes
In the case of negative results, is there a statistical power analysis (or an adequate Bayesian analysis or equivalence testing)? I don't know
Are the results described and interpreted correctly? Mostly
Have the authors appropriately emphasized the strengths and limitations of their study/theory/methods/argument? Mostly
Are the conclusions adequately supported by the results (without overstating the implications of the findings)? Mostly

Comments:

Main concern but easily fixed:

Lines 248-249 I am confused on why authors did not include their new analyses about movement body-orientation and amplitude here. It explains what people did when first introduced to the uncommon/less common body-orientation, which involve spatial-temporal reorganization, which would be part of the re-optimisation process. It also might imply that if emphasis was placed on maintaining a set spatial amplitude, re-optimisation for Head-Down would likely be the same as Head-Up. However, that still requires study. And it is this latter issue that should be mentioned in the limitation section on Lines 309-319.

Minor items:

Line 36: I don't understand "would first". I think authors meant just "feedforward ones remain unmodified."

As mentioned previously, this is an editorial preference, but I do not care for the use inanimate objects doing things. Consider changing "The present study aims…" to "In the present study, we aim…" on Line 111 and "…, the literature has…" to "…, researchers have …" on line 265.

Line 153: it would be great to have authors list a reference for the use of 10% of max velocity. Others have used 5% or a specific velocity threshold.

Lines 170-172: Needs a reference here, especially considering authors mention specific variables.

Results in general–I find it odd that authors present the main effects after the interactions. I have always seen main effects, followed by interactions. Consider changing this order.

Line 185-6: stats show only less movement in the first than 4-6, not blocks 2 and 3. Re-word for accuracy. Please check all stats and corresponding text, as there is so much going on, it is easy to miss.

Figure 3: Why is there a "ns" for 3 and 6 for C but not 3-5? Why is there a "ns" for 2 and 6 for E but not 3-5? The graphs are more confusing with "ns". I don't believe these are needed, so I would remove all and adjust the caption. Lines 206-209. Is there a way to include the results from one-sample t tests on the Figure?

Line 256: The word “first” seems odd here. If removed, it doesn’t alter the content, so I would remove it.

Supplementary file, Figure S2: the last word should be singular, thus “participant” rather than “participants”

Lines 296-7: “Stating that the values were first reversed, compared to Head-Up baseline, and then adapted back to values that were close to Head-Up baseline ones.” is misleading. It sounds like you mean there is a significant difference between Head-Up and Head-Down at block 4 before achieving ns at block 5 and 6 again. Results showed no significant differences at these blocks. Re-word for clarity please.

Evaluation round #1

DOI or URL of the preprint: https://doi.org/10.17605/OSF.IO/D9JPF

Version of the preprint: 1

Authors’ reply, 27 June 2024

Download author’s reply

Decision by Anne Koelewijn, posted 13 March 2024, validated 13 March 2024

Feedback-driven adaptation of gravity-related sensorimotor control to an upside-down posture - Revision Requested

Dear authors,

Thank you for your submission. Three reviewers have now assessed the paper. Generally, the paper was well received, and the paper was written clearly and the experiment straightforward. However, there are a couple of questions about the methods and some clarifications requested that need to be addressed before I can recommend the paper.

In addition to the reviewer comments below, I would like to add one suggestion. The introduction clearly identifies the gap in the research but the authors could elaborate further on why it is useful to investigate this gap. While people experience microgravity, e.g., during a visit to space, and increased g-forces during high-speed activities, I do not immediately see when people would experience an inversion of the gravity direction.

Kind regards,

Anne Koelewijn

Reviewed by Jan Hondzinski , 05 March 2024

Download the review

Reviewed by anonymous reviewer 1, 20 February 2024

This paper examines the effects of inversion (being held in an upside down position) on point-to-point reaching movements. The manuscript is quite clear but could be improved with some small modifications.

Major:

I have only one major query; in the methods it is indicated that data from blocks 1-6 were analyzed, but it seems that a total of 9 blocks were conducted overall – does this mean that some of the data was excluded from the analyses? If so, why? Or have I missed some explanation of this in the text?

Minor:

Spacing between consecutive letters appears to be very tight, making it hard to read in some cases (e.g. the abbreviation 'CNS' looks like 'ONS'. Recommend changing this for legibility (either the font or the spaces between letters). There were similarly a number of blank pages inserted into my reviewer copy. I recommend that the authors revise these points in future submissions.

