Peer Review File

Manuscript Title: Acceleration of 1I/`Oumuamua from radiolytically produced H2 in H2O ice

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

A Summary of Key results:

This paper proposes a theory and theoretical calculations to explain some of the observed characteristics of 1I/'Oumuamua, centred around the observed comet-like non-gravitational acceleration. They propose it originates in the release of H2, which itself is naturally produced via dissociation of H2O in the body by GCRs over long timescales. The study purports to provide a self-consistent picture of the behaviour of 1I, with it being an originally H2O-rich planetesimal that had subsurface H2 generated in interstellar space and was subsequently outgassed during its solar passage.

While an H2-rich composition has been suggested before for 1I, the new science in this study is using previously established studies of icy body GCR processing, using it to produce quantitative calculations of the outgassing, and showing it to be a plausible match to the observed non-gravitational forces. Additionally, H2 release also qualitatively explains the lack of observed dust around the object, due to the reduced gas-drag on dust particles relative to normal comets.

B Originality and Significance

Overall, the authors have written a convincing argument to explain the nature of 1I. On the whole, this explanation of acceleration forms a physically plausible picture, and is worthy of publishing in Nature.

C Data and Methodology

The submitted manuscript does not comment on some details and raises several questions that are not addressed, particularly in terms of self-consistency of their arguments and modelling. Some of these issues may already be answered in their modelling, maybe the manuscript simply needs some extra clarity in places. Because of this, I cannot recommend acceptance of the current version, but I would like to see the answers to the questions below in section F.

D Appropriate Use of Statistics and Treatment of Uncertainties
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Fine, no comment.

E Conclusions

Qualitatively the conclusions appear to be sound. The robustness of these needs to be addressed in the comments below.

F Suggested Improvements
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1. According to equation (2), the model integrates the H2 flux over the entire surface of the body. Yet as in the case of normal comets, to get a non-grav acceleration you need an imbalance in the mass-loss, with most occurring on the sunlit hemisphere to match the reported anti-solar acceleration. Hence the acceleration from equation 2 is at least a factor ~1/2 less than calculated, and probably more, see next (linked) point.

2. In the modelling section the authors frequently refer to a "jet" of H2. They do not describe their assumptions of the morphology of this jet. In the text it clearly conflicts with the assumption of whole-surface release of H2. Is it simply emission from the sunlit hemisphere, or more localised? If the whole hemisphere, do they take into account the momentum transfer as normal to the local surface? The more localised the jet emission, the higher the rate of H2 loss required to match the observed acceleration. What they mean by the "jet" of H2 should be clearly described and quantified.

3. I found it peculiar that the authors did not actually give the mass loss rate of H2 required to match the observations anywhere. I suggest giving a mass loss rate at a fiducial distance of 1.25 au, where there are also observations used in Micheli et al. (2018).

4. It seems clear that from figure 2 that for either of the thermal inertias modelled, the near surface temperature at ~1-1.5 au or more would be > 180K, allowing for H2O sublimation to also take place. Indeed they state "While higher surface temperatures are possible (Figure 2), the water matrix will begin to sublimate somewhat above 140 K". But surely there is an inconsistency here - if their own models predict higher temperatures allowing H2O sublimation, why do they ignore it? Given that they assume >50% of the H2O remains in the subsurface layers, this would have an additional effect on the non-gravitational acceleration. The authors should at least estimate this additional push from H2O sublimation, in context for the H2-induced acceleration.

5. Figure 3 appears to show that H2 release could have occurred from well before perihelion. This raises the question whether the subsurface layers would have lost a substantial amount of H2 before observation at > 1.2 au post-perihelion. The authors need to briefly show there would still be enough H2 left to produce the observed acceleration.

G References

These were all suitable and well chosen, although sometimes I felt some of the previous work did not need all the references given.
Abstract line 10: Reference 5 is really a dynamical paper and takes the non-detection of a dust/gas coma from other studies.

