Reply to the Reviewer:

Thanks for the careful reading and for your helpful comments and suggestions for our revised manuscript. Please find below point-by-point replies (in black) to your comments (which are reprinted in blue). To give you an overview of all the changes in the updated manuscript, we also provide a diff-document that highlights the changes between the initial submission and this re-submission.

The authors did only minor edits to the paper. I would encourage the authors to incorporate into the paper most of the arguments and numerical results used when replying to the reviewers. I think that results similar to the ones reported in the two Figures of their responses to the reviewers, and the related discussion, should be incorporated into the paper, as they address relevant issues. In general, I think that most of my concerns required to be addressed in the paper, not only in the response to the referee. As an example, the motivation of the Gaussianity assumption and its link to Tikhonov regularization provided in the response would be useful to the reader of this journal. I am fine with most of their replies to my questions. Here is a point that I would like to further discuss: ORIGINAL COMMENT: While I am convinced of the effectiveness and usefulness of the proposed approach, I am wondering whether the difference between the proposed approach and the traditional one has been overemphasized by taking a regularization (prior) for β that is too small. In fact, it seems to me that the data are overfitted when using the traditional approach (REF/CEM). It would be interesting to see what happens when γ and β are increased (one could do that using the L-curve rule, for the deterministic inversion to compute the MAP point). In general, I think that the parameters used for all the priors should be motivated. AUTHORS RESPONSE: It would be extremely difficult (likely impossible), to take into account the correlation structure embedded in (the covariance matrix of) the approximation errors via a regularization parameter, thus loosing valuable information. Furthermore, the mean of the approximation errors is not negligible in all cases, that is, disregarding the unknown rheological parameters induces systematic bias, which would be challenging (at best) to accommodate by changing γ and β. NEW COMMENT: I was not arguing that it is possible to account for the correlation structure in the approximation errors via a regularization parameter, nor that changing γ and β could prevent from systematic bias arising from disregarding unknown parameters. I was saying that it might be more fair, or at least informative, to increase the regularization for the traditional approach (REF/CEM) to avoid overfitting the data. For this reason I would recommend that the authors added a case where the regularization is significantly increased (e.g. scaling the prior covariance by a factor of 10 or 100), so that the data are not over-fitted. This might make the paper conclusion stronger, if it turns out that the amount of regularization required to avoid overfitting would make the results hardly useful.

Thank you for the clarification and recommendation. We have added a Supplementary Material to the manuscript, where we incorporated several numerical examples recommended by the reviewers. In the revised manuscript, we refer to these new studies in the Numerical Examples section (see Section 5). Specifically, via these new numerical examples we study the effect of

- using a true auxiliary parameter, a_{true}, which is a given function rather than a sample from the associated prior,
- using different (in terms of mean and variance) prior distributions for the basal sliding coefficient, and
- using a reguralization-type approach, where we tune/scale the parameters of the prior (as suggested above) and likelihood, to account for the approximation errors.