Please note that in several points here I used capitals here to identify several suggested changes – this is to clarify that it is only to allow me to identify the specific point in the text I would recommend changing – no 'shouting' is intended.

Abstract

23: Suggest " we are experts at producing A VARIETY OF movements" rather than the current form (it could be taken to mean 'we are experts at producing variable movements' when most of the time when we think of expertise we observe low variability.

24: the scientific literature has shown that movement kinematics ARE rapidly adapted to new gravity conditions

31: FURTHERMORE, comparing the evolution…

Methods

Although I don't believe it is likely to affect the results of the present study, it is typical practice to report the gender breakdown of participants (Male/Female/Other).

Figure 1: Part A seems to illustrate that participants were not in the apparatus that held their feet in position when they performed the 'upright' reaching parts. Did participants disengage from the tilt-frame when performing upright reaching actions?

119: 'the participant was either standing head-up or head-down". Were participants "standing" while upside down, or is it more accurate to say they were 'supported by the tilt frame?'. I ask this question as standing would imply engagement of postural muscles used to hold an upright position, while hanging upside down from the tilt frame might not engage these muscles, or engage other muscles to help maintain the position, and so could lead to subtle differences in muscle coactivation patterns in each case

Study design section indicates that participants had a mandatory 30s break between each block. It would be good to indicate this on Figure 1 (I took the figure as showing that participants alternated between head-up and head-down reaching without any pauses). Similarly the 90s break periods between blocks 4-5 and 8-9 could be illustrated in Figure 1 to help clarify this.

136: Typo; participants performed as MANY pointing movements as possible…

Figure 2: The color lines with arrows are a nice way to show the 'head up' and 'head down' conditions, but I'd suggest also including a text label for each arrow for clarity.

Line 167 'carrying out repeated measure of variance analyses' – suggest including the abbreviation ANOVA to also clarify this was the test being used.

Line 172-177: Results: Nice to start with the qualitative description of the movement times in each condition, but it would also be nice to know in the paper if there was a significant difference between them?

Line 212: repetition of 'kinematic'.

254: typo 'procee'

295: 'are part of the programme fo the foot hanging' – suggest replacing this with a phrase along the line of 'are reported in other studies'.

Reviewed by anonymous reviewer 2, 10 March 2024

The authors assessed how reversing body orientation with respect to gravity (i.e., body upside down) affects the control of vertical arm movements (upward and downward) performed with a straight arm. Three main kinematic parameters were analyzed, namely the relative time to peak acceleration, velocity and deceleration, and these parameters were compared between the head up and the head down position (i.e., everyday orientation vs reversed orientation relative to the gravito-inertial vector). The authors observed that for the

first couple of blocks, body orientation relative to gravity significantly affected the relative time to peak velocity and deceleration. This effect vanished in the next blocks, indicating that the CNS 'required' some time and experience to optimize / re-optimize motor control taking body orientation with respect to gravity into account in order to minimize muscular effort.

The experiment is straight-forward and the results are clearly reported.

My main issue / question regarding the results relates to the way the kinematic parameters were 'computed'. In particular, I was wondering whether the pattern of results would be the same would the RtP(A, V, and D)s be normalized by movement duration (for each movement and direction). Put differently, because movement amplitude was (more or less) the same for all movements, I suspect (but I might be wrong) that the relative times to peak acceleration, velocity or deceleration might be different when performing a fast vs a slower movement. For instance, I would expect the relative time to peak velocity to occur 'earlier' for faster movements (and I want to emphasize that I'm really talking about relative time and make no confusion with absolute time, for which this fact seems obvious).

Regarding the statistical analyses, the authors mention that they assessed normality using the KS test. Why not the Shapiro-Wilk test? I'm asking that because though none of the two tests is very reliable for smaller sample sizes (say with less than 20-25 Ss), the Shapiro-Wilk test is more specific and highly recommended (because it has more power) with few data points (here only 18). Also, were the normality tests run on the residuals (this is not specified in the manuscript)?

Still regarding statistics, if I'm not mistaken, the Tukey HSD is not the most appropriate 'post-hoc' test for repeated measures, as one of the underlying assumptions is the independence of measures between conditions (groups on which means are computed).

There are still some typos in the manuscript. For instance:

line 136 '… participants performed as MANY pointing movements as possible…

line 193 ' As shown by a ONE-SAMPLE T-test…'