Thank you very much for reviewing our manuscript. We prepare the final responses denoted by blue color for each individual point. This document indicates the revisions for each point with Line numbers in the revised manuscript.

Reviewer 1

- Approach is relatively novel, a mathematical model used to represent this process is a good idea. In my view, though the model is nifty and mathematically neat (except some small areas where further details/assumptions should be stated to ensure applicability), there is essentially no field data that adequately corroborates the result. The lake used to 'validate' the model fits reasonably well along one depth problem, but the entire bathymetry of the lake is not represented, and it is stated that this lake may actually be composed of two different thaw features. It would be of great added value to compare the model to multiple lakes in an area where wind and uneven segregated ground ice distribution are not a large factor to see whether the lakes formed do indeed follow the model. Also to predict time series thaw in lakes as compared to model predictions. As it is, the comparison to data concerns me as it looks like validation, but the model would be more 'trustworthy' if stated in theoretical terms as opposed to in direct application.

Thank you for your positive overall evaluation. Our point-by-point response will be presented below.

- For the most part, though some of the derivation needs a bit of clarification; likely the results hold, but as a reader I struggled to follow some of the steps. Sometimes the assumptions previously stated should be reiterated (especially in the discussion) to ensure the readers understand the limitations of this model.

We included the complete assumptions for the theoretical equation in addition to the quasi-steady state approximation (Line 152-155).

> The material of permafrost and talik is assumed to be fully saturated with ice and water, respectively. Also, the thermal constants (thermal conductivity, latent heat, and thawing temperature) are constant and isotropic, and the change in volume of water on thawing and freezing is negligible. Under such assumptions…

- Are the results sufficient to support the interpretations and conclusions? Not really… as noted above, the model derivation is nice, but comparing the model results to one measured lake is a little worrying.

There are only a very limited number of talik depth measurements under an isolated lake in a continuous permafrost – many of these examples are single drill points. The example at Peatball lake is, to our knowledge, the only quasi-3D dataset available in the Arctic. We strongly believe that the TEM sounding survey of the Peatball Lake is the most comprehensive dataset, and therefore most appropriate for this comparison. We will indicate the data availability in the revised manuscript.

We added, "The dataset at Peatball lake is, to our knowledge, the only quasi-3D talik depths available under an isolated lake in a continuous permafrost in the Arctic because others are mostly sporadic talik depth measurements at single drill points", around Line 314-317.

- As noted above, some detail in derivations is missing. The manuscript is also quite long, and so I was unable to review the supporting information as I would like and cannot comment on the quality of the derivations therein.

We tried to improve the mathematical derivation with brief background information and better variable names (Line 152-211).

- The authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, though the introduction may need a few additional references for some 'obvious' concepts which are clearly not the author's own ideas, but generally accepted in the field.

Thank you. We increased number of the references in the introduction. For example:

Jeffries, M.O., Morris, K. and Liston, G.E.: A method to determine lake depth and water availability on the North Slope of Alaska with spaceborne imaging radar and numerical ice growth modelling. Arctic, pp.367-374, 1996.

Jones, B.M., Grosse, G., Farquharson, L.M, Roy-Léveillée, P., Veremeeva A., Kanevskiy, M.Z., Gaglioti, B.V., Breen, A.L., Parsekian, A.D., Ulrich, M., and Hinkel, K.M.: Lake and drained lake basin systems in lowland permafrost regions. Nature Reviews Earth and Environment 3: 85-98. https://doi.org/10.1038/s43017-021-00238-9, 2022.

Jorgenson, M.T.: Thermokarst terrains. In: Shroder, J. (Editor in Chief), Giardino, R., Harbor, J. (Eds.), Treatise on Geomorphology. Academic Press, San Diego, CA, vol. 8, Glacial and Periglacial Geomorphology, pp. 313–324, 2013.

Jorgenson, T., Yoshikawa, K., Kanevskiy, M., Shur, Y., Romanovsky, V., Marchenko, S., Grosse, G., Brown, J., and Jones, B.: Permafrost characteristics of Alaska. In: Proceedings of the 9th International Conference on Permafrost, Extended Abstracts. June 29–July 3, 2008, Fairbanks, AK. Kane, D.L., and Hinkel, K.M. (Eds.), Institute of Northern Engineering, University of Alaska Fairbanks: 121–122, 2008.