Page 2 line 30/31: The phrase "sufficient energy received from solar irradiation to power the non-ballistic trajectory" is a little unclear. The authors should simply say H_2O and CO_2 sublimation could not produce the observed non-gravitational acceleration.

H Clarity and Context

The paper itself was very well written in terms of English and grammar. My only suggestion for change is

Page 2 line 30/31: The phrase "sufficient energy received from solar irradiation to power the non-ballistic trajectory" is a little unclear. The authors should simply say H_2O and CO_2 sublimation could not produce the observed non-gravitational acceleration.

Referee #2 (Remarks to the Author):

The manuscript presents a clear, physically motivated and, most importantly, simple model that can explain the non-gravitational acceleration detected on 1I, without recurring to exotic physical or formation processes.
The entire model is in my opinion convincing, and possibly the most reasonable explanation provided so far for the anomalous acceleration observed on this interstellar object.

I do not see any evident flaw in any of the work presented in the manuscript.
Nevertheless, I have some minor comments that I would recommend the authors consider before finalizing the manuscript for publication.

- L. 31: in my mind, the word "ballistic" suggests the motion of something under a simple, often single-source, gravity field. In this case, the anomaly in the motion of 1I is present even after accounting for more complex gravitational terms (e.g. the effects of planets, asteroids, and general relativity). I would therefore recommend a word like "non-gravitational", implying that all gravitational contributions have been taken into account.

- The initial literature review (L. 1-37) is extremely well written, synthetic and complete, and covers all the key points of the topic. I might however recommend to the authors to at least briefly summarize some of the key properties of the non-gravitational acceleration they are modeling in the main paper (e.g. the radial direction and its radial dependency), since they may be unfamiliar to a reader without immediate access to the original references.

- L. 57-58: this paragraph properly states that the quoted dimensions are degenerate with the albedo, but since they are dramatically dependent on the assumed albedo I would suggest mentioning the value for which they were derived (especially since they are quoted with many significant digits, which are meaningless if the albedo is not constrained).

- The paper does not directly mention or explain that H_2 emission would not have been observationally evident in any of the datasets we have for 1I. This might be a trivial point, but I suggest at least briefly mentioning it for the sake of a non-experienced reader.

- L. 99: this statement (the water matrix becoming more crystalline towards the surface) is not immediately obvious to me. I suggest adding either a reference or a brief explanation.

- The authors nicely explain (L. 126-130) why this process is effective and relevant on a small object like 1I, while becoming less meaningful for larger km-sized comets like the ones we routinely see in the solar system. However, is there anything in the proposed process that might explain why 1I is the only ~100 m comet we've observed so far?

- Methods and Figure 1 caption: can the authors better explain how "T_jet" relates to the temperature of the object's ice? There is something in subsequent paragraphs (e.g. L. 173-182), but it comes after the equations and Figure 1 are introduced, and does not completely discuss the topic.

- The use of the letter "A" for the geometric albedo is a bit unusual in the field of small bodies. I might recommend using "p" for it, and reserving "A" if referring to the Bond albedo.

In addition to the scientific and technical comments above, I would also like to point out a few minor editorial suggestions:

- If I remember correctly, titles of Nature papers are limited to 75 characters. The proposed title is longer, I recommend shortening it.

- I do not see a "Data Availability" statement, but I assume it's not particularly relevant for this paper, since it doesn't make use of any newly acquired dataset.

- A very minor point, which I hope can be ignored with the approval of the editor, is the fact that more than 50 references are used in the main text. If I remember correctly, Nature has a stricter limit. In case this needs to be enforced for editorial reasons, there might be some multiple references for the same topic that can be removed, but I'd recommend keeping everything if the editor agrees.

And a few typos I noticed while reading the manuscript:

- L. 109: should read "Micheli et al." if the plural form "posit" is used.

- L. 120: "constraints... are important".

- Figure 2-3: I recommend re-drawing the figures using the symbol "au" lowercase, to be consistent with the rest of the paper.

- L. 186: "albedo".

