Centre of pressure versus centre of mass stabilization strategies: the tightrope balancing case

Pietro Morasso

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Review timeline
Original submission: 16 March 2020
1st revised submission: 12 July 2020
2nd revised submission: 14 August 2020
Final acceptance: 14 August 2020

Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Note: This manuscript was transferred from another Royal Society journal without peer review.

Review History

RSOS-200111.R0 (Original submission)

Review form: Reviewer 1

Is the manuscript scientifically sound in its present form?
No

Are the interpretations and conclusions justified by the results?
No

Is the language acceptable?
Yes

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
No

Recommendation?
Major revision is needed (please make suggestions in comments)
Comments to the Author(s)

Review: RSOS-200111
Title: CoP vs. CoM stabilization strategies: the tightrope balancing case

This manuscript describes a simulation study of a tightrope walker controlled in AP direction by an Ankle strategy and in ML direction by a CoM strategy. Overall this is an interesting study, and it is well written. However, I do have some concerns relating to the model used and the interpretation of the data, as outlined below.

Major
1. In equation 5, it seems that a linearized pendulum is assumed, because otherwise, both terms would have sin(q) instead of q. Why was a linearized pendulum used (why not a full pendulum?) and at the very least, it should be mentioned clearly in the text that this was done.
2. Why are reaction forces not taken into account? It seems that these may be quite important? Especially given that a displacement of the pole to the left would lead to a reaction force to the right, which would impose an opposite effect to the effect that is wanted.
3. Why is rotation of the beam not taken into account? It seems that in human balancing, the counter rotation mechanism (see Hof 2007) is thought of as important (see also one of our recent papers; van den Bogaart 2020)
4. The author used a simple euler integration, which, in general, is not suitable for integrating stochastic differential equations. I’m no expert on this, but know that for stochastic differential equations, more sophisticated integration procedures should be used.
5. Page 10 lines 9-10; here it is argued that the q_ap and q_ml are uncorrelated, which supports the hypotheses that these stabilization strategies can effectively function in an independent manner. However, it seems that this is quite logical here, as the Ap and ML equations are not coupled in any way, and hence, basically, 2 independent systems are simulated, both with a different controller. Hence, it should come at no surprise that the controllers function in an independent manner. Or do I oversee something here?
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References
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Hof AL. The equations of motion for a standing human reveal three mechanisms for balance. J Biomech. 2007;40(2):451-457. doi:10.1016/j.jbiomech.2005.12.016

Review form: Reviewer 2

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
No

Is the language acceptable?
Yes

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
No

Recommendation?
Accept with minor revision (please list in comments)

Comments to the Author(s)
Please see attached file (Appendix A).

Decision letter (RSOS-200111.R0)

We hope you are keeping well at this difficult and unusual time. We continue to value your support of the journal in these challenging circumstances. If Royal Society Open Science can assist you at all, please don’t hesitate to let us know at the email address below.

Dear Dr Morasso,

The editors assigned to your paper ("CoP vs. CoM stabilization strategies: the tightrope balancing case") have now received comments from reviewers. We would like you to revise your paper in accordance with the referee and Associate Editor suggestions which can be found below (not including confidential reports to the Editor). Please note this decision does not guarantee eventual acceptance.

Please submit a copy of your revised paper before 26-Jul-2020. Please note that the revision deadline will expire at 00.00am on this date. If we do not hear from you within this time then it will be assumed that the paper has been withdrawn. In exceptional circumstances, extensions may be possible if agreed with the Editorial Office in advance. We do not allow multiple rounds of revision so we urge you to make every effort to fully address all of the comments at this stage. If deemed necessary by the Editors, your manuscript will be sent back to one or more of the original reviewers for assessment. If the original reviewers are not available, we may invite new reviewers.
To revise your manuscript, log into http://mc.manuscriptcentral.com/rsos and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision. Revise your manuscript and upload a new version through your Author Centre.

When submitting your revised manuscript, you must respond to the comments made by the referees and upload a file "Response to Referees" in "Section 6 - File Upload". Please use this to document how you have responded to the comments, and the adjustments you have made. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response.

In addition to addressing all of the reviewers' and editor's comments please also ensure that your revised manuscript contains the following sections as appropriate before the reference list:

• Ethics statement (if applicable)
If your study uses humans or animals please include details of the ethical approval received, including the name of the committee that granted approval. For human studies please also detail whether informed consent was obtained. For field studies on animals please include details of all permissions, licences and/or approvals granted to carry out the fieldwork.