Czudek, T. and Demek, J.: Thermokarst in Siberia and its influence on the development of lowland relief, Quaternary Research, 1(1), pp.103-120, 1970.

- Some minor issues noted below, and some jargon and unnecessarily complex language used to describe especially mathematical derivations.

We reduce the number of jargons by choosing better terms (e.g. fusion energy → thaw heat; conduction heat → outgoing heat; Line 158-168). However, as this study covers the different field of studies, some jargons are unavoidable. More explanation was added in the revised manuscript so that broader range of readers can understand the content better.

- For the most part, some missing units and inconsistent use of symbols detailed below

We provided the missing units (e.g. unit for heat conductivity on Line 161).

- I am unsure of the comparison with field data - I think either this section should be expanded to include more sites, removed (which I am sure the authors agree would detract from the merit of this contribution), or perhaps re-phrased as an example application of this new method and not a test of the method proving its efficacy.

It is hard to obtain the talik depth measurements under an isolated lake in a continuous permafrost as stated above.

- L 23 … the Euler equation and the calculus of variations

The original manuscript focused on the mathematical technique, which appears as "Euler equation in the calculus of variation". We propose more complete explanation with the physical context beyond the Newtonian mechanics, which hopefully helps readers to understand the background of this idea in some extent. "Euler–Lagrange equation" replaces "Euler equation in the calculus of variation" in the revised manuscript.

We added the following paragraph at the beginning of Chapter 2, Theory (Line 132 – 147).

> The approach used in this study is based on Lagrangian mechanics, which generalizes the classical Newtonian mechanics, using the stationary action principle (the principle of least action). The action is defined as the integral of the Lagrangian, which consists of kinetic and potential energy of the system. In this application, the Lagrangian simply becomes the potential energy due to absence of kinetic energy. The variational principle that is the main tool in Lagrangian mechanics can indeed derive the equations in Newtonian mechanics. One of the related research topics using the variational principle to fluid mechanics is phase boundary propagation, which can be analyzed by the phase field model or diffusion-interface model (Cassel, 2013). This model explains the diffuse phase boundary without surface tension that appears in Newtonian interfacial physics between a liquid and a gas. According to the second law of thermodynamics, the free energy of the system must decrease monotonically to ensure a non-negative entropy production (Singer-Loginova and Singer, 2008). This requires that the time rate of change of the phase boundary be expressed by the functional derivative of the free energy functional, which corresponds to the talik total energy flux in relation to permafrost thaw. This study directly and analytically solves the Euler-Lagrange equation based on the stationary action principle rather than the entropy functional used in the phase field method.

- L 24 an extremum of the functional -> a minimum of the energy associated with the functional description of the phase boundary (for clarity)

Thank you for the suggestion. We revised it, accordingly (Line 226). Equation (15) uses the method of Lagrange multipliers which is a common tool in the machine learning field (e.g. maximum entropy principle) for optimization. We indicate the name of the method (method of Lagrange multiplier) for readers to understand the physical interpretation. (Please see around Line 221 – 225)

- L 32 stabilizes thermokarst lakes -> stabilizes the size? shape? growth rate? be specific

It implies "deepens lakes and results in more uniform horizontal lake expansion …". We revised the part (Line 33-34).

- L 45 … above an unfrozen water body (CITATION NEEDED)

We cite followings:

Burn, C.R.: Tundra lakes and permafrost, Richards Island, western Arctic coast, Canada, Canadian Journal of Earth Sciences, 39(8), pp.1281-1298, 2002.

Jeffries, M.O., Morris, K. and Liston, G.E.: A method to determine lake depth and water availability on the North Slope of Alaska with spaceborne imaging radar and numerical ice growth modelling. Arctic, pp.367-374, 1996.

- L 50 … bed consequently subsides (CITATION NEEDED)

We cited Jorgenson and Shur, 2007; Shur and Osterkamp, 2007; Jorgenson, 2013; French, 2018 (Line53-54).

