Associate editor (Fiona Clubb):

Many thanks for your constructive engagement with the review process and your response to the reviewers' comments. Your manuscript has now been re-reviewed by both reviewers. Reviewer 1 has some minor comments to be addressed, but Reviewer 2 has suggested there is still a fundamental problem with a core assumption of the adjoint method that you present - that $\nabla \cdot c = 0$. They are concerned that this assumption is at the heart of the method and invalidates the use of the adjoint method. This issue would need to be fully and comprehensively addressed or rebutted in a further response before the paper can be accepted in ESurf.

Re: Thank you for this comment. We agree that this point deserves more thorough explanations, but this assumption is valid under the consideration of the Stream Power Law. Please see the detailed response to Reviewer 2 and the new appendix presented in the revised version of the manuscript.

Important note to associate editor and reviewers*: Since the original version of our code that was used for the first version of the paper, the Firedrake package has been upgraded. While we initially intended to maintain compatibility with the original version used in our code, that version has become increasingly difficult to install due to numerous dependency issues. To ensure the reproducibility of our results with minimal installation effort, we have modified and updated our code to be compatible with a more recent release of Firedrake. This update has slightly affected the results of the adjoint model, especially those dependent on regularization parameters. It changes a few results in initial condition inversions, with a more realistic topography in the Massif Central case. The rest is not different from the previous version. We therefore provide updated figures and text describing the results obtained with the new Firedrake version, along with a link to the repository containing the new code. We also took this opportunity to fix the bug about outgoing sedimentary flux raised by reviewer 1, improving both the stability and inversion performance with the updated Firedrake version.*

If you feel like this issue can be addressed and would like to submit a further revised manuscript, please also address the following points in addition to those raised by the reviewers:

Reviewer 1 mentioned in the first round of reviews that the use of the case studies was limited and there were not clear scientific questions behind them. The revised manuscript does a much better job of addressing clearer scientific questions, but I think that the structure is still odd here. None of the method sections contain any information about the purpose or setup of the synthetic or natural case studies, which are also not properly mentioned in the introduction. This makes it difficult for the reader to understand what results are actually presented in the manuscript. I suggest expanding Section 4.1 and moving it to the methods, as well as better integrating the case studies/scientific questions throughout the intro and methods. For example, the purpose of choosing these specific case studies should come earlier in the paper.

Re: Thanks for this suggestion. This has been taken into account. In the revised version the forward model, adjoint and the case studies are embedded in a "Methods" section. We have added a synthetic overview of the scientific questions in the intro, and modified the "natural cases" section in order to avoid redundancy (lines 106-115, 368-378 and 426-431).

Figure 2: panel d is missing an x axis

Re: Thanks for pointing this out. However, we do not see this in our version of the manuscript, maybe something went wrong during the conversion and upload of the paper. We will carefully review the files during the submission process to ensure that everything is as expected.

Figure 3: legend label is cut off in panel c.

Re: Thanks for pointing this out. We do not see it either in our version but we will take care of display issues during the submission process.

Referee 1 (John Armitage):

The manuscript is a significant improvement on the original submission and I thank the authors for taking the time to consider my earlier comments. I do however note a few questions/comments that could be addressed:

Line 150 to 156: The Firedrake space function is mentioned without the context of what Firedrake is or does. I think that these technical details could come after the description of the adjoint method and the introduction of the Firedrake package that aids the implementation of the adjoint method.