• Data accessibility
It is a condition of publication that all supporting data are made available either as supplementary information or preferably in a suitable permanent repository. The data accessibility section should state where the article's supporting data can be accessed. This section should also include details, where possible of where to access other relevant research materials such as statistical tools, protocols, software etc can be accessed. If the data have been deposited in an external repository this section should list the database, accession number and link to the DOI for all data from the article that have been made publicly available. Data sets that have been deposited in an external repository and have a DOI should also be appropriately cited in the manuscript and included in the reference list.

If you wish to submit your supporting data or code to Dryad (http://datadryad.org/), or modify your current submission to dryad, please use the following link: http://datadryad.org/submit?journalID=RSOS&manu=RSOS-200111

• Competing interests
Please declare any financial or non-financial competing interests, or state that you have no competing interests.

• Authors’ contributions
All submissions, other than those with a single author, must include an Authors’ Contributions section which individually lists the specific contribution of each author. The list of Authors should meet all of the following criteria; 1) substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data; 2) drafting the article or revising it critically for important intellectual content; and 3) final approval of the version to be published.

All contributors who do not meet all of these criteria should be included in the acknowledgements.

We suggest the following format:
AB carried out the molecular lab work, participated in data analysis, carried out sequence alignments, participated in the design of the study and drafted the manuscript; CD carried out the statistical analyses; EF collected field data; GH conceived of the study, designed the study, coordinated the study and helped draft the manuscript. All authors gave final approval for publication.
• Acknowledgements
Please acknowledge anyone who contributed to the study but did not meet the authorship criteria.

• Funding statement
Please list the source of funding for each author.

Once again, thank you for submitting your manuscript to Royal Society Open Science and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Kind regards,
Anita Kristiansen
Editorial Coordinator
Royal Society Open Science
openscience@royalsociety.org
on behalf of R. Kerry Rowe (Subject Editor)
openscience@royalsociety.org

Associate Editor's comments:

Comments to the Author:

Thank you for your patience while we sought reviewers at this difficult time - as we're sure you can imagine, many reviewers have faced extreme pressures on their time (moving to online teaching, remote working, home schooling or caring responsibilities, etc.), and the journal is grateful for the support of the referees who have provided substantial reviews here.

Based on the reviewers' commentary, we would like you to revise the manuscript, and the revision and associated point-by-point reply will be sent back to at least one, more likely both, of the reviewers for their view of the changes made. With this in mind, we urge you to take care to be sure you have fully responded to the referees' concerns before resubmitting.

Reviewers' Comments to Author:
Reviewer: 1

Comments to the Author(s)
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Hof AL. The equations of motion for a standing human reveal three mechanisms for balance. J Biomech. 2007;40(2):451-457. doi:10.1016/j.jbiomech.2005.12.016

Reviewer: 2

Comments to the Author(s)
Please see attached file. (Review for RSOS-200111.pdf)

Author’s Response to Decision Letter for (RSOS-200111.R0)

See Appendix B.
RSOS-200111.R1 (Revision)

Review form: Reviewer 1

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
Yes

Is the language acceptable?
Yes

Do you have any ethical concerns with this paper?
Yes

Have you any concerns about statistical analyses in this paper?
No

Recommendation?
Accept with minor revision (please list in comments)

Comments to the Author(s)

Previous comment 8. Page 9, line 30 or so: It would be good to show for both controllers the imposed torques, so that the reader gets a better idea about what actually is happening?
Author answer: As a matter of fact the control torques are plotted in figure 4 (now figure 5). Panel B compares, for the ML controller, the control torque, with the destabilizing gravity torque. Panel A shows, for the AP controller, the same information because the CoP position is proportional to the control torque and the CoM position to the gravity torque.
New comment: but why not show the torques for panel a? This way, the figure is more similar to figure b.

Previous comment 9. Figure 5 AP and ML are switched.
Author answer: I don’t think so.
New comment: my meaning was that in all previous figures, AP was plotted first, and then ML. In figure 5 (now 6), AP is on the right, and ML on the left. This may be confusing, as AP is always on the left.

