Point-by-point replies to each comment are listed below.

1. **Comment:** *The application of Geometric Brownian Motion (GBM) and Poisson processes to model lake area change and formation/drainage events is innovative and well-justified within the context of landscape-scale heterogeneity and stochasticity. This is a significant step beyond previous deterministic or analytical models.*

   Reply: Thank you for this comment, which emphasizes the innovative nature of our modelling approach.

2. **Comment:** *The three idealized regimes (Complete Drainage, Oscillation, Stabilization) effectively demonstrate the model's behavioral range and provide a useful conceptual framework for understanding possible long-term trajectories of thermokarst landscapes. The links to real-world examples for each regime are well-made.*

   Reply: Thank you for this comment, which mentions that our presentation of the three idealized regimes provides an effective demonstration of the model's behavioural range and contains well-made links to real-world examples.

3. **Comment:** *The current merging algorithm is identified correctly as a significant weakness. The assumption that merged lake area is the sum of the two original areas and that the new lake is perfectly circular is physically unrealistic and computationally expensive. It likely leads to a drastic overestimation of lake sizes and an underestimation of lake numbers.*

   Reply: We agree that the merging algorithm is a significant weakness. We are planning to test a new approach following suggestions from Reviewer 2 as part of the revision process. However, we argue that even without this change, a publication could still be beneficial for the permafrost modelling community and invite further model development led by other researchers with more expertise in potentially useful fields such as percolation theory. As mentioned in the comment, the weakness of the current merging algorithm is already addressed in the current version of the manuscript.

4. **Comment:** *The high percentage of unusable data points (30-33%) in the remote sensing dataset severely impacts the robustness of the parameter estimation. This uncertainty propagates through the observation-based simulations and limits the confidence in the derived parameters (λf, λd, μ, σ).*

   Reply: Thank you for highlighting the issue of low confidence in parameter estimates due to gaps in the observational data.

5. **Comment:** *A crucial result is that the stochastic component (σ) dominates the deterministic drift (μ) in the observed period. This implies that, for the 2000-2020 period in this region, random environmental variability was a stronger driver of annual lake area changes than any clear climate-driven trend, explaining the lack of strong correlations.*

Reply: Thank you for highlighting this. We will emphasize this point more in the "Conclusions". Furthermore, we will review this finding after testing the methods described in comments 7 and 8 by Reviewer 2.

6. **Comment:** *The inability to find significant correlations between model parameters and climate variables (TDD, P) is a notable negative result. While honestly reported, it highlights the current impossibility of confidently projecting lake dynamics under climate change scenarios with this model, as intended in the abstract. This is a major constraint on its immediate application in ESMs.*

   Reply: We agree that this issue significantly limits the immediate application of the model in ESMs. We will address implications for ESMs more directly in the "Discussion" and "Conclusions". Before that, however, we will review this finding following comment 8 by Reviewer 2.

7. **Comment:** *The observation-based simulations project water area fractions increasing to over 50%, which is acknowledged as rare. This, combined with the high volatility, suggests the model parameters derived from 20 years of data may not be stable or representative of centennial-scale dynamics, potentially overestimating expansion.*

   Reply: We agree that the results of the observation-based simulations suggest that the model parameter derived from 20 years of data might not be suitable for centennial-scale dynamics. We will add one or two sentences to the "Discussion" that mentioning this directly.

8. **Comment:** *The suggestion that the idealized simulations could be interpreted as spanning 10 ka with a 10-year time step is helpful for context, but the parameters would then be "per decade." This should be stated explicitly in the text to avoid confusion (e.g., in Table 1, add a note: "Parameters are per year; for a 10-year time step interpretation, values would be per decade").*

   Reply: Thank you for the suggestion of adding a note to Table 1 regarding the interpretability of hypothetical parameter values as being "per decade", and will implement this.

9. **Comment:** *The comparison with the van Huissteden et al. (2011) model is good. The explanation for the differing results (their reliance on a prescribed river network vs. your data-driven drainage rates) is plausible and highlights the advantage of your approach, but also its current data dependency.*

   Reply: Thank you for this comment, which mentions the usefulness of our comparison with the model by van Huissteden et al. 2011.

10. **Comment:** *The definition of "abrupt drainage" as a complete (>90% loss) and rapid event is clear. However, the discussion of results from other studies (Jones et al., 2011, 2020) that use different thresholds (e.g., >25% loss) is slightly confusing. A*

small table summarizing different study's definitions and converting their rates to a common framework would be helpful.

Reply: We agree that the current comparison with other studies (Jones et al. 2020 and Jones et al. 2011) on lake drainage is confusing considering the different thresholds that were used to define drainage events. To improve the comparison between these two studies with ours, we are planning to identify all lakes in our simulations that have lost at least 25% of their area. Since there are no standard thresholds or definitions for different drainage types, as far as we are aware, this number is somewhat arbitrary, but in line with the definition in Jones et al. 2011 & 2020. This will effectively also include our definition of gradual drainage and make a comparison more justified. With this change, we do not see a benefit of a table, as we will only use one definition of lake drainage for the comparison.

11. **Comment:** *The axis titles of many figures are obscured, or the figures are not very clear, for example, in Figures 5, 6 and B1.*

    Reply: Thank you for pointing this out. We will revise the figures to include fully readable axis and axis titles.

12. **Comment:** *There are some spelling errors in the manuscript. For instance, the first reference should read "thermokarst lake" instead. The authors should carefully proofread the manuscript to avoid such errors.*

    Reply: Thank you for pointing this out. We will carefully proof-read the manuscript.