- L. 198-199: many of the spaces between numbers and units in this part of the text are incorrect.

Author Rebuttals to Initial Comments:

Nature manuscript 2022-09-14667A: Resubmission V1: Response to Referees
Authors: Jennifer Bergner, Darryl Z. Seligman

Response to Reviewer 1's Comments:

A Summary of Key results:

This paper proposes a theory and theoretical calculations to explain some of the observed characteristics of 1I/'Oumuamua, centred around the observed comet-like non-gravitational acceleration. They propose it originates in the release of H2, which itself is naturally produced via dissociation of H2O in the body by GCRs over long timescales. The study purports to provide a self-consistent picture of the behaviour of 1I, with it being an originally H2O-rich planetesimal that had subsurface H2 generated in interstellar space and was subsequently outgassed during its solar passage.

While an H2-rich composition has been suggested before for 1I, the new science in this study is using previously established studies of icy body GCR processing, using it to produce quantitative calculations of the outgassing, and showing it to be a plausible match to the observed non-gravitational forces. Additionally, H2 release also qualitatively explains the lack of observed dust around the object, due to the reduced gas-drag on dust particles relative to normal comets.

B Originality and Significance

Overall, the authors have written a convincing argument to explain the nature of 1I. On the whole, this explanation of acceleration forms a physically plausible picture, and is worthy of publishing in Nature.

We thank Reviewer 1 for the encouraging words and the insightful and constructive report.

C Data and Methodology

The submitted manuscript does not comment on some details and raises several questions that are not addressed, particularly in terms of self-consistency of their arguments and modelling. Some of these issues may already be

answered in their modelling, maybe the manuscript simply needs some extra clarity in places. Because of this, I cannot recommend acceptance of the current version, but I would like to see the answers to the questions below in section F.

We hope that we have now addressed the outstanding questions that were raised in our previously submitted manuscript.

D Appropriate Use of Statistics and Treatment of Uncertainties
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Fine, no comment.

E Conclusions

Qualitatively the conclusions appear to be sound. The robustness of these needs to be addressed in the comments below.

F Suggested Improvements
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1. According to equation (2), the model integrates the H2 flux over the entire surface of the body. Yet as in the case of normal comets, to get a non-grav acceleration you need an imbalance in the mass-loss, with most occurring on the sunlit hemisphere to match the reported anti-solar acceleration. Hence the acceleration from equation 2 is at least a factor ~1/2 less than calculated, and probably more, see next (linked) point.
Equation 2 represents the total number of H2 molecules that need to be sublimating per unit time in order to explain the non-gravitational acceleration. There is no integral over the surface; the term 4/3 pi r_a r_b r_c rho represents the mass that needs to be accelerated. Then, the total number of H2 molecules needed to power the acceleration over the entire trajectory is found in Equation 3. This is equated to the total number of H2 molecules contained within outgassing shells in Equation 6 to determine if the outgassing shells contain sufficient H2 to power the observed acceleration — integrated over the entire observed apparition. This treatment implicitly assumes that the H2 content within an entire ellipsoidal shell should ultimately be available for outgassing. At a given time we expect that outgassing will only occur from the illuminated surface; however, ‘Oumuamua was tumbling with a rotational period much shorter than the time in which its heliocentric distance changed substantially. Therefore, over time all surfaces of the body should be exposed to the Sun and subject to H2 outgassing. We have added text describing this to the Methods section, and also expanded the description of Equations 1-3 to clarify our approach.