Previous comment 10. I was able to run the supplied code without any problems, but it did not directly lead to plots of the results, and it may also not be immediately obvious how to do so. It would be great if the code could be extended, such that such plots are also generated.
Author answer: I must say that there is no way to extend the code in order to reproduce exactly the plotted patterns. With reference to the answer to the major question no. 4, the numerical integration of the equations of sway in the two planes is based on repeated random sampling to obtain numerical results. Thus any simulation run provides independent examples of the sway patterns. The patterns illustrated in the figures are just a representative example of the underlying stochastic process. I added a sentence about that at the beginning of the Results section.
New comment: but if the plots were generated at the end of the script, this would already give the interested reader more idea of which variables are what, and how plots are generated. In addition, by setting the random number generator to a certain state, it should be possible to create a script that exactly produces the figures as they are seen in the paper.
Review form: Reviewer 2

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
Yes

Is the language acceptable?
Yes

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
No

Recommendation?
Accept with minor revision (please list in comments)

Comments to the Author(s)
Two minor comments about Figure 3:
(1) units on the y axes - should they be deg/sec?
(2) author should consider using same range along x and y axes for the two subplots - it might help in comparing the two plots.

All other concerns have been addressed.
I am excited by the ideas in this paper, and I recommend that the manuscript be accepted for publication.

Decision letter (RSOS-200111.R1)

We hope you are keeping well at this difficult and unusual time. We continue to value your support of the journal in these challenging circumstances. If Royal Society Open Science can assist you at all, please don’t hesitate to let us know at the email address below.

Dear Dr Morasso

On behalf of the Editors, we are pleased to inform you that your Manuscript RSOS-200111.R1 "CoP vs. CoM stabilization strategies: the tightrope balancing case" has been accepted for publication in Royal Society Open Science subject to minor revision in accordance with the referees' reports. Please find the referees' comments along with any feedback from the Editors below my signature.

We invite you to respond to the comments and revise your manuscript. Below the referees’ and Editors’ comments (where applicable) we provide additional requirements. Final acceptance of your manuscript is dependent on these requirements being met. We provide guidance below to help you prepare your revision.

Please submit your revised manuscript and required files (see below) no later than 7 days from today’s (ie 10-Aug-2020) date. Note: the ScholarOne system will ‘lock’ if submission of the
revision is attempted 7 or more days after the deadline. If you do not think you will be able to meet this deadline please contact the editorial office immediately.

Please note article processing charges apply to papers accepted for publication in Royal Society Open Science (https://royalsocietypublishing.org/rsos/charges). Charges will also apply to papers transferred to the journal from other Royal Society Publishing journals, as well as papers submitted as part of our collaboration with the Royal Society of Chemistry (https://royalsocietypublishing.org/rsos/chemistry). Fee waivers are available but must be requested when you submit your revision (https://royalsocietypublishing.org/rsos/waivers).

Thank you for submitting your manuscript to Royal Society Open Science and we look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Kind regards,
Royal Society Open Science Editorial Office
Royal Society Open Science
openscience@royalsociety.org

on behalf of Prof R. Kerry Rowe (Subject Editor)
openscience@royalsociety.org

Associate Editor Comments to Author:
Thank you for so constructively engaging with the reviewers' and the editor's comments. A handful of changes are left to be incorporated before the paper may be accepted.

Reviewer comments to Author:
Reviewer: 2

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===PREPARING YOUR MANUSCRIPT===

Your revised paper should include the changes requested by the referees and Editors of your manuscript. You should provide two versions of this manuscript and both versions must be provided in an editable format:

- one version identifying all the changes that have been made (for instance, in coloured highlight, in bold text, or tracked changes);
- a 'clean' version of the new manuscript that incorporates the changes made, but does not highlight them. This version will be used for typesetting.

Please ensure that any equations included in the paper are editable text and not embedded images.

Please ensure that you include an acknowledgements' section before your reference list/bibliography. This should acknowledge anyone who assisted with your work, but does not qualify as an author per the guidelines at https://royalsociety.org/journals/ethics-policies/openness/.

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Please ensure that you include a summary of your paper at Step 2 ‘Type, Title, & Abstract’. This should be no more than 100 words to explain to a non-scientific audience the key findings of your research. This will be included in a weekly highlights email circulated by the Royal Society press office to national UK, international, and scientific news outlets to promote your work.

