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Peer Review File

Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus

Corresponding Author: Mr Flynn Ames

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Ames,

Your manuscript titled "Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In the revised manuscript, please:

- Make sure to discuss the role of salinity in stratification, eventually presenting your results in terms of density.

- Make sure the space of parameters is fully covered or add a caveat discussing the limitations and implications of the approach.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

Important: The response to reviewers must not include any figures, tables or graphs. If you wish to respond to the reviewer reports with additional data in one of these formats, please add them to the main article or Supplementary Information, and refer to them in the rebuttal. Due to current technical limitations, any figures, tables, or graphs embedded in your rebuttal will not be included in the peer review file, if published.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joao Duarte, PhD
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Joe Aslin
Deputy Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus" by Ames et al.

The authors set out to investigate the interaction between buoyant plumes and stratification in the ocean on Enceladus. This is done using theoretical arguments and a 2-D y-z model. So far, so good. However, I'm afraid I have some concerns about how the methods, and these must be addressed before this can be published. I also feel it is a bit out of scope for this journal. Consequently, I have to recommend a rejection from Nature Comms EE.

Main concerns
I have two concerns. The first is the use of \alpha-space to describe stratification. That may work in lakes, but in an ocean, even one with a salinity, S, below 20, you can still have stratification due to salinity that can compensate for the stratification due to temperature, T. This happens in several locations in the world's oceans, e.g., the Baltic, and to some extent in the Arctic. The role of salinity is not discussed much at all, and with an ice cover and vertical heat fluxes, you will get a stratification due to salinity as well. Can the plumes be stopped by a freshwater lid under the ice?
For the work to really illustrate what is happening on Enceladus I think the results need to be presented in density rather than \alpha space. Then the introduction can be cut back quite hard, and figure 1 can be referenced much earlier in the text, because we know how T and S (and P, although the effects of compressibility here is of course small) control density. I also wonder if insight into the plume dynamics itself could be gained from, e.g., the work by J. S. Tuner (his book Buoyancy effects in Fluids has some quite applicable parts).

Then there is the concern of the plume dynamics. It very much looks like the plumes are implicitly simulated by an enhanced diffusivity, i.e., vertical mixing. In principle, and for simplicity, I agree with the constant diffusivity approach for Enceladus, but I wonder if the parameter space may be much larger than the one covered here. On Earth, the globally averaged diffusivity is ~10^-4 m^2 s^-1, but it varies over several orders of magnitude and is much lower in the abyssal ocean away from topography. If Enceladus has a smooth topography, the diffusivity in its interior will be of similarly low values, because there is no mechanism for driving the mixing.

Minor concerns
How large are the errors in density introduced by the linear equation of state and how does your constant values compare to the same parameters on Earth?

L275: Why is R the moon's radius? The ocean depth would have made much more sense since that's the length scale you are transporting over.

Reviewer #2 (Remarks to the Author):

The authors designed a nice study that I recommend for publication once my comments below are addressed. The work applies theoretical models and numerical simulations to study the effect of inverse stratification on particulate transport to the plumes of Enceladus. They find that regardless of the mean salinity of the ocean, there is inverse stratification, caused by temperature or salinity stratification, which impedes transport to the south polar ice-interface. The authors use a more comprehensive analytical framework and explore a wider parameter regime compared to previous studies. The manuscript is also clearly presented and articulated.

Major points:
1. Given a constant energy for mixing, the vertical turbulent diffusivity κ_z is a function of stratification, which in turn is a function of the depth of the inverse layer H_strat (see Eq. 2 & 3 in Zeng & Jansen 2021). It would be more useful to derive H_strat as a function of mixing energy instead of κ_z in the analytical framework.

2. Besides modulating the freezing point T_f, which is included in this work, the ice topography has at least two more major effects: (1) if freezing and melting occur at different vertical levels, there will be an energy source/sink for the ocean circulation (See Jansen et al. 2023); (2) the slope of the isopycnals will be modulated by the ice shell thickness since both the level of T_f and salinity source/sink follows the ice shell topography. In the numerical simulations presented in the manuscript, the ice shell is assumed to be flat. How these two points could affect the results with a more realistic ice shell topography should be addressed in the manuscript.

Minor points:
1. Line 62: by an ice shell –> by conductive or convective heat loss through the ice shell.
2. Eq. 4: by how much could the vertical variation in density and gravity affect the pressure calculation?
3. Fig. 2: It would be useful to show the borderline where the inverse stratification extends to the full ocean depth in the bottom left panel. Below the borderline, the contour lines would be horizontal because H_strat is fixed and the age doesn't depend on salinity.
4. The convective timescale is estimated to be 50-500 yrs according to Zeng & Jansen 2021. How does it compare with the numerical simulations and is it a borderline when the diffusive scaling begins to underestimate the ideal age?
5. It would be useful to compare the 2D results to other 3D simulations (e.g. Kang 2022; Zeng & Jansen 2024) to see how well the GM scheme is in parameterizing the heat and tracer transport. Is the meridional heat transport in agreement with the scaling laws derived in Kang & Jansen 2022 and Kang 2022 ("Different ice-shell geometries on Europa and Enceladus due to their different sizes: Impacts of ocean heat transport." The Astrophysical Journal 934, no. 2 (2022): 116.)?
6. Fig. 3: it would be better to add plots that show the potential density anomalies, as the isopycnal slope affects the GMREDI parameterization.
7. Line 291: top-to-bottom -> bottom-to-top.
8. Line 315-317: according to the observed amount of H2 in the plume and the solubility of H2 in the Enceladus' ocean, H2 bubbles are likely to exist only in the upper part of the ocean.
9. Some typos: Line 343: The -> The theory. Line 351: simulation -> simulations.

Reviewer #3 (Remarks to the Author):

Please see attached PDF.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Ames,

Your manuscript titled "Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Joao Duarte, PhD
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Joe Aslin
Deputy Editor,
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have clarified my misunderstandings and revised the manuscript accordingly. I recommend it is published in its current state.

