Reply on RC2
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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 case, 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 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