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  1) One version identifying all the changes that have been made (for instance, in coloured highlight, in bold text, or tracked changes);
  2) A 'clean' version of the new manuscript that incorporates the changes made, but does not highlight them.
-- An individual file of each figure (EPS or print-quality PDF preferred [either format should be produced directly from original creation package], or original software format).
-- An editable file of each table (.doc, .docx, .xls, .xlsx, or .csv).
-- An editable file of all figure and table captions.
Note: you may upload the figure, table, and caption files in a single Zip folder.
-- Any electronic supplementary material (ESM).
-- If you are requesting a discretionary waiver for the article processing charge, the waiver form must be included at this step.
-- If you are providing image files for potential cover images, please upload these at this step, and inform the editorial office you have done so. You must hold the copyright to any image provided.
-- A copy of your point-by-point response to referees and Editors. This will expedite the preparation of your proof.

At Step 6 'Details & comments', you should review and respond to the queries on the electronic submission form. In particular, we would ask that you do the following:
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-- If you are requesting an article processing charge waiver, you must select the relevant waiver option (if requesting a discretionary waiver, the form should have been uploaded at Step 3 'File upload' above).
-- If you have uploaded ESM files, please ensure you follow the guidance at https://royalsociety.org/journals/authors/author-guidelines/#supplementary-material to include a suitable title and informative caption. An example of appropriate titling and captioning may be found at https://figshare.com/articles/Table_S2_from_Is_there_a_trade-off_between_peak_performance_and_performance_breadth_across_temperatures_for_aerobic_scope_in_teleost_fishes_/3843624.

At Step 7 'Review & submit', you must view the PDF proof of the manuscript before you will be able to submit the revision. Note: if any parts of the electronic submission form have not been completed, these will be noted by red message boxes.

Author’s Response to Decision Letter for (RSOS-200111.R1)

See Appendix C.
We hope you are keeping well at this difficult and unusual time. We continue to value your support of the journal in these challenging circumstances. If Royal Society Open Science can assist you at all, please don't hesitate to let us know at the email address below.

Dear Dr Morasso,

It is a pleasure to accept your manuscript entitled "CoP vs. CoM stabilization strategies: the tightrope balancing case" in its current form for publication in Royal Society Open Science.

You can expect to receive a proof of your article in the near future. Please contact the editorial office (openscience_proofs@royalsociety.org) and the production office (openscience@royalsociety.org) to let us know if you are likely to be away from e-mail contact -- if you are going to be away, please nominate a co-author (if available) to manage the proofing process, and ensure they are copied into your email to the journal.

Due to rapid publication and an extremely tight schedule, if comments are not received, your paper may experience a delay in publication. Royal Society Open Science operates under a continuous publication model. Your article will be published straight into the next open issue and this will be the final version of the paper. As such, it can be cited immediately by other researchers. As the issue version of your paper will be the only version to be published I would advise you to check your proofs thoroughly as changes cannot be made once the paper is published.

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Thank you for your fine contribution. On behalf of the Editors of Royal Society Open Science, we look forward to your continued contributions to the Journal.

Best regards,
Lianne Parkhouse
Editorial Coordinator
Royal Society Open Science
openscience@royalsociety.org

on behalf of the Associate Editor and Professor R. Kerry Rowe (Subject Editor)
openscience@royalsociety.org

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Appendix A

Review for RSOS-200111.

The author has outlined an interesting mechanism for upright balance control for the case when the balance task constrains the COP under the feet to have minimal motion in certain directions (ML in the present manuscript). The key idea is that when the COP cannot function as a control variable due to the above constraint, the COM is controlled instead via the movements of the body segments. The example used in the manuscript is tightrope walking, and the COM is controlled by moving the pole that the walker carries. The author provides simulation data only, and speculates about the application of this notion to human movements.

This is an interesting proposal. Barring a few points of confusion regarding interpretation and extension of this idea, the application of the proposal to a variety of movements that may be considered as 'non-traditional' to biomechanics and motor control communities makes this manuscript quite attractive for publication.

The points of confusion that are pointed out first. This is followed by requests for minor clarifications.

Overall, this reviewer found the second and the fourth paragraphs of the Discussion a bit discursive, and not very helpful.

The first and main point of confusion is the claim that the COPS vs COMS strategies are a generalization of the ankle vs hip strategy for sway control in the sagittal plane. The author makes this claim in the Discussion (page 11, and again on page 12 in the context of hand stands). This generalization is underdeveloped, and frankly, does not seem necessary for this manuscript. First, on what basis is hip engagement for maintaining balance considered a COMS strategy? Does engaging hip motion/torques (presumably in addition to ankle involvement) change COM only? Generating hip torques could change COP through inter-joint coupling. Second, the author switches from the decoupling of planes of motion (frontal vs sagittal) in the presented model to two strategies elucidated in the sagittal plane. It seems that this claim will need evidence, and such evidence is not provided in this manuscript.