Reviewer #2 (Remarks to the Author):

Dear Editor,

The authors have done a commendable job in responding to my review. I recommend publication after addressing the minor comments below:

1. Lines 288-293: The impact of ice topography on the lateral density gradient depends on the mean salinity of the ocean, as the density structure varies between temperature-dominated and salinity-dominated regimes in these simulations (as shown in Fig. 3). The authors should provide a clearer discussion on this point.

2. Regarding H_2 bubbles: I do not have a specific published paper in mind. If the authors wish to explore this topic further, they could calculate the solubility of H_2 in water as a function of pressure and compare it with observations.

Best regards,
Yaoxuan

Reviewer #3 (Remarks to the Author):

I have completed my review of the revised version of "Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus" by Ames et al. All of my comments have been addressed and the current version of the manuscript is in excellent shape. It is ready for publication.

Thank you,
Ashley Schoenfeld

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Review of "Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus" by Ames et al.

Summary

The study by Ames et al. seeks to explore the sensitivity of ocean stratification within Enceladus to vertical diffusion and to thermal expansivity (which is related to salinity and heat flux). The study finds that for a steady state assumption for the ice shell (i.e., melting at the poles), there is no parameter choice permitting an unstratified ocean under the plumes. Consequently, the timescale of potential hydrothermal product to reach the plumes is, at minimum, on the order of hundreds of years.

Overall, I find this paper to be very well written, with clear and concise communication of methods and results. I find the implications of the study to be very interesting and important. In particular, the results of fresh-water flux at the poles potentially inhibiting core to ice-shell communication is notable; I hope that this study will inspire more exploration to that end, as well as inspire the community to make careful considerations when designing future Enceladus missions.

I only really have a few minor notes: some typos, suggested rewordings for some clunky sentences, and a few comments. I list them all in order below.

Thank you.

Line 16 (abstract): After reading the paper, this sentence isn't entirely true, since core to shell connectivity is possible, just at the equator. So, it might be worth including that caveat here.

Line 29: Break up sentence → "... in our solar system. Among them Enceladus..."

Line 59: remove "much"

Line 73: change "should" to "could"

Line 93: I think it's worth spending an extra sentence to explain where this range for κ_z comes from, since it is such a loose constraint.

Line 98: add "believed to be sourced..."

Line 113: delete "on"

Line 132: Is there meant to be a period at the end of equation 2?

Line 157: remove comma after “bottom” and place a comma after “plumes”

Line 158: End the sentence after the paratheses.

Line 173: I suggest taking the section of the sentence in the paratheses and making it its own sentence. Additionally, the phrasing “… does not change the results discussed below qualitatively” is worded awkwardly. Perhaps “as qualitatively discussed below” is better phrasing. And lastly, I suggest being more specific with where this discussion takes place; I don’t know where the follow up to this thought is actually located.

Line 178: add a “and” after “magnitude”; change “dependant” to “depends”

Line 181: This paragraph can be joined with the previous paragraph

Line 194: remove comma after “κ_z"

Line 201: I suggest rewording this sentence. You can try this: “For large κ_z, after the inverse layer has saturated at ocean depth, further increasing κ_z results in the inverse layer actually being able to transport more efficiently, decreasing tracer age.”

Line 271: The seems like an interesting result that should be discussed further. Even with the barrier presented by freshwater fluxes, there is still a direct connectivity between the core and the ice shell, just not below the poles. Might be worth some discussion on how horizontal flow may be important in getting core materials to the plumes, or any first order approximation of these timescales.

Line 273: remove comma after “scaling”

Line 275: I think it might be worth explaining a bit more where this scaling comes from, as it is not necessarily intuitive until you see the geometrical set up presented in reference 54

Line 319: Consider rewording the sentence. Here’s a suggestion: “If the source of the observed nano-particles are not from Enceladus’ core, Cassini observations are not necessarily inconsistent with ocean stratification below the south pole.”

Line 323: remove “a” and replace with “that”, then remove “that” before “maintains”; “…implies that melting at the poles maintains…”

Line 338: Similar to my previous comment, but top-to-bottom is seemingly possible, give a higher salinity.

Line 343: Typo, switch “The” with “This”

Line 354: I think it should be “Enceladus’ ocean”

Line 359: remove period after “geometry”

Line 385: remove extra “of” after “plausible”

Line 393: remove extra “the” before “explicit”

Line 431: comma after “Enceladus”

Line 505: “for examples” → “for example”

Response to reviewers

Black - Reviewer comments

Blue - Our response

Red - Changes made to the text

We would like to thank the reviewers for taking the time to review our work and provide constructive feedback. Please see below for our responses.

Upon review, we discovered that a term within our ice flow rate formulation, extracted from Kang and Flierl (2020), was incorrect. We believe the term should be $\ln(T_i/T_m)$ rather than $\ln(T_i)$ as typed in the referenced manuscript. As a result, the magnitude of freezing and melting used in our previous submission was ~ factor 5 stronger than would be expected for the melting viscosity η_{melt} we use (i.e., corresponding to 5 times larger η_{melt} than we use, albeit still within the range of uncertainty for Enceladus - Cadek et al., 2019). For consistency, all simulations have been re-equilibrated with the adjusted freezing rate. This has very little quantitative impact upon numerical solutions for the tracer age and inversion depth. It has no impact upon the conclusions presented in the main text. To illustrate this, we have performed sensitivity tests to a 5 times larger η_{melt} (i.e., freezing rate magnitude similar to our previous submission), with corresponding numerical solutions provided in the supplementary material (Fig. S4).

Reviewer 1

Review of “Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus” by Ames et al.

The authors set out to investigate the interaction between buoyant plumes and stratification in the ocean on Enceladus. This is done using theoretical arguments and a 2-D y-z model. So far, so good. However, I’m afraid I have some concerns about how the methods, and these must be addressed before this can be published. I also feel it is a bit out of scope for this journal. Consequently, I have to recommend a rejection from Nature Comms EE.

The “Aims and Scope“ section of the Nature Communications Earth & Environment website (https://www.nature.com/commsenv/aims) states Nature Comms EE is:

“An open-access journal publishing high quality, editorially selected and peer reviewed advances in the Earth, environmental and planetary sciences”.