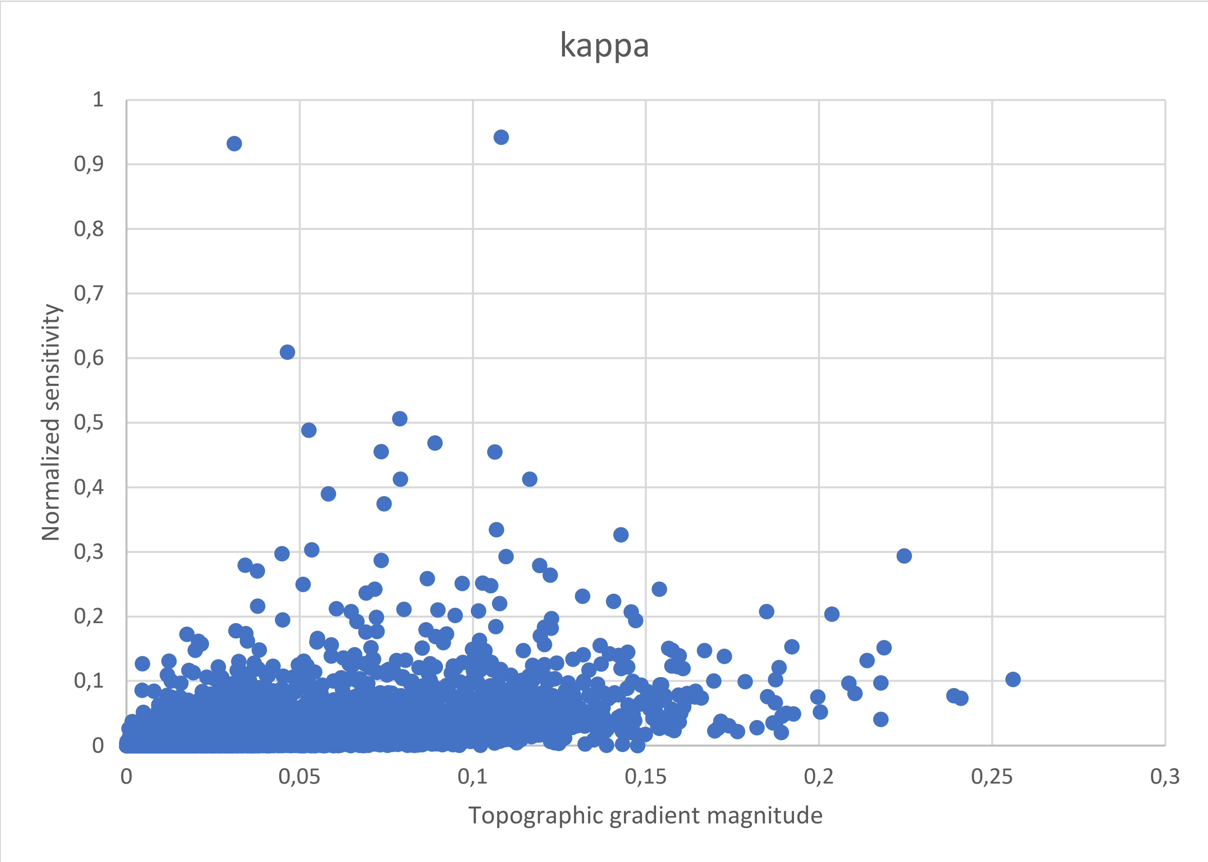
Re: Thanks for this suggestion. This is now explained in more detail in the revised version of the paper (lines 100-105).

