How do differences in interpreting seismic images affect estimates of geological slip rates? Responses to Comments of the Reviewers

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I thank the reviewers for their careful reading of the previous manuscript and their constructive suggestions. I have taken the comments and improve this manuscript. Please find responses to comments below.

Comments from Referees 1-1
I am wondering if there might be further papers discussing the total amount of slip and slip rates in the areas studied and discussed by the author. This would improve the comparison and give more insights about the interpretation bias.

Author's response 1-1
I agree that including more papers discussing the total amount of slip and slip rates in the studied area would provide additional insights into the manuscript's main focus–interpretation bias. Unfortunately, to be best of my knowledge, no other studies have published seismic images in the Central Himalaya.

Comments from Referees 1-2
I think the title can be simplified with the removal of the last sentence "An example of a shear fault-bend fold" since the author also state the second case is not a pure fault-bend fold but also characterized by fault-propagation folding.

Author's response 1-2
I will shorten the title, since I agree that the main issue is clearly communicated in the question

Comments from Referees 1-3
I suggest adding a couple of references, which are strictly related to the balancing methodologies used through time. Pease refer to the manuscript by Wang et al., 2018 (https://doi.org/10.1016/j.jsg.2017.11.014), where the authors well describe the various methodologies used for cross section balancing and where they propose an improvement of the Area Depth Strain Method. However, there is a substantial lack of methodological limits and problematics in literature, which are discussed in Carboni et al., 2019 (https://doi.org/10.1016/j.jsg.2018.10.011).

Author's response 1-3
The references pointed out to me for inclusion are valuable suggestions, and I have added them in the main text (**line 74 and line 77**).

Comments from Referees 1-4
Usually we obtain the amount of shortening and shortening rates throughout a "manual" step-by-step restoration of a balanced cross-section. In my experience, in the case of complex and quite shortened belts, it is not straightforward to obtain the true displacement by applying the methods of Suppe or the ADS method, thus a manual step-by-step restoration is needed. In that case there would also be the classical problem of number approximation, plus the

interpretation and restoration bias. I think it could be interesting also to explore this issue, and compare the results of the amount of shortening obtained by the manual restoration, with the one obtained by using the methods you already explored. I think that it would be enough to work with only the two case studies you have already analysed.

Author's response 1-4

Thank you for raising the point about exploring the issue of comparing the amounts of shortening obtained by manual restoration (line-length restoration) vs. by Suppe et al.'s or area-depth strain (ADS) methods.

It is true that Suppe et al.'s method would not be effective in more structurally complex cases. In some cases, restorations of line lengths or balanced cross sections could provide more detailed, exact estimates of displacements. However, not even restorations of line lengths are always possible, especially when data quality cannot provide sufficient details about features, such as well-defined paired horizons in hanging walls and foot walls.

Several good comparisons between ADS analysis and bed length restorations are presented in Groshong et al. (2012). One can notice that the structures studied by both methods are based on laboratory sand models, figures drawn based on theoretical models, or high-quality seismic images. All these three cases allowed better control of structures, which is not possible in my study.

In the case of the Bardibas thrust in Nepal (Almeida et al., 2018), the quality of the seismic image does not allow us to interpret in detail (as mentioned in the main text, lines 140-145). Lack of details would affect total slip estimation even if ADS analysis was applied. Conducting line lengths restorations, for which more details of given structures are required, would be more challenging, and highly likely lead to overinterpretation.

All of the above point to why I chose Suppe et al.'s method here, or to how to choose a suitable method, and why my result differs from Almeida et al.'s result.

The advantage of Suppe et al.'s method is the relations between geometric features are clear; thus it can be applied even when few variables are known. That is, Suppe et al.'s method can predict the total slip based on what the image in Almeida et al. can provide.

Of course, this method has inherited limitations (line 73): It is unreliable when there is significant sub-resolution deformation. To validate Suppe et al.'s method, here I demonstrate that the method is primarily applicable in an analogous case (from Le Béon et al., 2019). I assume that both Le Béon et al.'s and Almeida et al.'s geological structures studied are in their initial stages (i.e., younger and less complex structures) and are nearly model-like individual structures. Thus, in the case with a relatively simple structure located at the foremost part of two frontal fold-and-thrust belts, I suggest the simplification made in Suppe et al.'s study is acceptable.

Le Béon et al. provide higher image quality, and apply ADS appropriately. Thus, ADS performs nearly as well as Suppe et al.'s kinematic model, and their result agrees with mine.

In contrast to Le Béon et al.'s caes, the main cause of the difference between mine and Almeida et al.'s result is the choice of method given the data quality and the application of the method.

Almeida et al., I believe, aimed to apply the ADS method. However, their application did not agree with how the method had been developed (line 142). In addition, their data quality could not to support ADS application (line 140). Also, Almeida et al. mentioned that they applied area-relief calculation following Lavé and Avouac (2000). However, the area-relief calculation used by Lavé and Avouac (2000) was not designed for estimating the total slip from seismic images: it was developed for calculations based on flights of fluvial terraces, which have different resolutions, scales, and considerations. What is more, the depth of décollement, required for this calculation, was subjectively determined by Almeida et al., and resulted in an unnoticed large bias.

In short, the differences in interpretations shown in this study are not originated from applying different methods. This further implies that choosing a method compatible with data quality and applying it appropriately can help us avoid subjective uncertainty and thus minimize differences in interpretations among interpreters. In this short communication submission, I would like to focus on a more generalized but less discussed issue: effects on further application of minor differences in interpretations, instead of on how different interpretations could be when various models are selected (Line 38).

References

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Suppe, J., Connors, C. D., and Zhang, Y.: Shear fault-bend folding, AAPG Mem., 303–323, https://doi.org/10.1306/m82813c17, 2004.

Comments from Referees 2-1
In section 3.11 l113 make sure on how you computed the standard deviation/uncertainty (is it based on few measurements or PDFs?)

Author's response 2-1
In section 3.11, I have added the following explanation regarding what the computation of uncertainty is based on: The insertion is: distributed according to the assigned PDFs (**line 116**).

Comments from Referees 2-2
Section 3.2 l139: can you precise what do you mean by ‘claimed to be decided based

Author's response 2-2
In section 3.2, I have modified the sentence in line 139 to: The décollement depth was stated by Almeida et al. (2018) to have been based on Suppe et al.’s (2004) shear fault-bend fold model together with a decided ramp location) (**line 141**).