L 84 advanced the technique by including (parameter of)<-remove advective heat transport

We revised the part accordingly (Line 91).

- L 141 k_L needs units!

Thank you. It should be W/(m·℃). We added it on Line 161-162.

- L 151 should read: the letters in bold denote vectors.

We revised the part accordingly (Line 176).

- L 152 remove 'also'

We revised the part accordingly (Line 177).

- L 181 in the horizontal direction -> the horizontal gradient

We revised the part accordingly (Line 206).

- L 182 this is not easily interpreted, especially for the general audience of this journal. Are you implying Stoke's theorem? I think it would strengthen the derivation to begin from a more 'certain' or understandable place than eq 183

No, it is not Stoke's theorem although it is related. We quantify the free energy functional for the Euler-Lagrange Equation. Please see the response above.

- Eq 13 I think it may make more logical sense to present this in the opposite direction - the integral along the phase boundary (line) is not something that I can interpret easily or can be visualized, whereas something more like the flux across the phase change surface, or the volume integral of the total energy in the lake is more easily interpreted. I would start with the resulting equation and state which theorem (Stoke's?) is used to get the first equation. Importantly providing a physical interpretation (in more simple terms) of what each expression (start and derived result) means and how it is useful and what it tells us about the system. This would greatly increase the utility of the work for those who are less interested in the mathematics and more interested in their application.

We added the paragraph presented above at the beginning of Theory chapter to clarify the approach before the derivation (Line 132-147). Also, Equation (15) uses the method of Lagrange multipliers which is a common approach in machine learning field, lately. We hope the name of the method helps for readers to understand the physical interpretation.

- l 187 section heading is redundant: optimum phase boundary shape - is sufficient

It is a good point. The section title was revised as optimum phase boundary (Line 212)

- l 190 "here, we present the …" - present is not a good word choice

We revised as, "Thermally optimum function type $\varphi(x, y)$ of the phase boundary can be derived using this method."

- l 193 "weighted phase boundary area" this is not easily visualized/interpreted. Can you describe more concretely what this key phase means and what quantity is weighted along the phase boundary before using it (it occurs several times throughout)

The weight is alpha. Note was added on Line 211. The area element without weight in Cartesian coordinate can be expressed as,

$$\sqrt{\varphi_x{}^2 + \varphi_y{}^2 + 1}.$$

The corresponding phase boundary area is,

$$A[\varphi] = \iint_B \sqrt{\varphi_x{}^2 + \varphi_y{}^2 + 1}\, dxdy$$

However, without the weights the optimum shape is sphere, which is irrelevant here. As we consider the vertical thermal gradient, the phase boundary area must be weighted as

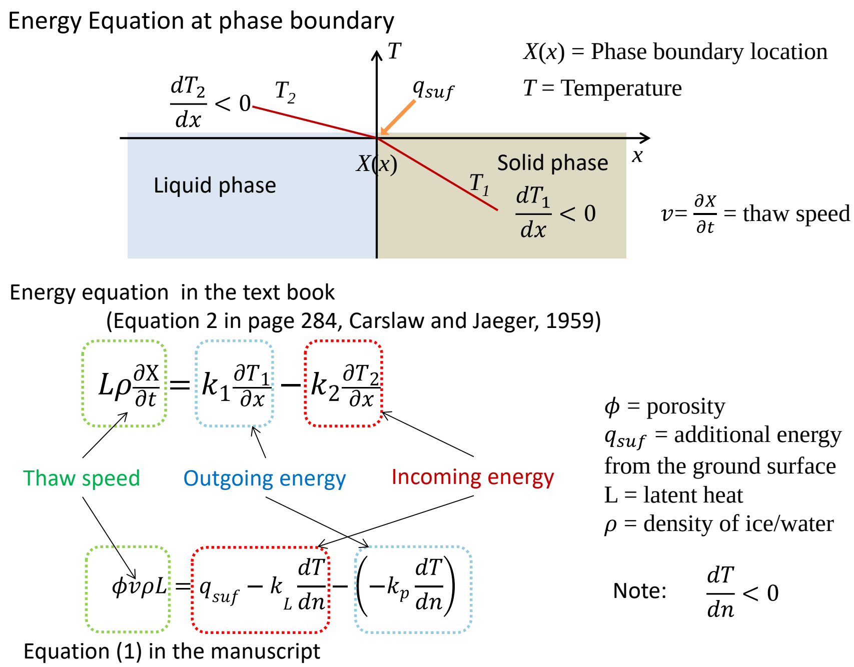
$$A[\varphi] = \iint_B \sqrt{\alpha_x{}^2\varphi_x{}^2 + \alpha_y{}^2\varphi_y{}^2 + 1}\, dxdy$$