We would argue that our work falls under the umbrella of Planetary Science and thus under the remit of Nature Comms EE.

Furthermore, relevant (and important) work has been published in this journal recently that also investigates the transit of hydrothermally-derived material to Enceladus’ south polar plumes - see Schoenfield et al., (2023).

Schoenfield et al., consider the conditions required to entrain particulates within Enceladus’ ocean, and estimate bottom-to-top transit timescales within Enceladus’ ocean using scaling laws. However, they start from the assumption that Enceladus’ ocean is convecting from bottom to top (i.e., fully unstratified). While we do not have quarrel with their conclusion starting from this assumption of an unstratified ocean, we show in our manuscript that an unstratified ocean is not guaranteed, and that their conclusion of short bottom-to-top transit timescales (month to year) is potentially a very large underestimate. Accounting for the effect of salinity and the non-linearity of the equation of state, we find that the bottom-to-top transit timescales could be larger by 3-6 orders of magnitude.

In addition, our results suggest that in the absence of alternative particulate mechanisms, transit timescales of several months - inferred from observations (see Hsu et al., 2015) - requires an unsteady ice shell to be achievable, providing an additional perspective not considered in Schoenfield et al.

Schoenfield et al. is a very interesting paper which has the potential to strongly influence the community. We believe it is crucial to place their results in the wider context of all plausible stratifications, and publishing in the same journal as them is an ideal place to do this.

Main concerns

I have two concerns. The first is the use of \alpha-space to describe stratification. That may work in lakes, but in an ocean, even one with a salinity, S, below 20, you can still have stratification due to salinity that can compensate for the stratification due to temperature, T. This happens in several locations in the world's oceans, e.g., the Baltic, and to some extent in the Arctic. The role of salinity is not discussed much at all, and with an ice cover and vertical heat fluxes, you will get a stratification due to salinity as well. Can the plumes be stopped by a freshwater lid under the ice?

For the work to really illustrate what is happening on Enceladus I think the results need to be presented in density rather than \alpha space. Then the introduction can be cut back quite hard, and figure 1 can be referenced much earlier in the text, because we know how T and S (and P, although the effects of compressibility here is of course small) control density. I also wonder if insight into the plume dynamics itself could be gained from, e.g., the work by J. S. Tuner (his book Buoyancy effects in Fluids has some quite applicable parts).

We were surprised by the comments of the reviewer as the main goal of our paper is to explore the modulation of stratification by ocean salinity effects. At no point, do we not consider the effect of salinity on ocean density and hence stratification. A range of ocean mean salinity from 5 to 22.5 g/kg is explored in both our analytical and numerical models. (see for example Fig. 2). In our numerical model, we account for melting and freezing at the ice interface that induces salinity anomalies. We prescribe a melting and freezing, required to balance the ice flow induced by a poleward-thinning Enceladus ice shell (see Extended Methods section). Salinity is a prognostic variable in our numerical simulations that varies in space and time. Density is computed within the model at each grid point as a function of temperature, salinity and pressure using a non-linear equation of state. To be fully clear, the density in the model is computed from the local salinity, not the mean salinity.

Example numerical solutions for salinity anomalies (about the mean salinity) are plotted in Fig. 3 (left panel). Indeed, it can be seen how salinity anomalies ensure stable stratification (Fig. 3, middle right panel) underneath the south polar ice shell across all mean salinities tested. This forms a core point of the manuscript (that a freshwater lid under the south polar ice shell should impede particulate transport to Enceladus' plumes if Enceladus' ice shell is assumed in steady state) and we emphasise this in our conclusions (section 4), e.g., in the following excerpt at line 329:

"At high salinities, enough to prevent α_T becoming negative, polar freshwater melt - required under the assumption of steady state - ensures the upper ocean remains stably stratified by freshwater lenses overlying a well-mixed ocean "

Thus, our work not only accounts for ice-shell freshwater exchange, but is in fact a cautionary tale where we show that the neglect of freshwater fluxes can result in significantly different solutions from an analytical model that neglects them (e.g., see the divergence of numerical and analytical solutions for the transport timescale in the lower right panel of Fig. 2, which is discussed in the results section)

It is difficult to see where such a misunderstanding could have emerged (reviewers 2 and 3 did not run into this issue). We reread our manuscript carefully with this concern in mind. The main place in the text we identified as the possible source of this misunderstanding is the starting point of the derivation of the analytical model, Eq. 1. This equation does not show salinity explicitly, it is a heat budget which has to hold regardless of whether salinity effects are included (both heat and salt conservation are satisfied independently, their combination through the use of nonlinear α_T/β_S ensures that the density conservation is also automatically satisfied).

We identified another possible source of the misunderstanding as the schematics in Fig. 1, which illustrates the mechanism of inverse stratification at low salinity, but do not illustrate possible effects of melting and freezing. This is done partly because our analytical model focuses upon the former mechanism for stratification, but also because this mechanism is more complex and so deserves a schematic of its own (as well as a description in the introduction). We're not using α_T space to describe the stratification here, but it is illustrated to highlight how the same boundary forcings can either stratify or destabilise the ocean depending upon whether α_T is positive or negative. We have adjusted the caption in Fig. 1 to provide additional emphasis upon density, to hopefully clear up any misunderstanding that this was the case:

"Where α_T <0, cooling from the overlying ice makes water more buoyant, while warming from the underlying core makes water denser. These effects reverse where α_T >0, and the resultant presence of buoyant water underlying denser water induces convective overturning (indicated with solid black arrows)"

The stratification metric we employ in our numerical solutions is the buoyancy frequency N^2 (see middle right panel, Fig. 3). This is a function of the vertical density gradient and is a common metric for stratification in Earth ocean modelling (e.g., Li et al., 2020). We have now also overlain contours of the potential density anomaly (about the reference density) over plots of the buoyancy frequency, to further emphasise the role of density. We have also tweaked the wording in places where we mention our usage of the non-linear equation of state to add additional emphasis upon the fact that it computes water density.

