The authors developed a differentiable land model. They investigated the optimization performance using temperature observations from one and two depths. A synthetic study was performed followed by a real-world application. Particularly, they found that it is hard to estimate accurately parameters that serve as confounding factors. The manuscript is easy to follow and well organized. However, I found the contribution of the proposed incremental and thus suggest the rejection of the manuscript.

1. First of all, there are missing key literature reviews on differentiable land surface model (LSM). Differentiable LSM is not new anymore. Several studies in the past few years developed different differentiable LSMs by refactoring the physical models into a framework that supports automatic differentiation (e.g., JAX, PyTorch, and Julia), including but not limited to, Fang and Gentine (2024; doi: 10.1029/2024MS004308) and Jiang et al. (2025; doi: 10.1029/2024WR038116). They are neither referred or discussed in the manuscript. It is also unclear what's the benefit of the proposed differentiable LSM compared to the existing one, instead of having a new model available in the market.

2. Second, the so-called "Neural physics" might be over-stated. While the model is reimplemented in PyTorch, no trainable deep learning layer is used except the adoption of a convolutional layer with fixed parameters to solve the heat equation. In that sense, the whole model is still pure physics-based and no DL is involved to learn additional dynamics that are not captured by the model. It is oftentimes expected to see the adoption of hybrid modeling in these differentiable modeling, which in fact has been studied in Fang and Gentine (2024) and Jiang et al. (2025).

3. Essentially, the adopted optimization technique is nothing new but a gradient-based optimization. While supported by AD, this is fundamentally no different from other gradient-based approach. Trying different initial values is also nothing new in optimization field which is always struggling in getting the global optimal solution.

4. Lastly, the challenge of estimating dependent or confounding parameters is not addressed except a restatement of the existing difficulty in this particularly setting. In fact, such equifinality issue is a long-standing challenge in calibrating earth system model or any physical models. I would love to see any new angle to tackle the problem. But again, I didn't find new results out of here.