The purpose of Section 2.1 is to show validity of these weights.

- l 195 it does not seem logically evident that the shape of a talik would preferentially minimize the total permafrost thaw given an amount of incoming energy. Please either provide a reference, explain the logic, or reconsider. The energy would simply Óow according to the thermal gradients, and more energy would be used where gradients are strongest, no? If there is data for the temperature gradients here or in other lakes that would be helpful.

The free energy of the system must decrease monotonically to ensure a non-negative entropy production (the second law of thermodynamics). This law suffices for the minimization of talik total thaw. We added one phrase on Line 221-223 about it.

Also, it is obvious from the energy conservation equation at the phase boundary (Equation 1) that the milder thermal gradient in solid permafrost results in faster thaw. We prepare a diagram below for convenience.



- eq 14 the volume is a double integral and not a triple integral - why?

It is because phi(x,y) describes the depth of talik (distance from the surface).

- l 224 (and future occurrences) “this 3D Stefan equation” should it not be the solution to the 3D Stefan equation? Where do you introduce the Stefan equation? eq 4? State in the text when this occurs otherwise this conclusion to the methodology here doesn’t really make sense

Stefan equation describes the phase boundary depth (active layer depth or frost depth) under the quasi-steady state heat balance. The solution of the Euler-Lagrange equation describes the phase boundary depth beneath a seasonal water body. Hence, we think the elliptic function is the 3D Stefan equation (not a solution of Stefan equation). We added one sentence (Line 252-254)

- eq 24 - define units for new parameters, and define r_{deg} in text. Where is this equation from? Appendix? if so please say where to find derivations, if not consider including this.

It is dimensionless (ratio; m/m) (added on Line 259). We could not find such a degradation ratio as a function of thawed permafrost depth in the literature. Therefore, we newly introduced it. Equation 24 is the definition.

- l 245 unlikely -> not a

There is no evidence that it is impossible either.

- l 239-248 note if there was evidence of segregated ground ice in these sites

Please see the references, Kanevskiy et al., 2013, 2016.

- eq 25 r_{sub} what is this?? Is it the same as r_{deg}?? rename or deÒne

Sorry, that is a typography. All r_sub should be r_deg. We fixed it (on Lines 281 and 284).

- l 269 missing work : … not eh western shore determined form….

We revised the part accordingly (Line 301-302).

- lSSS 3.1 describe thaw rates a bit more (especially in reference to Figure) the range is wide, I assume the high thaw rates are observed in a similar location… also how complex is the shoreline shape? Does it vary around the lake?

Relatively flat lake bathymetry observed by Lenz et al. (2016) suggests uniform subsidence with deep talik. However, we understand that it is complex near shoreline. This result actually led to the discussion chapter.

- l 283 solution to … also remove 'the' based on 27 talk thickness …"

We removed "the" (Line 318)

- l 301 model geometry not geometry model

We revised the part accordingly (Line 336).

- l 304 lake expansion is most rapid

We revised the part accordingly (Line 339).

- l 324 "wind erosion effect" not mentioned until now, please elaborate earlier or save this for the section reporting it exclusively

The "wind-erosion effect" was deleted because it is not discussed in Peatball Lake case study (Line 359).

Meanwhile, we revised the Line 70-71 as, "It has been proposed that winds at the lake surface cause currents and water waves, which trigger thermomechanical bank erosion, resulting in asymmetrical elliptical orientation (Livingstone, 1954; Rex, 1961; Carson and Hussey, 1962; Mackay, 1992; Arp et al., 2011).".