Then there is the concern of the plume dynamics. It very much looks like the plumes are implicitly simulated by an enhanced diffusivity, i.e., vertical mixing. In principle, and for simplicity, I agree with the constant diffusivity approach for Enceladus, but I wonder if the parameter space may be much larger than the one covered here. On Earth, the globally averaged diffusivity is ~10^-4 m^2 s^-1, but it varies over several orders of magnitude and is much lower in the abyssal ocean away from topography. If Enceladus has a smooth topography, the diffusivity in its interior will be of similarly low values, because there is no mechanism for driving the mixing.

We realise that our presentation of the mixing in the model could have been clearer. There are two separate contributions to the vertical mixing.

We indeed parameterise convection in numerical simulations by enhancing the vertical diffusivity in regions to 1 m^2/s only where and when the ocean is statically unstable. This is common practice in Earth ocean modelling (Klinger et al., 1996). The energy for this mixing is provided by the potential energy stored in statically unstable stratification.

There is also a background vertical diffusivity κ_z used to represent unresolved vertical mixing induced by tides (e.g., Hay et al., 2019) and ice shell libration (Rekier et al., 2019; Wilson et al., 2018). This value is prescribed by us, and we test the sensitivity of our solutions to this prescribed value.

The two contributions represent two different mechanisms. While they are mathematically analogous they are in fact on very different footings, as for example the enhanced vertical diffusivity for convection is part of the numerical solution. Perhaps more importantly, these two contributions are almost separated spatially. In the inverse stratified layer, the convective adjustment by enhanced mixing is rarely, if ever, triggered, and vertical diffusion is dominated by the background vertical diffusivity κ_z (as assumed in our theoretical model). Where there is regular convection in the lower layers, the effect of background vertical diffusivity κ_z is negligible.

In our manuscript, κ_z exclusively refers to the prescribed background vertical diffusivity representing tides and libration. In our analytical model, we explore κ_z spanning five orders of magnitude, from the molecular diffusivity (for temperature) $O(10^{-7})$ m^2/s, up to $O(10^{-3})$ m^2/s. The molecular diffusivity is the lowest plausible value of κ_z within Enceladus (assuming tides and libration are unable to make a significant contribution to vertical mixing). Our upper bound is taken from an estimate by Zeng and Jansen (2021) who, in turn, obtain this using the existing upper bound estimate for librationally-induced energy dissipation within Enceladus' ocean (Wilson and Kerswell, 2018). In our numerical model, we simulate three orders of magnitude of κ_z: 10^{-3}, 10^{-4}, and 10^{-5} m^2/s. Note it is computationally prohibitive to simulate lower magnitudes of κ_z because the vertical resolution of the model required to resolve stratified layers at the ocean top would be very fine (O(10m) for a 40km deep ocean).

We emphasise that, in contrast to our work, previous work had only performed simulations with one value of κ_z, across either one or two values of ocean mean salinity.

To clarify the above aspects, we have made a few changes to the text. Firstly, while we mention the enhanced convective adjustment in the extended methods section, we appreciate that this is worth mentioning in the main text. We have added the following statement at line 232:

“Unresolved mixing by convection is parameterised via the diffusive adjustment scheme of [56]”

Secondly, we have tweaked the following excerpt in the introduction (line 95) in case this was a source of confusion:

“κ_z - an effective vertical diffusivity, used to parameterise vertical mixing induced by tides and libration”

Finally, it is indeed possible that κ_z varies spatially within Enceladus' ocean (although we'd caution saying there is no mechanism for driving mixing in the ocean interior - see, for example, Rekier et al., (2019) and discussion therein regarding the possible role of internal shear layers within Enceladus' ocean). We have now added the following excerpt at line 127 where we mention the possibility for spatial variation in κ_z:

“As on Earth, κ_z may exhibit spatial and temporal variations within Enceladus due to stratification, topography, and profiles of tidal and librational energy dissipation, which may be concentrated within boundary layers at the ocean top and bottom [52]. Given existing uncertainties, κ_z here is assumed constant and uniform”

Minor concerns

How large are the errors in density introduced by the linear equation of state and how does your constant values compare to the same parameters on Earth?

Again, we do not assume a linear equation of state in the numerical model nor in the analytical solution. We looked for the possible source for this confusion, possibly the linear approximation for critical temperature in analytical theory is the reason here. This linear approximation for the critical temperature does not imply a linear equation of state. The very fact that we account for a critical temperature is in contradiction with a linear equation of state. In a linear equation of state, α_T and β_S are constant and do not change sign, i.e. there is no critical temperature.

We have included the following excerpt in the introduction (line 87) to further emphasise that an inversion depth (and thus a critical temperature) relies on the existence of non-linearity in the equation of state and that therefore, resolving the inversion depth requires the use of a non-linear equation of state:

"Only a handful of simulations have been conducted at salinity < 20 g/kg, computing water density using a non-linear equation of state that permits a varying α_T (thus permitting an inversion depth)"

L275: Why is R the moon's radius? The ocean depth would have made much more sense since that's the length scale you are transporting over.

R is the horizontal length scale of salinity contours in the ocean. In other words, the horizontal scale over which a region of ice melting is taking place. This in turn influences the penetration depth of such anomalies into the ocean interior (H_{lens}).

The slope S of salinity contours can be approximated by their vertical scale (penetration depth H_{lens}) divided by the horizontal scale R - i.e., $S \sim H_{lens}/R$ (This is Eq. 16 from Zhang et al., 2024).

Zhang et al., (2024) use this scaling for the contours to derive what is Eq. 7 in our manuscript, which expresses the penetration depth H_{lens} as a function of processes that flatten the salinity contours (κ_{GM}) and processes that steepen them (κ_z)

The continuous regions of melting in our simulations take up about a third of the domain (see Fig. S1). A third of the length of the domain is ~60 degrees latitude, which is ~ 242 km at Enceladus' ice interface (assumed 20 km beneath the surface here). Given this length scale R (and Eq. 7) is an approximation, we use the moon's radius of 252 km for simplicity as in Zhang et al.,

We appreciate that this was probably not clear enough in our initial submission. We have therefore added more description at line 297 to rectify this:

"where κ_{GM} is the eddy diffusivity and R a horizontal length scale defining the horizontal scale of salinity contours within the freshwater lens (which closely follows a region of melting - see Fig. 3 and Fig. S1), taken here to equal the moon's radius (~ ⅓ the length of the simulation domain)."