2. In the modelling section the authors frequently refer to a "jet" of H2. They do not describe their assumptions of the morphology of this jet. In the text it clearly conflicts with the assumption of whole-surface release of H2. Is it simply emission from the sunlit hemisphere, or more localised? If the whole hemisphere, do they take into account the momentum transfer as normal to the local surface? The more localised the jet emission, the higher the rate of H2 loss required to match the observed acceleration. What they mean by the "jet" of H2 should be clearly described and quantified.
We thank the reviewer for bringing up this point and we do agree that the outflow geometry assumptions were not sufficiently described in the previous version. We have also decided not to use the terminology ‘jet’, since our model does not specify an outgassing morphology. The point remains that our model assumed that all H2 lost from the body is powering the radial acceleration. In reality, the outgassing outflow may not be fully collimated, in which case some momentum would not contribute to the radial acceleration. The details of this were discussed in Sekanina 2019, Seligman & Laughlin 2020 and Jewitt and Seligman 2022. The degree of collimation is not constrained, though in a scenario in which outgassing occurs from the sun-facing surface, the recoil will largely be anti-solar. We have added a ‘zeta’ term to Equation 1 which parametrizes the degree of collimation vs. isotropy of the outflowing gas. We test our model for zeta=1, i.e. a fully collimated outflow, and zeta=0.5, i.e. an entirely isotropic hemispherical outflow. These cases are represented in Figure 1 as separate contour lines to illustrate the range of outcomes for different outflow geometries. Although the allowable parameter space becomes more restricted for increasingly isotropic outflow morphologies, this does not significantly detract from our proposed hypothesis, because large regions of parameter space remain possible.

3. I found it peculiar that the authors did not actually give the mass loss rate of H2 required to match the observations anywhere. I suggest giving a mass loss rate at a fiducial distance of 1.25 au, where there are also observations used in Micheli et al. (2018).

We have added the H2 mass loss rate at 1.25 au to the end of the H2 budget section.

4. It seems clear that from figure 2 that for either of the thermal inertias modelled, the near surface temperature at ~1-1.5 au or more would be > 180K, allowing for H2O sublimation to also take place. Indeed they state "While higher surface temperatures are possible (Figure 2), the water matrix will begin to sublimate somewhat above 140 K". But surely there is an inconsistency here - if their own models predict higher temperatures allowing H2O sublimation, why do they ignore it? Given that they assume >50% of the H2O remains in the subsurface layers, this would have an additional effect on the non-gravitational acceleration. The authors should at least estimate this additional push from H2O sublimation, in context for the H2-induced acceleration.

We agree that this was not sufficiently addressed in the original manuscript. H2O certainly could have sublimated from the near-surface layers close to (before and after) perihelion. While we know that H2O alone could not provide sufficient acceleration due to energetic constraints (Sekanina 2019, Seligman & Laughlin 2020), spectroscopic observations did not rule out H2O outgassing. In fact, the original papers attributing the anomalous acceleration to outgassing assumed (to varying extents) H2O driven activity (Micheli et al. 2018, Seligman, Laughlin & Batygin 2019). From Seligman+2020 (Table 1), sublimation from a pure water body is insufficient to explain the non-gravitational acceleration by approximately a factor of 2 (although this number relies on unconstrained bulk properties of the object and outflow). We therefore estimate that H2O sublimation could explain at most 50% of the non-gravitational acceleration and likely even less. It is also important to note that if the surface developed an insulating regolith during interstellar radiation processing (e.g. Jewitt+2017, Fitzsimmons+2018, Seligman+2018), H2O sublimation would not be expected to contribute to the non-gravitational acceleration. We have added text to the manuscript to explain this.

5. Figure 3 appears to show that H2 release could have occurred from well before perihelion. This raises the question whether the subsurface layers would have lost a substantial amount of H2 before observation at > 1.2 au post-perihelion. The authors need to briefly show there would still be enough H2 left to produce the observed acceleration.

We thank Reviewer 1 for bringing up this very reasonable point, and we agree that it is important to address. We have added a paragraph to the Thermal Modeling methods section noting that Figure 3 shows that H2 outgassing may be possible prior to perihelion. We discuss (i) the challenges of projecting non-gravitational acceleration prior to ~1 au post-perihelion, (ii) other factors that could contribute to or prevent outgassing prior to 1 au, and (iii) that our model is qualitatively consistent with H2 outgassing pre-perihelion for parts of parameter space.

G References

These were all suitable and well chosen, although sometimes I felt some of the previous work did not need all the references given.