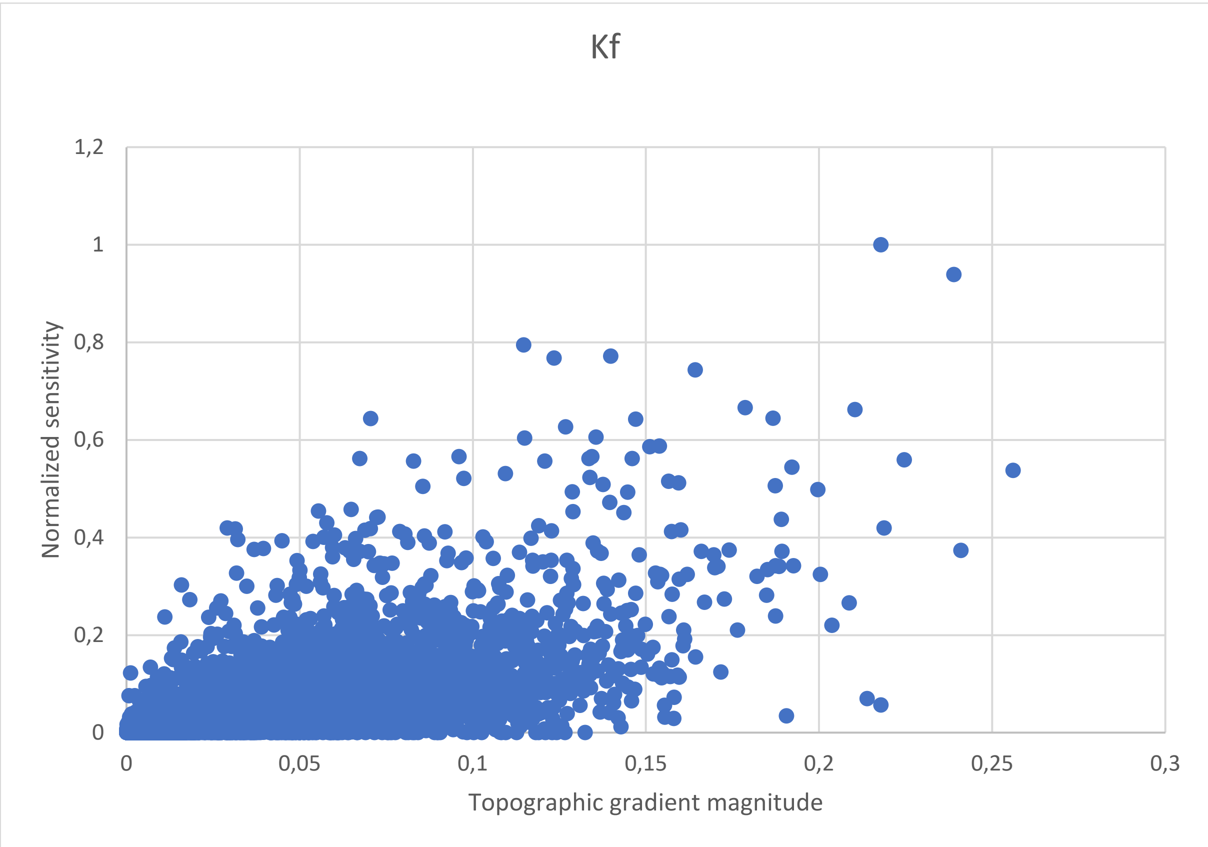
Section 4.1.1: It is possible that I missed it, but it is implicit that m=2 for the tests. If it is not stated in the methods, then it should be. If it is stated in the methods, maybe a short sentence here would be helpful to remind the reader.

Re: in our tests, the *m* coefficient is always equal to 0.5 but it can be changed by the user. It is true that we omitted to precise this point, and thanks to this remark this error has been corrected (lines 140-143).

Figure 1: Does sensitivity scale linearly with topographic gradient, for both the diffusivity coefficient and the erodability? It would be interesting to see a comparison with gradient as well as topography.

Re: We have made some tests in order to plot the sensitivity to the topographic gradient. The two following graphs show the scaled sensitivity with respect to the magnitude of the topographic gradient for the same model as on Figure 1. Although there seems to be a slight dependency for the erodibility (K_f) coefficient, the points are still very scattered, probably because there is also a strong dependency of the fluvial erosion rate on the drainage area. For the diffusion coefficient, it's even less clear, probably because the model is more sensitive to the topographic curvature than to its gradient.





Line 265 and more generally: what is the criteria for convergence of the adjoint method?

Re: convergence of the inverse problem is measured via the reduction of the misfit function. In this case it is a residual variation threshold (around 0.1%) and/or a number of iterations (30 to 50 depending on the cases). This is now explained in the revised version (lines 223-225).

Section 4.1.2: I find the rectangles of different coefficients a little arbitrary. Are they inspired by the sensitivity tests? Furthermore, would there be merit in doing a sort-of checker board test to explore what regions the model can resolve? I'm thinking of something like the classic test that is done in seismic tomography to explore in which regions the inversion can be interpreted.

Re: Thank you for this remark, now we have changed this test in order to incorporate a checkerboard instead of two simple rectangles, which were indeed completely arbitrary. Text has been modified between lines 299 and 328.

Line 287 to 289: The coefficients alpha and k are not chosen arbitrarily? It would be interesting to know the inversion sensitivity to alpha and k (in Equation 19).