Reviewer 2

The authors designed a nice study that I recommend for publication once my comments below are addressed. The work applies theoretical models and numerical simulations to study the effect of inverse stratification on particulate transport to the plumes of Enceladus. They find that regardless of the mean salinity of the ocean, there is inverse stratification, caused by temperature or salinity stratification, which impedes transport to the south polar ice-interface. The authors use a more comprehensive analytical framework and explore a wider parameter regime compared to previous studies. The manuscript is also clearly presented and articulated.

We thank the reviewer for their constructive feedback and comments which we believe has helped us improve the manuscript.

Please see below for responses to individual comments

Major points:

1. Given a constant energy for mixing, the vertical turbulent diffusivity κ_z is a function of stratification, which in turn is a function of the depth of the inverse layer H_strat (see Eq. 2 & 3 in Zeng & Jansen 2021). It would be more useful to derive H_strat as a function of mixing energy instead of κ_z in the analytical framework.

We see the reviewers point here and agree that deriving the analytical framework as a function of the energy available for mixing would be more useful, given this is a dominant constraint upon κ_z. We have attempted this (using the equations from Zeng and Jansen mentioned). Currently, our problem is the treatment of the area parameter A, which varies as a function of the radial distance from the centre of the moon. The advantage of our existing theory is that it is formulated in terms of pressure. This means the ice and ocean contributions can be isolated (to a good approximation) which allows us to obtain a solution for the inversion depth. When trying to replicate this approach, if we approximate A as the surface area at H_{strat} (so 4*pi*radius_at_H_{strat}^2), letting the radial distance at H_{strat} = $r_{surface}$ - H_{ice} - H_{strat}, we end up with a quartic equation for H_{strat} (if we approximate the thermal expansion coefficient as a linear function of temperature, salinity and pressure, analogous to our current approach for the critical temperature). We're yet to find a way to solve this quartic equation analytically.

The additional caveat is that (we'd argue) to robustly test such an analytical model, the energy available for mixing should be inserted as a forcing into the numerical simulations, rather than a κ_z. This would be especially important given that the model stratification - which also controls κ_z - can vary significantly in magnitude between simulations. While we have tested implementation of the Osborn relation (Eq. 2 from Zeng and Jansen 2021) into the MITgcm, we think the question of how energy available for mixing governs the magnitude and spatial distribution of κ_z (which we believe is important) requires dedicated work that is beyond the scope of our manuscript here. The current analytical framework suffices for the goals of this manuscript, to demonstrate that Enceladus' ocean is likely stratified nearer the ice interface and that improved constraints upon ocean salinity, induced mixing, and the ice shell state could reduce uncertainty in how such stratification may influence particulate transport to the plumes.

2. Besides modulating the freezing point T_f, which is included in this work, the ice topography has at least two more major effects: (1) if freezing and melting occur at different vertical levels, there will be an energy source/sink for the ocean circulation (See Jansen et al. 2023); (2) the slope of the isopycnals will be modulated by the ice shell thickness since both the level of T_f and salinity source/sink follows the ice shell topography. In the numerical simulations presented in the manuscript, the ice shell is assumed to be flat. How these two points could affect the results with a more realistic ice shell topography should be addressed in the manuscript.

We agree that this is worth mentioning in the manuscript and have added in the following discussion at line 288:

"Also note the stratifying influence of eddies is likely modulated by the shape of the overlying ice shell - an effect not considered here. Because Enceladus' ice shell is expected to extend deeper into the ocean at the equator than at the poles, melting and freezing should occur at different depths. In this case, the lateral density gradient at the equator, and thus the magnitude of equatorial eddy restratification, would likely be weaker than obtained in our numerical solutions. Numerical solutions for the equatorial age should be interpreted with this in mind."

Minor points:

1. Line 62: by an ice shell –> by conductive or convective heat loss through the ice shell.

This sentence now reads:

"The assumption is made on the basis that Enceladus' ocean is cooled from above and heated from below, by an ice shell *(via conductive or convective heat loss)* and a tidally-heated rocky core, respectively"

2. Eq. 4: by how much could the vertical variation in density and gravity affect the pressure calculation?

Using parameters from our numerical simulations (geometry and gravity profile), neglecting the vertical variation of gravity within the ocean and vertical variations in ocean density (namely the compressibility effect), results in an error of ~ 3 % at the ocean bottom (decreasing as one nears the ocean-ice interface).

3. Fig. 2: It would be useful to show the borderline where the inverse stratification extends to the full ocean depth in the bottom left panel. Below the borderline, the contour lines would be horizontal because H_strat is fixed and the age doesn't depend on salinity.

We agree and have added a dotted line in the bottom left panel of Fig. 2 to represent this borderline as suggested and made reference to this borderline in the figure caption and text.

4. The convective timescale is estimated to be 50-500 yrs according to Zeng & Jansen 2021. How does it compare with the numerical simulations and is it a borderline when the diffusive scaling begins to underestimate the ideal age?

We find in our simulations that the tracer can be transported through a convecting ocean region in ~ 10-50 years in our simulations, which is on the faster end (or slightly faster) than that suggested by Zeng and Jansen. We believe our simulations may not be suitable for gaining insights on this scaling because convection is parameterised here (out of necessity) rather than resolved explicitly. Such convection schemes, while (we'd argue are) suitable for our goals here (of exploring the influence of stratified ocean regions upon particulate transport), have not yet been benchmarked against convection-resolving simulations within Enceladus. We believe such work is required before insights into convecting regions of Enceladus' ocean can be reliably extracted from simulations that parameterise them.