The second point of confusion is that the author states: ‘The two angular oscillations (\(q_{ap}\) and \(q_{ml}\)) are uncorrelated, supporting the hypothesis that the two stabilization strategies can effectively function in an independent manner.’ This is a confusing because the model decouples the movements in the two planes. Isn’t it obvious that the output coordinates would be uncorrelated? Furthermore, it is known that delay-differential systems can be stabilized with feedback. So, using this data (or any simulation data presented in this manuscript) to support this ‘hypothesis’ is neither interesting nor illuminating.

Minor clarifications:
1. Please explain the dis/activation condition in equations 9 and 10.
2. This reviewer was unable to follow the ‘Control Action’ in equation 10. Please explain.
3. This reviewer was also unable to understand ‘alpha’: the lines 49-55 on page 7 are confusing.
4. Page 10-11: “We found that with a 60% pole reduction it was impossible to avoid a quick fall and for a 70% reduction stability could be achieved but only for limited stretches of time (30-60 s).” How come a larger reduction in pole length resulted in stable behavior? Is this a typo?
Appendix B

Anita Kristiansen
Editorial Coordinator - Royal Society Open Science

Review: RSOS-200111
Title: CoP vs. CoM stabilization strategies: the tightrope balancing case

Associate Editor’s comments:
Comments to the Author:
Thank you for your patience while we sought reviewers at this difficult time - as we’re sure you can imagine, many reviewers have faced extreme pressures on their time (moving to online teaching, remote working, home schooling or caring responsibilities, etc.), and the journal is grateful for the support of the referees who have provided substantial reviews here.
Based on the reviewers’ commentary, we would like you to revise the manuscript, and the revision and associated point-by-point reply will be sent back to at least one, more likely both, of the reviewers for their view of the changes made. With this in mind, we urge you to take care to be sure you have fully responded to the referees' concerns before resubmitting.

I read carefully the reviewers’ commentary, all of them well formulated and useful for improving the manuscript. I did my best to respond to modify the manuscript which is included in the revised version. The point by point answers (highlighted in yellow) to the reviewers’ concerns are included.
Reviewer #1

Comments to the Author(s)

Review: RSOS-200111

Title: CoP vs. CoM stabilization strategies: the tightrope balancing case

This manuscript describes a simulation study of a tightrope walker controlled in AP direction by an Ankle strategy and in ML direction by a CoM strategy. Overall this is an interesting study, and it is well written. However, I do have some concerns relating to the model used and the interpretation of the data, as outlined below.

I am quite grateful to this reviewer for his/her deep insight and useful suggestions.

Major

1. In equation 5, it seems that a linearized pendulum is assumed, because otherwise, both terms would have sin(q) instead of q. Why was a linearized pendulum used (why not a full pendulum?) and at the very least, it should be mentioned clearly in the text that this was done.

   Yes, the pendulum equation is linearized for simplicity. This is mentioned in the text. Considering that the simulations shows that both angles are consistently smaller than 1 deg changing q with sin(q) would not make any difference.

2. Why are reaction forces not taken into account? It seems that these may be quite important? Especially given that a displacement of the pole to the left would lead to a reaction force to the right, which would impose an opposite effect to the effect that is wanted?

   As stated in the introduction the model includes several simplifications, focusing on the feasibility of the intermittent control hypothesis for the stabilization in the coronal plane. In the preliminary version of the manuscript I listed 4 simplifications. In the review I add two. The fifth is related to the reaction forces that are neglected for two reasons, derived from the observation of videos of famous long distance tightrope walkers like Nick Wallenda: 1) the lateral movements of the pole are very slow, 2) as shown in the picture below, funambulists typically unload the weight of the pole with a strap on the shoulder and this attachment might have a damping effect on the reaction force. However, in principle the reaction force effect can be introduced in the model for a future deeper analysis.
3. Why is rotation of the beam not taken into account? It seems that in human balancing, the counter rotation mechanism (see Hof 2007) is thought of as important (see also one of our recent papers; van den Bogaart 2020).

   The answer is similar to the previous one. This is the sixth simplification of the model, motivated by the fact that observing videos of long distance walkers it appears that, for example in the case of Nick Wallenda walking over the Niagara falls, the pole is barely rotated during the more than 10 minutes walk. By increasing as much as possible the falling time constant (with a very long pole) it appears that the funambulist can also maximize the slowing down of the control action, reducing it to the quasi-static component. Of course there are balancing tasks, like slackline balancing, where the dynamics of the balancing action is strongly increased and in that case reaction forces or torques may be predominant.