Abstract line 10: Reference 5 is really a dynamical paper and takes the non-detection of a dust/gas coma from other studies.

We have removed Reference 5 from this sentence.

Page 2 line 30/31: The phrase "sufficient energy received from solar irradiation to power the non-ballistic trajectory" is a little unclear. The authors should simply say H2O and CO2 sublimation could not produce the observed non-gravitational acceleration.

This sentence has been removed due to the revision of the introductory material requested by the Editor.

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The paper itself was very well written in terms of English and grammar. My only suggestion for change is

Page 2 line 30/31: The phrase "sufficient energy received from solar irradiation to power the non-ballistic trajectory" is a little unclear. The authors should simply say H_2O and CO_2 sublimation could not produce the observed non-gravitational acceleration.
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##
Response to Reviewer 2's Comments:

The manuscript presents a clear, physically motivated and, most importantly, simple model that can explain the non-gravitational acceleration detected on 1I, without recurring to exotic physical or formation processes.
The entire model is in my opinion convincing, and possibly the most reasonable explanation provided so far for the anomalous acceleration observed on this interstellar object.

I do not see any evident flaw in any of the work presented in the manuscript.
Nevertheless, I have some minor comments that I would recommend the authors consider before finalizing the manuscript for publication.

We thank Reviewer 2 for the encouraging words and the insightful report. We are glad to hear that the reviewer finds the model convincing.

- L. 31: in my mind, the word "ballistic" suggests the motion of something under a simple, often single-source, gravity field. In this case, the anomaly in the motion of 1I is present even after accounting for more complex gravitational terms (e.g. the effects of planets, asteroids, and general relativity). I would therefore recommend a word like "non-gravitational", implying that all gravitational contributions have been taken into account.
This sentence has been removed due to the revision of the introductory material requested by the Editor.

- The initial literature review (L. 1-37) is extremely well written, synthetic and complete, and covers all the key points of the topic. I might however recommend to the authors to at least briefly summarize some of the key properties of the non-gravitational acceleration they are modeling in the main paper (e.g. the radial direction and its radial dependency), since they may be unfamiliar to a reader without immediate access to the original references.
We are glad that Reviewer 2 found the literature review comprehensive. We have added a sentence describing that the best fit indicates acceleration primarily in the anti-solar direction and with a $1/r$ or $1/r^2$ radial dependency.

- L. 57-58: this paragraph properly states that the quoted dimensions are degenerate with the albedo, but since they are dramatically dependent on the assumed albedo I would suggest mentioning the value for which they were derived (especially since they are quoted with many significant digits, which are meaningless if the albedo is not constrained).
We have added the adopted albedo value for the quoted dimensions.

- The paper does not directly mention or explain that H_2 emission would not have been observationally evident in any of the datasets we have for 1I. This might be a trivial point, but I suggest at least briefly mentioning it for the sake of a non-experienced reader.
This is a good point, we have added a sentence pointing this out.

- L. 99: this statement (the water matrix becoming more crystalline towards the surface) is not immediately obvious to me. I suggest adding either a reference or a brief explanation.
Indeed, this point is not important and we have removed it from the text.

- The authors nicely explain (L. 126-130) why this process is effective and relevant on a small object like 1I, while becoming less meaningful for larger km-sized comets like the ones we routinely see in the solar system. However, is there anything in the proposed process that might explain why 1I is the only ~100 m comet we've observed so far?

We expect that, if there is indeed a size dependency on the activity level of planetesimals, then there would be a strong observational bias against discovering small comets. We have added a sentence about this to the discussion of `Oumuamua's activity

- Methods and Figure 1 caption: can the authors better explain how "T_jet" relates to the temperature of the object's ice? There is something in subsequent paragraphs (e.g. L. 173-182), but it comes after the equations and Figure 1 are introduced, and does not completely discuss the topic.
First, we note that we have changed the terminology from T_jet to T_H2 to reflect that we are not explicitly modeling the outflow geometry (partly in response to the points raised by Reviewer 1). Following this suggestion, we have added a sentence after T_H2 is introduced describing our assumptions of how it relates to the temperature of the ice surface.