Importantly here, 10-50 years or 50-500 years for the convective timescale does not impact our conclusions as age increases considerably over short vertical distance once a stratified region of ocean is reached.

5. It would be useful to compare the 2D results to other 3D simulations (e.g. Kang 2022; Zeng & Jansen 2024) to see how well the GM scheme is in parameterizing the heat and tracer transport. Is the meridional heat transport in agreement with the scaling laws derived in Kang & Jansen 2022 and Kang 2022 ("Different ice-shell geometries on Europa and Enceladus due to their different sizes: Impacts of ocean heat transport." The Astrophysical Journal 934, no. 2 (2022): 116.)?

We have added plots of the vertically-integrated meridional heat transport achieved by the GMREDI scheme to the supplementary materials and made reference to this at line 420:

"For reference, numerical solutions for the vertically-integrated meridional heat transport achieved by GMREDI are plotted in Fig. S5. Heat is transported equatorward, in qualitative agreement with previous work [40, 61]. When extrapolated to the full globe, the total meridional heat transport of ~7-10 GW achieved by GMREDI is in approximate agreement with scalings by [61] at $\kappa_z = 10^{-3}$ m^2/s. Solutions at lower κ_z produce a larger heat transport than predicted by existing scalings (possibly owing to the diabatic fluxes present within convecting regions, which are more extensive at low κ_z - see Fig. S3). This is not expected to impact the conclusions presented in the main text."

Our plotted solutions are extrapolated for the globe (i.e., scaled up to account for the lack of a zonal dimension). Admittedly, we're unable to determine if other papers have done the same, which makes drawing comparisons with their numerical solutions more challenging.

If a constant α_T is assumed (the interface value at 2×10^{-5} K^{-1} - taken from a solution at 15 g/kg mean salinity), along with a meridional temperature contrast $\Delta T = 0.1$ °K and a $\kappa_z = 10^{-3}$ m^2/s, the κ_z - limit scaling from Kang (2022) (more appropriate for reference simulations at larger κ_{GM}) suggests a meridional heat transport of ~ 5 GW. Scaling up corresponding numerical solutions (those below salinity 15 g/kg at the corresponding κ_z), we get ~ 7 GW maximum meridional heat transport, which is in reasonable agreement with the scaling. Note our simulations include a salinity forcing which is not accounted for in the scaling by Kang (2022). We find meridional heat transport is larger at higher salinity, which is the behaviour we'd expect for a salinity forcing that is contributing to the eddy-induced transport.

However, we don't observe the expected drop-off in eddy heat transport with decreasing κ_z. This is likely because of the necessary (for numerical stability) use of slope clipping for the GM component of the scheme (beyond a density slope of 5), meaning that diabatic heat fluxes are still maintained within convecting regions of the ocean (although the additional use of a freshwater forcing could perhaps also account for some of this discrepancy).

Previous high resolution simulations suggest that eddies should act to homogenise the vertical heat flux within convecting regions of ocean (i.e., mix laterally; Kang et al., 2022) within Enceladus. With this in mind, we believe the discrepancies with scalings at low κ_z do not affect the robustness of the conclusions presented in the main text.

As with the above point, we believe GMREDI is suitable for the purposes of this manuscript, but simulations that are both convection and eddy resolving are required to explore how eddies interact with convection within Enceladus' interior to set up the meridional heat transport within convecting regions of ocean.

6. Fig. 3: it would be better to add plots that show the potential density anomalies, as the isopycnal slope affects the GMREDI parameterization.

We agree that visualisation of potential density anomalies could be useful here and have adjusted Fig. 3 to include contours of the potential density anomaly. Rather than adding a separate plot, these contours have been overlain over the N2 plot, which we have maintained given this is our chosen metric for ocean stratification, and clearly visualises the relationship between ocean stratification and ideal age. Note that we have also changed some of the colour maps in Fig. 3 to guarantee perceptual uniformity (this has no effect on any remarks made in the text).

7. Line 291: top-to-bottom -> bottom-to-top.

Thanks for spotting - Done.

8. Line 315-317: according to the observed amount of H2 in the plume and the solubility of H2 in the Enceladus' ocean, H2 bubbles are likely to exist only in the upper part of the ocean.

We thank the reviewer for making us aware of this - this is a very interesting point that we would like to add into the text. We're hoping to ask if the reviewer had a reference in mind for this specific point (as we have been unable to find one)? We could only find the conference presentation by Zeng et al.

9. Some typos: Line 343: The -> The theory. Line 351: simulation -> simulations.

Thanks for spotting. These typos have been fixed.

Reviewer 3

Summary

The study by Ames et al. seeks to explore the sensitivity of ocean stratification within Enceladus to vertical diffusion and to thermal expansivity (which is related to salinity and heat flux). The study finds that for a steady state assumption for the ice shell (i.e., melting at the poles), there is no parameter choice permitting an unstratified ocean under the plumes. Consequently, the timescale of potential hydrothermal product to reach the plumes is, at minimum, on the order of hundreds of years. Overall, I find this paper to be very well written, with clear and concise communication of methods and results. I find the implications of the study to be very interesting and important. In particular, the results of fresh-water flux at the poles potentially inhibiting core to ice-shell communication is notable; I hope that this study will inspire more exploration to that end, as well as inspire the community to make careful considerations when designing future Enceladus missions. I only really have a few minor notes: some typos, suggested rewordings for some clunky sentences, and a few comments. I list them all in order below. Thank you.

We thank the reviewer for their constructive feedback and comments which we believe has helped us improve the manuscript. We're pleased to hear that the reviewer's shares our hope of inspiring more exploration of the implications of stratification upon plume sample interpretation and future mission design.

Please see below for responses to individual comments.

Line 16 (abstract): After reading the paper, this sentence isn't entirely true, since core to shell connectivity is possible, just at the equator. So, it might be worth including that caveat here.

Thanks for pointing this out as well as in your later point. We've added an additional word (given the word constraints) to add a subtle emphasis on our focus on the south pole.