- l 341 is the assumption that the radial thermal gradient is zero accurate? Other publications report much more rapid than vertical thaw (though my focus is discontinuous PF) see McClymont et al. Devoie et al. work at Scotty Creek. Please cite something or report thermal gradients to support this.

Thank you. We cited McClymont et al. (2013) and Devoie et al. (2021) to support the approximation that the inter-seasonal average of the horizontal thermal gradient is negligible (Line 376-377)

McClymont, A.F., Hayashi, M., Bentley, L.R. and Christensen, B.S.: Geophysical imaging and thermal modeling of subsurface morphology and thaw evolution of discontinuous permafrost, Journal of Geophysical Research: Earth Surface, 118(3), pp.1826-1837, 2013.

Devoie, É.G., Craig, J.R., Dominico, M., Carpino, O., Connon, R.F., Rudy, A.C. and Quinton, W.L.: Mechanisms of Discontinuous Permafrost Thaw in Peatlands, Journal of Geophysical Research: Earth Surface, 126(11), p.e2021JF006204, 2021.

- l 371 the preceding discussion all hinges on the zero lateral gradient assumption - please highlight this otherwise it seems unlikely

Thank you. We revised the assumption statement as "… assuming all other properties and horizontal thermal gradient variation are equal…" on Line 400-403.

- l 395-396 this is the Òrst mention of anthropogenic processes, and none of the bullet points are direct human activities. Suggest to remove this idea unless there is an additional section on anthropogenic change

We removed the human activity impact statement (Line 430). The part is "Talik development is natural processes governed by local conditions that favor talik initiation and growth include:"

- l 408 what are horizontally oriented lakes? Please describe as this term is not clear

The horizontally orientation here is direction dependent elongation. We revised this part to, "…analyze the horizontally oriented lakes with direction dependent elongation…" (Line 443-444)

- l 443 easier to understand would be: Wind-driven wave action make the water bodies round…. (remove asymptotically - this does not belong)

We removed "asymptotically".

- l 493 why is it more rapid?

We removed "more rapid" as the speed of thaw is not discussed here. We revise the part as, "In general, the shallower lakes common in coastal areas, such as Teshekpuk, Barrow, and Kuparuk, are more elongated likely due to wind wave erosion. Whereas lakes in Umiat (ice-rich permafrost), Seward Peninsula (ice-rich permafrost), and Inigok (ice-poor permafrost) tend to be rounder because of talik development and the presence of deeper lakes (on the order of 10-20 m in some instances). This remote-sensing based evidence implies that the wind effect seems to be limited by the lake thermal subsidence due to sub-lake talik development, while shallow lakes with the bedfast ice may continue elongating by wind erosion." (Line 527-532)

- l 510 for the horizontal stage (COMMA), A in Figure 6,

We revised the part accordingly (Line 546).

- l 527 solution to 3D Stefan equation is limited

It should be kept as is. This is not a solution of 3D Stefan equation but the 3D Stefan equation.

- l 535 what about anisotropic thermal properties? Maybe also discuss these as well?

Sorry. We could not catch it. Thermal properties (e.g. thermal conductivity, latent heat, and thawing temperature) should be isotropic, constant, and uniform. However, the thermal field is anisotropic (e.g. vertical temperature gradient).

- l 541 state the thermal effect on thermokarst morphology, I am not sure what this refers to in the manuscript as is written now, so it is either unclear or unsupported

Thermal effect is described by the 3D Stefan equation. We added, "(e.g. ellipsoidal talik geometry)" on Line 577.

- l 557 wieighted phase boundary (again weighted according to what?)

It was explained about it earlier. Please refer to it. Note was added on Line 211. Thank you.