- The use of the letter "A" for the geometric albedo is a bit unusual in the field of small bodies. I might recommend using "p" for it, and reserving "A" if referring to the Bond albedo.
We have updated "A" to "p"

In addition to the scientific and technical comments above, I would also like to point out a few minor editorial suggestions:

- If I remember correctly, titles of Nature papers are limited to 75 characters. The proposed title is longer, I recommend shortening it.
The title has been shortened

- I do not see a "Data Availability" statement, but I assume it's not particularly relevant for this paper, since it doesn't make use of any newly acquired dataset.
Indeed, there is no new dataset, but we have added a Code Availability statement.

- A very minor point, which I hope can be ignored with the approval of the editor, is the fact that more than 50 references are used in the main text. If I remember correctly, Nature has a stricter limit. In case this needs to be enforced for editorial reasons, there might be some multiple references for the same topic that can be removed, but I'd recommend keeping everything if the editor agrees.
Thank you for pointing this out. We have removed some references (listed below) but kept many of them, pending input from the Editor on whether the reference limit is strict.

And a few typos I noticed while reading the manuscript:

- L. 109: should read "Micheli et al." if the plural form "posit" is used.
Updated

- L. 120: "constraints... are important".
Fixed

- Figure 2-3: I recommend re-drawing the figures using the symbol "au" lowercase, to be consistent with the rest of the paper.
Fixed

- L. 186: "albedo".
Fixed

- L. 198-199: many of the spaces between numbers and units in this part of the text are incorrect.
Fixed

##

Additional changes

1. In addition to varying the outgassing temperature, water:dust mass ratio, and outflow geometry zeta, we also now consider two values for the H_2/H_2O ratio: 0.3 and 0.4. These correspond to conversion yields of 23 and 28% of the original H_2O, both of which are consistent with the constraints that are available. Because H_2/H_2O in Oumuamua is unconstrained, we felt that it was helpful to illustrate how the outcomes vary when this parameter is varied. Figure 1 has been expanded accordingly.
1. We realized that a factor 8/pi is more appropriate than 4/3 in calculating the H_2 velocity (e.g. Jackson & Desch 2021, Levine+2021), and have updated our calculations accordingly (Methods, H_2 budget). This does not meaningfully impact our discussion or conclusions.
2. We have added a brief comparison to the dust-poor comet 2P/Encke since it is likely dust-poor for a different reason than the mechanism discussed here (e.g. thermal processing in the inner solar system vs. small size)
3. We have updated the acknowledgements section

Reviewer Reports on the First Revision:

Referee #1 (Remarks to the Author):

The authors have successfully answered my questions and comments, and I am now satisfied that the new science presented in this manuscript is sound. I recommend this manscript for publication in Nature. I have a few minor comments on the revised paper listed below, but these do not affect my recommendation.

Page 6 line 104 - H_2O need not have been on the surface, sublimation from the thermal wave around perihelion could have released sub-surface ice. I suggest changing "may not have been present on the surface" to "may not have been present near the surface".

Page 7, page 126 -The possible bias in detecting small comets due to lack of dust comae would be most significant for Oort-Cloud comets, as they should suffer much more from the cosmic-ray exposure required for H_2 production. The authors may wish to specify these comets, rather than comets in general.

Page 10, line 184 - Given the sublimation temperatures reached before observations started, there would probably have been a higher $N(H_2,shell)$ before detection. The condition $N(H_2,shell)/N(H_2,accel)>1$ is only related to the number in the shell at detection. Their model is fine with this according to their Figure 1 and the last sentences on page 14, but they should note here that pre-discovery gas release might require significantly larger $N(H_2,shell)$ than $N(H_2,accel)$ i.e. not just above 1.

Page 13, line 244 - The non-gravitational acceleration was required to remove long-term trends from 76 days of astrometry, and not "constrained largely from several days of high-cadence observations". This statement should be corrected.