Although as we discuss in response to your later point, we realised upon review that our statement regarding stratification at the equator was slightly inaccurate:

"We show that, under a steady state assumption for the ice shell, which requires melting at the poles, there is no parameter choice permitting an unstratified ocean from top to bottom there"

Line 29: Break up sentence → "… in our solar system. Among them Enceladus…"

We appreciate this suggestion although prefer keeping the mention of Enceladus in the first sentence of the paragraph, so have kept the original first sentence intact for now.

Line 59: remove "much"

We appreciate this suggestion but are hesitant to remove the word 'much' given there are some studies (discussed in the introduction) that explore Enceladus' ocean dynamics and do consider ocean stratification (however this consideration has been lacking in studies aiming to *interpret* plume samples).

Line 73: change "should" to "could"

Done.

Line 93: I think it's worth spending an extra sentence to explain where this range for κz comes from, since it is such a loose constraint.

We agree and have added extra detail to this sentence to make it clearer how the range was derived, adding the references to the upper and lower bound estimates for energy dissipation Zeng and Jansen used to derive this constraint:

"By approximating κ_z as a function of the total energy available for ocean mixing, [31] derived the range of uncertainty of κ_z within Enceladus to span four orders of magnitude, from ∼ 10−7 to 10−3 m2 s−1, corresponding to existing bounds in estimates of tidal and librational energy dissipation [41, 42]"

Line 98: add "believed to be sourced…"

Done.

Line 113: delete "on"

Done.

Line 132: Is there meant to be a period at the end of equation 2?

Yes good spot - Done.

Line 157: remove comma after “bottom” and place a comma after “plumes”

Done.

Line 158: End the sentence after the parentheses.

We appreciate this suggestion although prefer to maintain the mention of ‘age’ in the first sentence so have kept the original sentence intact for now.

Line 173: I suggest taking the section of the sentence in the paratheses and making it its own sentence. Additionally, the phrasing “… does not change the results discussed below qualitatively” is worded awkwardly. Perhaps “as qualitatively discussed below” is better phrasing. And lastly, I suggest being more specific with where this discussion takes place; I don’t know where the follow up to this thought is actually located.

We actually didn’t provide further discussion regarding changing the globally integrated core heat output to either 10 or 30GW (the bounds proposed by Choblet et al., 2017), this is because doing so in the theoretical model only acts to shift contours in the left-hand panel of Fig. 2 upwards or downwards slightly, which has no significant effect upon remarks made later in the rest of the manuscript. We have amended this sentence to highlight the range proposed by Choblet et al. and removed the phrase in parenthesis for now to avoid confusion.

Line 178: add a “and” after “magnitude”; change “dependant” to “depends”

We have amended this sentence, albeit in a way that differs slightly from the suggestion:

“The predicted tracer age varies by over six orders of magnitude depending upon the chosen values of mean salinity and κ_z”

Line 181: This paragraph can be joined with the previous paragraph

Done.

Line 194: remove comma after “κz "

Done.

Line 201: I suggest rewording this sentence. You can try this: "For large κz, after the inverse layer has saturated at ocean depth, further increasing κz results in the inverse layer actually being able to transport more efficiently, decreasing tracer age."

We have made an amendment that differs slightly to the one proposed here. We have added the caveat that age can actually begin to decrease with increasing κ_z even when the inversion depth has not saturated at the ocean depth (see bottom left panel of Fig. 2). This is because at salinity > ~17 g/kg, the inversion depth can saturate above the ocean bottom, given its maximum H_{strat} (which can be approximated given the scaling derived in the previous paragraph):

"For larger κ_z, where the inverse layer saturates at either the ocean depth or its maximum H_{strat}, further increasing κ_z makes transport through the inverse layer more efficient, decreasing tracer age"

Line 271: The seems like an interesting result that should be discussed further. Even with the barrier presented by freshwater fluxes, there is still a direct connectivity between the core and the ice shell, just not below the poles. Might be worth some discussion on how horizontal flow may be important in getting core materials to the plumes, or any first order approximation of these timescales.

We thank the reviewer for this comment which made us realise that our statement here was slightly inaccurate. Efficient mixing can extend much closer to the ice-interface at the equator vs the poles. However, even at the equator, transport to the ice-interface can be prolonged by 100's of years because of the re-stratification effects of the eddy parameterisation scheme nearer the ice interface. We therefore replaced our statement here with the following at line 279:

"At the equator, saltier water (generated by ice freezing) can penetrate the inversion depth in solutions with 22.5 g/kg mean salinity. There, convection exists much closer to the ice-interface than at the poles. Despite this, age is still of order 100's of years at equatorial ocean top (see also numerical solution at lower κ_z in Fig. S3). This is because a weak stratification is maintained here against convection by the eddy parameterisation scheme (see Extended Methods). The eddy scheme is invoked because ice melting and freezing induce lateral gradients in ocean density, causing isopycnals (contours of constant density - see Fig. 3, middle right panel) to become 'sloped'. As in Earth's oceans, the parameterised eddies mimic the effect of baroclinic instability which extracts the potential energy stored in the sloped isopycnals and, in doing so, flattens them, re-stratifying the water column [see also 43, 60]."

Line 273: remove comma after "scaling"

Done.

Line 275: I think it might be worth explaining a bit more where this scaling comes from, as itis not necessarily intuitive until you see the geometrical set up presented in reference 54

We agree this was not clear in our initial submission and have now added more description at line 297:

"where kGM is the eddy diffusivity and R a horizontal length scale defining the horizontal scale of salinity contours within the freshwater lens (which closely follows a region of melting - see Fig. 3 and Fig. S1), taken here to equal the moon's radius (~ ⅓ the length of the simulation domain)."

Line 319: Consider rewording the sentence. Here's a suggestion: "If the source of the observed nano-particles are not from Enceladus' core, Cassini observations are not necessarily inconsistent with ocean stratification below the south pole."

Done.

Line 323: remove "a" and replace with "that", then remove "that" before "maintains"; "…implies that melting at the poles maintains…"

This sentence now reads:

"This assumption implies ice melting at the poles that maintains a stratification there and, as a result, large transport timescales, even in an otherwise predominantly convecting ocean"

Line 338: Similar to my previous comment, but top-to-bottom is seemingly possible, give a higher salinity.

