*Review of **"Contributions of supraglacial lakes to the Greenland Ice Sheet melting"** by Cotronei et al. (2026)*

This paper presents a simplified modelling approach to examine how ponded surface meltwater influences surface topography and the long-term mass balance of the Greenland Ice Sheet. The topic is interesting and clearly important, particularly for improving how the effects of surface meltwater ponding on albedo and hence melt—processes that are often omitted—are represented in regional and global climate models. However, I have a number of concerns regarding the overall rigor of the study, as well as the specific modelling set-up and framework.

I note that I have chosen to provide a more high-level, big picture, review here. I am not an expert in the mathematical aspects of the paper, and I am also the third reviewer; the first two reviews are detailed and include extensive line-by-line comments, with which I agree.

A side comment is that I think this paper would fit very well in EGU's *The Cryosphere* journal. I am not familiar with the Earth System Dynamics journal, so cannot comment on how well it fits there.

Major comments:

Although the paper includes a general background on the future evolution of the Greenland Ice Sheet (GrIS), particularly in terms of melt and potential mass loss, it lacks sufficient engagement with existing literature on supraglacial lake modelling and specifically, on the amplifying effects of surface meltwater (via albedo reduction) on further melt.

A key supraglacial lake model is not mentioned: *GlacierLake,* by Law et al. (2020), likely as the authors were simply not aware of it. This is a one-dimensional numerical energy-balance and phase-transition model developed for GrIS supraglacial lakes. It includes processes such as snowfall, in situ snow and ice melt, inflow from the surrounding catchment, ice lid formation, basal freeze-up, and thermal stratification. In contrast, the model presented here neglects the key processes of lake refreezing, which is important (see Dunmire et al., 2025 as another reviewer also suggests). Instead, the model presented here assumes that all meltwater drains to the ice sheet margin, which is not realistic. Given this, the authors may wish to consider how their modelling approach compares with, or could be informed by, the model of Law et al. (2020).

A related modelling framework to Law et al (2020) is presented by Buzzard et al. (2018), but instead developed for lakes on ice shelves. While this study is cited, it is only briefly mentioned in relation to model details in Section 2. In addition, the SHED model (Gantayat et al., 2023) represents surface, englacial, and subglacial hydrology for the GrIS, and includes lake formation and refreezing processes informed by both Law et al. (2020) and Buzzard et al. (2018). A more thorough comparison with these lake modelling studies would strengthen the manuscript and may help the authors to improve their model to make it more realistic, particularly in regard to lake refreezing.

It would also be beneficial for the authors to mention recent work examining the role of surface meltwater ponding (and slush) in amplifying melt through albedo feedbacks. For example, Ryan et al. (2025) demonstrate that meltwater ponding significantly increases the energy available for melting the GrIS, highlighting its impact on spatial albedo patterns. Similarly, Dell et al. (2025) show that slush can substantially reduce surface albedo and enhance melt on ice shelves. Although focused on slush rather than lakes, the underlying process and implications are comparable. Incorporating discussion of these studies would help better contextualize the importance of the processes considered here.

With regard to the model description and set-up, a number of important details are either insufficiently explained or omitted, particularly in the earlier sections. E.g. the fact that only a 2D transect of the ice sheet is modelled is mentioned too late.

I am very unclear about how lakes drain in the model. In reality, lakes either drain rapidly by hydrofracture to the ice sheet bed (i.e. vertical, rapid drainage through a crevasse/moulin; e.g. Das et al., 2008; Tedesco et al., 2013), or slowly, by overflow via a stream/river into nearby lake basin (Tedesco et al., 2013; Kingslake et al. 2015). Various hydrological models have dealt with both the slow/overflow form of drainage and rapid/hydrofracture type of drainage (e.g. Banwell et al 2016; Koziol et al 2018). I am unclear what is meant by vertical ‘seepage’ in the paper. The authors should clarify this. When ‘seepage’ is first introduced on line 41, four references are given (Stevens et al., 2015; Christoffersen et al., 2018; Chudley et al., 2019; Humbert et al., 2025), but none of those references mention the process of ‘seepage’. On line 127, the following is stated: “The seepage SE refers to the infiltration or percolation of liquid water as it moves vertically through the porous structure of the ice.” But the issue with this assumption is that glacier ice is impermeable (not porous), and hence cannot access the bed this way.

In summary, this study addresses an important and timely question. However, in its current form, the manuscript requires substantial revision. In particular, the treatment of key physical processes—especially lake drainage and the lack of refreezing—and the limited engagement with existing literature raise concerns about the realism and applicability of the results. I encourage the authors to more clearly position their model within the existing body of work, better justify their modelling choices, and clarify the representation (or omission) of key processes. Addressing these issues would significantly strengthen the manuscript.

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