- Suggestion: due to the lack of evidence supporting the conclusions on shape, it seems that the argument is stronger for the phases of formation and evolution of thermokarst lakes, so it may be more relevant to report more strongly on this aspect in the abstract and conclusion? As an alternative/addition to my previous comments on data comparison

We adjusted the title and the manuscript to reflect the discussion section. We suggest, "A New Stefan Equation to Characterize the Evolution of Thermokarst Lake and Talik Geometry"

Reviewer 2

- The discussion paper analyzes geometry of taliks forming below permafrost thaw lakes, compares the resulting model to field data from one site, and provides a more qualitative discussion on permafrost thaw lake dynamics and influence of those dynamics on talik growth. The work provides insights into environmental controls on thaw lake taliks and implicitly on their evolution in a warming Arctic. The subject is of significant interest to the readers of TC and the approach used is relatively novel and appropriate. Overall, the discussion paper provides some interesting insights and is a welcome contribution. I do think the manuscript could be improved by paying some more attention to the presentation of the model in Section 2 and to implications of the work.

Thank you for the overall positive evaluation.

- The title of the discussion paper focuses on the analysis of Section 2, but doesn't provide an adequate indication of Section 4, which provides a much richer picture of thaw lake and thaw lake talik dynamics. This paper might be more impactful if the authors can find a title that better reflects the breadth of the analyses.

It is a good idea as this title was created at the earlier mathematical derivation stage. We suggest, "A New Stefan Equation to Characterize the Evolution of Thermokarst Lake and Talik Geometry"

- Description around the derivation in Section 2 is often unclear and in some places imprecise and will make it difficult to follow for some. For example, it's clear in the scalar equation 1 that q_f is the energy available to thaw permafrost per unit time and area. Generalizing to the vector equation 5, it's a little easier to understand q_f as the velocity of the moving phase boundary scaled by the volumetric latent heat of fusion for water ice to make it an equivalent heat flux, as in equation 2. Referring to q_f as the 'fusion heat vector' is a bit obscure. This might be easier to follow and would avoid that jargon by doing the analyses in the velocity v instead (eg. By dividing both sides of eq 5 by \phi \rho L see Eq 2). An alternative approach would be to clearly describe the physical interpretation of the vector q_f around Eq. 5, give q_f a better name and stick to that name in the rest of the manuscript.

Thank you for the suggestion. We tried to improve the first introduction of heat equation.

We changed the name to q_f from "heat for fusion or thawing" to simply "thaw energy" while this term can be refreezing with negative q_f. Hence, the vector may be named, 'talik expansion vector". Also, the letter is altered to q_th. Please see the revised section (Line 157-189).

- It would be helpful to summarize assumptions behind Eq 1 when Eq 1 is introduced. This is addressed somewhat in Section 4, but it would be helpful have that stated more explicitly.

The assumptions for Equation (1) are now included in the revised manuscript (Line 152-155). They are as follows:

The material of permafrost and talik is fully saturated with ice and water, respectively. The thermal constants (thermal conductivity, latent heat, and thawing temperature) are constant and isotropic. Change in volume of water on thawing and freezing is negligible.

- The reader needs to know why the functional F is introduced this way in Equation 15 (i.e. you want to minimize the boundary area for a specified thaw volume, the symbol \lambda is a Lagrange multiplier, etc.)

As explained above for Reviewer 1, the second law of thermodynamics ensures a non-negative entropy production throughout the system. We included this explanation as proposed above. Please refer to Line 132-147 and Line 221-223 of the revised manuscript.

- Also in line 197, it would be clearer to say "for a specified talik volume" instead "for the total talik expansion". Similarly, the sentence starting on Line 194 could be clarified.

We adjusted the part (Line 221-223). Thank you for the good suggestion.

- This paper contains several insights that could inform representations of thaw lake dynamics in Earth System Models. If possible, it would be useful if the authors could comment on that.

We added following sentence at the end of the conclusion section (Line 612-614):

"Moreover, the analytical expression of the 3D Stefan Equation can be potentially incorporated in the global or regional scale Earth system model to describe missing sub-grid scale processes such as lake dynamics with minimal additional computational resources."

References

Cahn, J. W., & Hilliard, J. E. (1958). Free energy of a nonuniform system. I. Interfacial free energy. The Journal of chemical physics, 28(2), 258-267.

Cassel, K. W. (2013). Variational methods with applications in science and engineering. Cambridge University Press.

Singer-Loginova, I., & Singer, H. M. (2008). The phase field technique for modeling multiphase materials. Reports on progress in physics, 71(10), 106501.