4. The author used a simple Euler integration, which, in general, is not suitable for integrating stochastic differential equations. I'm no expert on this, but know that for stochastic differential equations, more sophisticated integration procedures should be used?

   The problem is not the Euler method per se, which is the simplest method of numerical integration of differential equations. Runge-Kutta or predictor-corrector methods would face the same criticism, due to the fact that, in principle, the solution of equation 4, i.e. q(t), is a stochastic process. The sophisticated techniques of integration of stochastic differential equation (SDE) are difficult to apply in our case due to the strong non linearity introduced by the intermittent controller. The solution adopted in the simulation experiments is based on the rationale of Monte Carlo methods, i.e. computational algorithms that rely on repeated random sampling to obtain numerical results which allow to estimate relevant statistic indicators of the underlying stochastic process.

5. Page 10 lines 9-10; here it is argued that the q_ap and q_ml are uncorrelated, which supports the hypotheses that these stabilization strategies can effectively function in an independent manner. However, it seems that this is quite logical here, as the Ap and ML equations are not coupled in any way, and hence, basically, 2 independent systems are simulated, both with a different controller. Hence, it should come at no surprise that the controllers function in an independent manner. Or do I oversee something here?

   As a matter of fact there is a direct, but small coupling effect due to the fact that the inertia matrix is not diagonal, although I admit that the off-diagonal terms are much smaller than the diagonal ones. However, the intermittency of the control actions might induce, in principle, some kind of dangerous entrainment of the two oscillatory patterns that apparently does not occur in the simulation experiments.

6. It seems that the last paragraph of the results is actually a more suitable paragraph for in the discussion?

   Thank you for the suggestion. I broke this paragraph in two parts. The first one is left in the results and the second one is shifted to the re-written discussion.

Minor

1. It’s odd that the authors don’t cite Hof et al (2007), which seems to fit perfectly in the first paragraph of the introduction.

   Good suggestion, thank you.
2. Page 3, line 47: “In front” - “in front”

Corrected, thank you.

3. Page 4 line 33 (and maybe other places) :“we” or “I” as it’s a single author paper.

Thank you for the observation. Actually I intended to use the royal we but this is not appropriate. However, using the first person implies an expression of subjective will, which perhaps is not appropriate either. Thus I turned to the passive form in the third person when I referred to a choice, as in the quoted example, and I kept we when referring to a fact or an opinion that may be shared by the community of researchers.

4. Page 7 line 40; it’s unclear what the q_p means, also not explained in the text?

As a matter of fact q_p is shown in figure 1. However, as suggested, I added the definition in the text: q_p is the angle between the line from the rope to the CoM of the pole and the vertical.

5. Page 7, equation 10; what lowpass filter was used? What cut off etc? I realize that this is mentioned in the table with parameters, but it may be good to also mention it here.

The transfer functions of the filter is $F(s) = \frac{1}{(s/\omega_f)^2+2(\omega_f)+1}$

6. Page 7 line 49; it would be good to have a plot of the phase plane, with indications of stable and unstable regions, and some indication of how the controller acts.

Good suggestion. Instead of adding a new figure I replotted the old figure 2 (now figure 3) color coding the segments related to the off-phases and on-phases respectively. Please note the slight difference between the shapes of the old figure 2 and the new figure 3, due to the random sampling issue, named in my answer to your question 10.

7. Page 8, line 10; the feedback delay is set at 200ms, yet there are short term reflexes that could be quicker, would this change the results much?

This is hard to say. Certainly there are quick reflexes that do not involve cortical processing and are probably associated to limited/local groups of muscles, for example for maintaining the approximate alignment of the body inverted pendulum. The postulated intermittent controller operates at a higher, global level and a feedback delay of the order of 200 ms is reasonable. In any case, one of the purposes of the simulation study is to show that even with such a large delay the system can be stabilized with a slow bounded stability.

8. Page 9, line 30 or so: It would be good to show for both controllers the imposed torques, so that the reader gets a better idea about what actually is happening?

As a matter of fact the control torques are plotted in figure 4 (now figure 5). Panel B compares, for the ML controller, the control torque, with the destabilizing gravity torque. Panel A shows, for the AP controller, the same information because the CoP position is proportional to the control torque and the CoM position to the gravity torque.