Page 22, Figure3. The figure x-axis implies thermal modelling at 0 au, when perihelion was at 0.25 au. Either the x-axis labelling should be fixed, or if this is not possible then a sentence added to the figure caption giving the true range of heliocentric distance modelled.

Page 14, line 253 - Do the authors mean it is *quantitatively* plausible that H_2 outgassing occurred pre-perihelion? After all, they have done the calculations to show this.

Author Rebuttals to First Revision:

Nature manuscript 2022-09-14667B: Resubmission V2
Authors: Jennifer Bergner, Darryl Z. Seligman

##

Response to Reviewer 1's Comments:

The authors have successfully answered my questions and comments, and I am now satisfied that the new science presented in this manuscript is sound. I recommend this manscript for publication in Nature. I have a few minor comments on the revised paper listed below, but these do not affect my recommendation.
We thank Reviewer 1 for the careful review and the additional helpful comments.

Page 6 line 104 - H2O need not have been on the surface, sublimation from the thermal wave around perihelion could have released sub-surface ice. I suggest changing "may not have been present on the surface" to "may not have been present near the surface".
We have updated this sentence as suggested.

Page 7, page 126 -The possible bias in detecting small comets due to lack of dust comae would be most significant for Oort-Cloud comets, as they should suffer much more from the cosmic-ray exposure required for H2 production. The authors may wish to specify these comets, rather than comets in general.
In fact, the observational bias would be due to the inability of small objects to retain surface dust due to their size, and not related to H2 production. Therefore, this particular feature should not be specific to Oort-Cloud comets.

Page 10, line 184 - Given the sublimation temperatures reached before observations started, there would probably have been a higher N(H2,shell) before detection. The condition N(H2,shell)/N(H2,accel)>1 is only related to the number in the shell at detection. Their model is fine with this according to their Figure 1 and the last sentences on page 14, but they should note here that pre-discovery gas release might require significantly larger N(H2,shell) than N(H2,accel) i.e. not just above 1.
This is a good point, we now specify that this condition is related to the model feasibility starting around the time of detection (~1 au post-perihelion).

Page 13, line 244 - The non-gravitational acceleration was required to remove long-term trends from 76 days of astrometry, and not "constrained largely from several days of high-cadence observations". This statement should be corrected.
Thank you for the correction, we have fixed this statement.

Page 22, Figure3. The figure x-axis implies thermal modelling at 0 au, when perihelion was at 0.25 au. Either the x-axis labelling should be fixed, or if this is not possible then a sentence added to the figure caption giving the true range of heliocentric distance modelled.
Thank you for catching this– there was a small bug in the time-distance interpolation for our plotting script. We have fixed this and now the perihelion is properly shown at 0.25 au.

Page 14, line 253 - Do the authors mean it is *quantitatively* plausible that H2 outgassing occurred pre-perihelion? After all, they have done the calculations to show this.
Indeed, we have removed 'qualitatively'.

##
Additional changes:

- Upon addressing the final comments from Reviewer 1, we realized that in calculating the feasibility criterion (Figure 1), when the albedo and the outgassing depth are both high, the depth can be larger than the length of the smallest semi axis of the body. These regions of parameter space are unphysical since the outgassing depth cannot be higher than any of the ellipsoid's dimensions. In Figure 1, these regions of parameter space that are not physically possible are now shown in white (i.e. without colormap). As can be seen in the new version of Figure 1, this does not impact our results or conclusions: the model is still feasible for a wide range of parameter combinations in both optimistic and conservative cases. The zeta=0.5 case for panel b is now (marginally) infeasible, but the majority of situations are not affected.

- We realized that a 'zeta' term was missing from Equation 6; it has been added.

- In the H2 budget Methods section we previously quoted the distance range for non-gravitational acceleration as '~1-3 au'. To be more precise, we have replaced this with '1.2-2.8 au'.

- We updated the caption of Figure 3 to conform with the formatting guidelines.