Responses to comments from Associate Editor on manuscript entitled 'Short communication: Learning How Landscapes Evolve with Neural Operators'

I've now looked through the reviewer comments, responses, and revised manuscript. Both reviewers found the approach quite interesting, were generally positive about the paper, and asked for some clarifications and more discussion. The revision contains some of this requested material, but in some places the review and response is much more enlightening than the final manuscript. I suggest bringing more of that discussion into the paper.
Thank you for the guidance. I have moved much of the discussion that featured in the review and response document into the main manuscript, with some modification to aid flow. Details on what was incorporated, and in some places expanded upon, follows. The main changes are to the discussion of the methodology and the results and discussion section.

There were requests to clarify what the technique does, and I think a few extra statements do a good job of making the approach easier to understand. However, there is a section where the mechanics of the technique are explained, this is reduced to five or six sentences, and an encouragement to read the Li paper. I think this section could add a few more sentences to help readers (i.e. a bit more on the steps of increasing then decreasing the dimensionality with the neural network). I leave it up to the author if they think clarifying details can be added succinctly.
I have added more detail on the Fourier Neural Operator approach, including its parameterisation in Section 2.2. This new material gives (a) additional information about how the input data is raised to a higher dimensional representation, following the procedures described in Li et al., 2022, (b) parameterizations used to do so in this study, i.e., the values of model 'width', (c) reference to Kovachki et al. (2023), where the apparent benefits of lifting the data to higher dimensions is discussed, and (d) a sentence directing the reader to the code provided, which includes the lift and later reduction in dimensions. In the expanded results and discussion section I return to the topic of raising to a higher dimension. In there I briefly summarise the responses that Li et al. provided to reviews and those that Kovachki et al. discuss in their paper regarding their results. I summarise their demonstrations of spectra at frequencies higher than k_{max} being reliably predicted despite higher Fourier modes being removed when generating the operators. They attribute these results to the raising of input data to higher dimensions. I give a (brief and tentative) view on why flow routing and the importance of noise in landscape evolution models might make achieving similar success, e.g. accurate prediction of specific valley geometries, challenging for landscapes. This discussion includes a summary of preliminary tests I have run, which demonstrate that changing model 'widths' from 1—32 appears to have little impact on the geometries of predicted landscapes. I note here, and in the revised manuscript, that these results are very much preliminary, and that further testing, e.g. lifting to much higher dimensions, might be fruitful.

A bigger issue for me is the reviewer comment about flow routing, valley structure, and connectivity. There is an interesting discussion in the response that does not find its way into the paper. The predicted surfaces do not have the same obvious features as the stream power model results. They lack the ridge and valley structure and degree of dissection in the stream power model

outputs. This is attributed to the smoothing effect of the fourier convolution. In addition the networks are not connected. The stream power model forces pit filling and drainage connectivity and the FNO appears unable to capture this behaviour. The reviewer suggests a workaround, which is then addressed in the response letter. I strongly urge the author to add more discussion of this in the manuscript.

I have expanded the discussion of sinks and flow routing in the results and discussion section to now include the material on lines 145 to 159, which I hope is satisfactory.

In addition, we are shown a comparison of the stream power-derived surface and FNO surfaces using a very basic elevation binning that only includes 10 bins. It is a rather crude approximation. Can I suggest adding in the hypsometric curve (which will give better granularity on where the models differ) and I think it would be quite interesting to have some swath profiles coming in from the boundary of the two sets of surfaces. The upstream tip of the erosion wave appears to be well reproduced, but how well does it do with the average mass loss behind that tip? I think a swath would show that quite clearly.

Thank you for the guidance, which I have followed to produce the new Figure 3. It shows transects through the SPM and FNO models, the histograms that were in figure 2 and the new hypsometric curves. I see little evidence in these panels, nor in the revised Figure 2 of a systematic bias in the predictions from the FNO models, e.g. erosion of the ‘escarpments’ being too quick or slow. Instead, I think the FNO models are providing ‘smooth’ predictions of landscape evolution, looking like a low pass filter of the predictions from the SPM model.

Overall I think this manuscript does an excellent job of introducing a new method to the landscape evolution community, it shows that the method is ripe for more exploration, and I will be happy to see this in ESURF with a few cosmetic changes.

Thank you. I hope that the new changes are satisfactory.