We're presuming the reviewer meant line 328 here? If so, we have taken a similar approach to the abstract where we have added the word 'there', given this sentence does emphasise the ocean may be predominantly convecting elsewhere.

Line 343: Typo, switch "The" with "This"

Done.

Line 354: I think it should be "Enceladus' ocean"

Agreed - Done.

Line 359: remove period after "geometry"

Done.

Line 385: remove extra “of” after “plausible”

Done.

Line 393: remove extra “the” before “explicit”

Done.

Line 431: comma after “Enceladus”

Done.

Line 505: “for examples” → “for example”

Done.

References

- Cadek, O. et al. Long-term stability of Enceladus' uneven ice shell. *Icarus* 319, 476–484 (2019). https://doi.org/10.1016/j.icarus.2018.10.003
- Hay, H. C. & Matsuyama, I. Nonlinear tidal dissipation in the subsurface oceans of Enceladus and other icy satellites. *Icarus* 319, 68–85 (2019). https://doi.org/10.1016/j.icarus.2018.09.019
- Hsu, H.-W. et al. Ongoing hydrothermal activities within Enceladus. Nature 519, 207–210 (2015). https://doi.org/10.1038/nature14262
- Kang, W. & Flierl, G. Spontaneous formation of geysers at only one pole on Enceladus's ice shell. Proceedings of the National Academy of Sciences 117, 14764–14768 (2020). https://doi.org/10.1073/pnas.2001648117
- Klinger, B. A., Marshall, J. & Send, U. Representation of convective plumes by vertical adjustment. Journal of Geophysical Research: Oceans 101, 18175–18182 (1996). https://doi.org/10.1029/96JC00861
- Li, G. et al. Increasing ocean stratification over the past half-century. Nature Climate Change 10, 1116–1123 (2020). https://doi.org/10.1038/s41558-020-00918-2
- Schoenfeld, A. M. et al. Particle entrainment and rotating convection in Enceladus' ocean. Communications Earth & Environment 4, 28 (2023). https://doi.org/10.1038/s43247-023-00674-z
- Rekier, J., Trinh, A., Triana, S. A. & Dehant, V. Internal energy dissipation in Enceladus's subsurface ocean from tides and libration and the role of inertial waves. Journal of Geophysical Research: Planets 124, 2198–2212 (2019). https://doi.org/10.1029/2019JE005988
- Wilson, A. & Kerswell, R. R. Can libration maintain Enceladus's ocean? Earth and Planetary Science Letters 500, 41–46 (2018). https://doi.org/10.1016/j.epsl.2018.08.012
- Zeng, Y. & Jansen, M. F. Ocean circulation on enceladus with a high-versus low-salinity ocean. The Planetary Science Journal 2, 151 (2021). https://iopscience.iop.org/article/10.3847/PSJ/ac1114/meta
- Zhang, Y., Kang, W. & Marshall, J. Ocean weather systems on icy moons with application to Enceladus. Submitted to Science Advances. https://oceans.mit.edu/JohnMarshall/wp- content/uploads/2024/01/BI_IcyMoon_SciAdv-3.pdf (2024).23

Response to reviewers

Black - Reviewer comments

Blue - Our response

Red - Changes made to the text

Reviewer 1

The authors have clarified my misunderstandings and revised the manuscript accordingly. I recommend it is published in its current state.

Thank you!

Reviewer 2

Dear Editor,

The authors have done a commendable job in responding to my review. I recommend publication after addressing the minor comments below:

We thank the reviewer again for their constructive feedback and comments which have helped us improve the manuscript. Please see responses below.

1. Lines 288-293: The impact of ice topography on the lateral density gradient depends on the mean salinity of the ocean, as the density structure varies between temperature-dominated and salinity-dominated regimes in these simulations (as shown in Fig. 3). The authors should provide a clearer discussion on this point.

We agree that this discussion could be clearer. We have made a few modifications to the text to emphasise the reversal of the lateral density gradient at the ice-interface at different mean ocean salinities. We've also modified the discussion of topography to clarify that its effect upon the lateral density gradient depends upon whether salinity or temperature-anomalies dominate the ocean density distribution:

"Also note the stratifying influence of eddies is likely modulated by the shape of the overlying ice shell - an effect not considered here. Because Enceladus' ice shell is expected to extend deeper into the ocean at the equator than at the poles, freezing should occur lower down in the water column than melting. Where salinity anomalies are the dominant control upon ocean density (e.g., Fig.3, lower), a large κ_z would be necessary to mix the buoyant (fresher) water downwards to sustain the lateral density gradient at the equatorial ice interface [36]. Without this, equatorial eddy restratification would likely be weaker than obtained in our numerical solutions for this regime. Numerical solutions for the equatorial age should be interpreted with this in mind. "

2. Regarding H2 bubbles: I do not have a specific published paper in mind. If the authors wish to explore this topic further, they could calculate the solubility of H2 in water as a function of pressure and compare it with observations.

We thank the reviewer for this comment and guidance. We prefer to leave exploration of gas bubble transport within Enceladus to future dedicated work. This point is beyond the scope of the manuscript. In addition, it takes us far from our expertise and we are uneasy speculating about the potential role of H_2 solubility without relying on published literature. Finally, we are aware of an on-going effort to review these aspects by a group in the US (preliminary work has been mentioned in an abstract at AbSciCon 2024), which brings together researchers much more qualified than us to provide an in-depth discussion.

We mention in the manuscript that gas bubble transport has only been suggested to be viable when neglecting stratification. We believe this should suffice to promote scepticism regarding the potential for gas bubble transport to explain the discrepancies between our results and timescales inferred from silica nano-particle observations.

Reviewer 3

I have completed my review of the revised version of "Ocean Stratification Impedes Particulate Transport to the Plumes of Enceladus" by Ames et al. All of my comments have been addressed and the current version of the manuscript is in excellent shape. It is ready for publication.

Thank you!