9. Figure 5 AP and ML are switched.

I don’t think so.
10. I was able to run the supplied code without any problems, but it did not directly lead to plots of the results, and it may also not be immediately obvious how to do so. It would be great if the code could be extended, such that such plots are also generated.

I must say that there is no way to extend the code in order to reproduce exactly the plotted patterns. With reference to the answer to the major question no. 4, the numerical integration of the equations of sway in the two planes is based on repeated random sampling to obtain numerical results. Thus any simulation run provides independent examples of the sway patterns. The patterns illustrated in the figures are just a representative example of the underlying stochastic process. I added a sentence about that at the beginning of the Results section.
Reviewer #2

Comments to the Author(s)

Review: RSOS-200111

Title: CoP vs. CoM stabilization strategies: the tightrope balancing case. The author has outlined an interesting mechanism for upright balance control for the case when the balance task constrains the COP under the feet to have minimal motion in certain directions (ML in the present manuscript). The key idea is that when the COP cannot function as a control variable due to the above constraint, the COM is controlled instead via the movements of the body segments. The example used in the manuscript is tightrope walking, and the COM is controlled by moving the pole that the walker carries. The author provides simulation data only, and speculates about the application of this notion to human movements.

This is an interesting proposal. Barring a few points of confusion regarding interpretation and extension of this idea, the application of the proposal to a variety of movements that may be considered as ‘nontraditional’ to biomechanics and motor control communities makes this manuscript quite attractive for publication.

I thank this reviewer for identifying the weak points of my work and helping me to improve it in a significant manner.

The points of confusion that are pointed out first. This is followed by requests for minor clarifications.

Overall, this reviewer found the second and the fourth paragraphs of the Discussion a bit discursive, and not very helpful.

The Discussion session has been re-written in order to take into account the criticism and suggestions of both reviewers.

The first and main point of confusion is the claim that the COPS vs COMS strategies are a generalization of the ankle vs hip strategy for sway control in the sagittal plane. The author makes this claim in the Discussion (page 11, and again on page 12 in the context of hand stands). This generalization is underdeveloped, and frankly, does not seem necessary for this manuscript. First, on what basis is hip engagement for maintaining balance considered a COMS strategy? Does engaging hip motion/torques (presumably in addition to ankle involvement) change COM only? Generating hip torques could change COP through interjoint coupling. Second, the author switches from the decoupling of planes of motion (frontal vs sagittal) in the presented model to two strategies elucidated in the sagittal plane. It seems that this claim will need evidence, and such evidence is not provided in this manuscript.

Hip engagement for maintaining balance is an example of utilizing the COMS strategy when environmental conditions (e.g. limited size of the support basis, soft standing surface) or physiological conditions (e.g. in the case of elderly people) reduce significantly the feasible range of motion of the CoP. However, this does not imply that, in general, engaging hip motion/torques mostly changes the CoM: hip torques could indeed change CoP through interjoint coupling if the BoS is sufficiently large. In other words, the clearcut distinction between COPS and COMS is that in the former case the oscillation of the CoP is larger and anticipates the oscillation of the CoM, whereas in the latter case it is the other way around and it achieves stability by engaging different body parts.
The second point of confusion is that the author states: ‘The two angular oscillations (\(qa_p\) and \(q_m\)) are uncorrelated, supporting the hypothesis that the two stabilization strategies can effectively function in an independent manner.’ This is a confusing because the model decouples the movements in the two planes. Isn’t it obvious that the output coordinates would be uncorrelated? Furthermore, it is known that delay-differential systems can be stabilized with feedback. So, using this data (or any simulation data presented in this manuscript) to support this ‘hypothesis’ is neither interesting nor illuminating.

As a matter of fact there is a direct, but small coupling effect due to the fact that the inertia matrix is not diagonal, although I admit that the off-diagonal terms are much smaller than the diagonal ones. However, the intermittency of the control actions might induce, in principle, some kind of dangerous entrainment of the two oscillatory patterns that apparently does not occur in the simulation experiments.

Minor clarifications:

1. Please explain the dis/activation condition in equations 9 and 10.

The activation/dis-activation conditions of equations 9 and 10 correspond to the fact that the trajectory of the delayed state vector enters one region coming from the other (see figure 2): from safe to unsafe or vice versa. Please refer to the new figure 2, specifically added for clarifying the point.