Re: There is indeed some necessary tuning to ensure that the coefficients alpha and k are appropriate for the inversion. As in most inversion procedures, regularization plays a crucial role, and the choice of regularization parameters strongly influences the results. A systematic exploration of the effects of alpha and k lies beyond the scope of this paper; however, we agree that readers should have some indication of the expected behavior when varying these parameters. To address this, we have added a graphical table as supplementary material showing several tests that illustrate the impact of changing alpha and k on the residual map (i.e., the integrand of *J* as defined in Equation 18).

On this graphical table, one can observe that increasing alpha suppresses short-wavelength residual noise, while increasing k promotes the grouping of regions with similar altitudes into a smaller number of broader, nearly flat surfaces. See text lines 245-247 and new supplementary material.

Figure 5c: The residual does not evolve or converge as in the other tests. Something is odd here, I think.

Re: Thank you very much for pointing this out. There was indeed a problem on the way time-distributed controls (like the outgoing sediment flux) were implemented to compute the residual functional, that prevented the residual to decrease correctly. Fortunately, this bug impacted only this part of the model. We have corrected this in the code and provided a new inversion test in the revised version of the manuscript (lines 355-359).

Referee 2 (Stefan Hergarten):

Dear authors, thank you very much for the revisions and the additional explanations. However, the fundamental problem that div(c) is in fact not zero (point 3) is still there. Imagine an equilibrium topography with uniform uplift U in 1D. Then Kf*A^m*S^n = U = const and thus d/dx(Kf*A^m*S^n) = d/dx(Kf*Am^m)*S^n + Kf*A^m*d/dxS^n = 0. So your assumption div(c) = d/dx(Kf*A^m) = 0 would imply d/dx S = 0 here, which means that river profiles under uniform uplift would be straight. You introduced an additional argument why div(c) should be zero. This argument is basically that the erosion rate Kf*A^m*S^n can be written as the negative divergence of a sediment flux (-div(Qf)) only if div(c) = 0. It can indeed not be written this way with Qf as a local property, computed from A and S at the considered point. The sediment flux at a given point cannot be computed from A and S at this point for the stream power law because the sediment flux is the integral of the erosion rate over the upstream catchment. So the additional argument cannot enforce div(c) = 0 as we know that it is not zero.

The argument about the propagation of information instead of material does also not imply div(c) = 0. Finally, the problem is that the assumption div(c) = 0 is just at the center of the adjoint method, which is the new aspect introduced in this manuscript. I honestly have no idea how to fix this problem and I am afraid that it cannot be fixed. And I feel that we should not have a wrong assumption right in the middle of a new approach.

Best regards,

Stefan

Re : We thank the reviewer S. Hergarten for giving a very careful attention to our work. We understand why the approach taken by the reviewer sounds like the divergence of the velocity should not be null but it actually misses a very important point: the Stream Power Law (SPL): $E=K_fA^mS^n$ is an equation already obtained by the assumption that the velocity divergence term is null. This is true for both steady-state or transient cases. To prove our point and explain why there was a misunderstanding at the first place we added a new section in the Appendix A: Incompressibility of the Stream Power Law lines 552-571. This section demonstrates why considering the Stream Power Law as a transport equation implies that the velocity at which the topographic information is transported is divergence-free. Questioning this is equivalent to questioning the use of the SPL, which, although legitimate from a very physically fundamental point of view, is out of the scope of our study as we, like many other studies modelling the evolution of the landscape, use the SPL formulation to model fluvial erosion. In addition, divergence-free physically means that the natural decrease of velocity vectors upstream does not result in topographic “accumulation”.

In addition, as we stated in our response during the first round of reviews, there are a lot of papers (since the early 70’s) which explain why K_fA^m can be interpreted in this equation as a kinematic term, and we think that we can rely on these studies to use the same assumption.

Based on the fact that the SPL + diffusion equation has been very frequently interpreted as a diffusion-advection equation, while this point has never even been questioned elsewhere, we have chosen to add a detailed explanation for this assumption in appendix A at the end of the manuscript. We are however very grateful to S. Hergarten for these remarks, as they have allowed us to dive more deeply into this formulation and its understanding as a diffusion-advection equation.