2. This reviewer was unable to follow the ‘Control Action’ in equation 10. Please explain.

The following text is inserted after equation 10.

The control variable \(\gamma(t)\) consists of displacing the pole CoM in the opposite direction of the body CoM disequilibrium (\(\gamma = -(Pq + Dq)\)) during the on-phase, whereas the two CoMs are kept aligned in the off-phase (\(\gamma = 0\)). Since this is a discontinuous variable it will be smoothed out by the arm control system. For simplicity, this smoothing is implemented in the simulation package by a simple low-pass filter (LPF), which is characterized by the following transfer function: \(F(s) = \frac{1}{(s/\omega_f)^2 + 2\xi(s/\omega_f) + 1}\). The control action, namely the generation of the torque \(T_{ml}\) supposed to compensate the destabilizing effect of gravity, is then the biomechanical consequence of this controlled shift.

3. This reviewer was also unable to understand ‘alpha’: the lines 49-55 on page 7 are confusing.

The rationale of alpha is explained in the new figure 2.

4. Page 10-11: “We found that with a 60% pole reduction it was impossible to avoid a quick fall and for a 70% reduction stability could be achieved but only for limited stretches of time (30-60 s).” How come a larger reduction in pole length resulted in stable behavior? Is this a typo?

It is not a typo but a badly expressed evaluation: by “70% reduction” I intended “reduction of the pole length to 70% of the original length”
Appendix C

Reviewer: 1

Thank you for your patience and attention

Comments to the Author(s)

Previous comment 8. Page 9, line 30 or so: It would be good to show for both controllers the imposed torques, so that the reader gets a better idea about what actually is happening?

Author answer: As a matter of fact the control torques are plotted in figure 4 (now figure 5). Panel B compares, for the ML controller, the control torque, with the destabilizing gravity torque. Panel A shows, for the AP controller, the same information because the CoP position is proportional to the control torque and the CoM position to the gravity torque.

New comment: but why not show the torques for panel a? This way, the figure is more similar to figure b.

Good suggestion: I added a second scale on panel A that allows the comparison among the two oscillations in terms of torque.

Previous comment 9. Figure 5 AP and ML are switched.

Author answer: I don’t think so.

New comment; my meaning was that in all previous figures, AP was plotted first, and then ML. In figure 5 (now 6), AP is on the right, and ML on the left. This may be confusing, as AP is always on the left.

Agreed. Thank you for the suggestion. My previous answer was due to a misunderstanding of your comment.

Previous comment 10. I was able to run the supplied code without any problems, but it did not directly lead to plots of the results, and it may also not be immediately obvious how to do so. It would be great if the code could be extended, such that such plots are also generated.

Author answer: I must say that there is no way to extend the code in order to reproduce exactly the plotted patterns. With reference to the answer to the major question no. 4, the numerical integration of the equations of sway in the two planes is based on repeated random sampling to obtain numerical results. Thus any simulation run provides independent examples of the sway patterns. The patterns illustrated in the figures are just a representative example of the underlying stochastic process. I added a sentence about that at the beginning of the Results section.

New comment: but if the plots were generated at the end of the script, this would already give the interested reader more idea of which variables are what, and how plots are generated. In addition, by setting the random number generator to a certain state, it should be possible to create a script that exactly produces the figures as they are seen in the paper.

I added the following sentence in the Data Availability section: “Each simulation run lasts 240 s (changed by the user if required). For each run the random number generators are reset, thus the generated oscillatory patterns are independent. As a consequence, the reported graphs are just representative
samples, slightly different from trial to trial, obtained by plotting $q_1$ (body sway in the sagittal plane) and $q_2$ (body sway in the coronal plane) and the corresponding derivatives."
Reviewer comments to Author:
Reviewer: 2

Comments to the Author(s)

Two minor comments about Figure 3:

(1) units on the y axes - should they be deg/sec?

Correct, thank you for the indication.

(2) author should consider using same range along x and y axes for the two subplots - it might help in comparing the two plots.

Your suggestion is a reasonable possibility, but I preferred to leave the two subplots with different scales for purely graphical reasons because the B subplot would have been strongly compressed. For taking into account your concern I added a note in the figure caption. Moreover, figure 4 captures at least one part of your suggestion because the two oscillations are plotted on purpose with the same scale in order to enhance the different ranges.

All other concerns have been addressed.

Thanks for your patience and attention.

I am excited by the ideas in this paper, and I recommend that the manuscript be accepted for publication.