Dear Editor,

We would like to thank the reviewers Oliver B. Duffy and Zoe Mildon for their very helpful and insightful comments. Please find attached our revised paper and below a summary of our responses to their comments and suggestions (in red). We have addressed all the reviewers concerns and made the necessary changes.

In addition to addressing the reviewers comments we have added some additional text and a figure in the discussion section. In the original version we explained that we are considering a limited range of the geometries that can be predicted from this model but did not return to this topic in the discussion. We have included new text in the discussion and an accompanying figure (Fig. 7) that emphasises that the model has the potential to account for a wider range of geometries than those explicitly described in the manuscript. This modification does not change the model or any results but should broaden the impact and appeal of the manuscript.

Where we mention line number below, we refer to the revised version with the markup option set to "simple mark-up".

If you have any additional questions/comments, please do not hesitate to contact me on stratos.delogkos@ucd.ie.

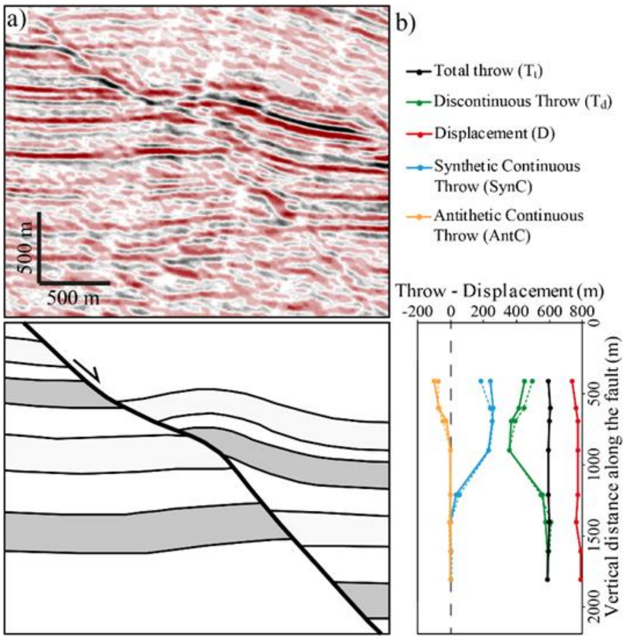
Yours sincerely,
Efstratios Delogkos, Muhammad Mudasar Saqab, John J. Walsh, Vincent Roche and Conrad Childs.

Oliver B. Duffy - Reviewer 1

1) I suggest the authors add extra sketches (or modify existing sketches in earlier figures) to support section 4.2. as this discussion is relatively complex and difficult to digest.
Taking into consideration both this and the following comment, this discussion has been modified and references to existing figures have been added. Hopefully this makes it easier to follow.

2) In section 4.2. I was finding myself getting a little confused by terminology (e.g. lines 209-210). The use of the term normal drag when referring to 'monoclines between different stratigraphic sequences or between different fault segments' needs clarifying. To me, what is described in lines 209-210 and shown in Ferrill et al 2017 (their figure 6) is more reminiscent of a 'fault-propagation fold' (e.g. Coleman et al., 2019), but this may be me being confused by the scale or the description. Either way, the terminology can probably be clarified or shown in the sketch as mentioned in Point 1 above.
Normal drag, which is defined as the folding adjacent to a fault such that a marker is convex towards the slip direction (see Peacock et al., 2000), is considered to have either a pre-cursory (i.e. fault-propagation fold) or frictional origin. The text has been modified to clarify that in this case the authors refer to the pre-cursory (i.e. fault-propagation fold) origin of normal drag (line 235).

3) Perhaps rotate Fig 5b by 90 degrees and put it to the right of 5a so that there is more of a visual link between the two parts.
Thanks for the suggestion. Figure 5 has been modified accordingly.



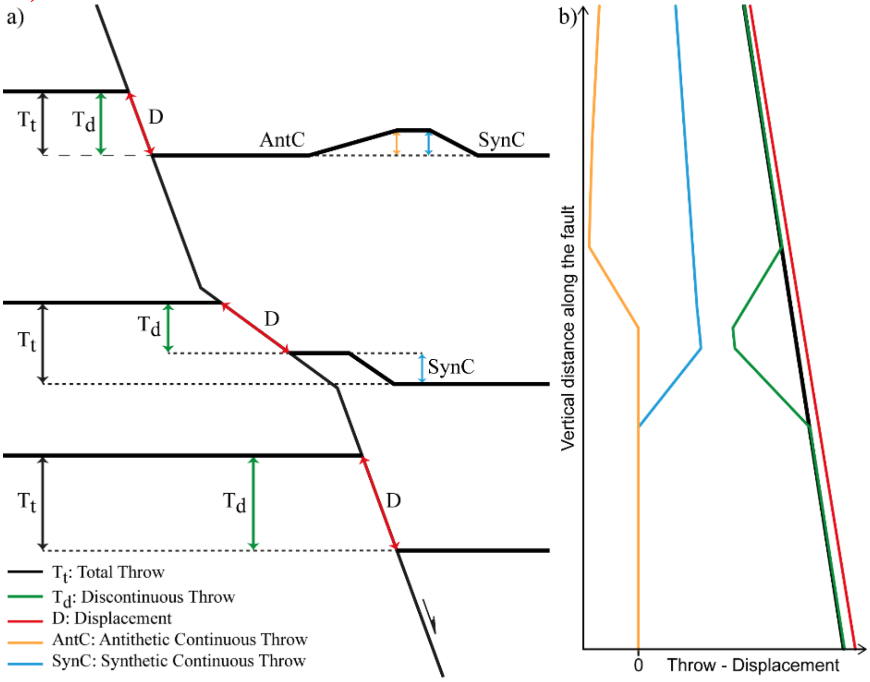
4) Possible references that could be considered for citation:
Spahić, D., Grasemann, B. and Exner, U., 2013. Identifying fault segments from 3D fault drag analysis (Vienna Basin, Austria). Journal of Structural Geology, 55, pp.182-195.
Long, J.J. and Imber, J., 2010. Geometrically coherent continuous deformation in the volume surrounding a seismically imaged normal fault-array. Journal of Structural Geology, 32(2), pp.222-234.
Thanks for bringing these references to our attention.

Zoe Mildon - Reviewer 2

1) Initially I was confused reading the manuscript because the authors refer to 'along-fault bends', and yet all figures and later discussion refers to concave or convex bends down dip of the fault (contrast this with Faure Walker et al., 2009; Iezzi et al., 2018 who discuss fault bends as changes in strike along the fault scarp at the surface). My interpretation is that the authors are actually discussing "down dip fault bends", and if this is correct then it should be clarified in the title and abstract of the paper.
Yes, the interpretation that we are actually discussing "down-dip fault bends" is correct. Actually, the title of the manuscript refers to "fault-bend folding" and, in literature, as a rule of thumb, "fault-bend folding" refers to the mechanism of folding when the layered rocks fold in response to slip over a down-dip fault bend (Suppe, 1983; Fossen, 2016). However, we understand the cause of confusion and, therefore, we further clarify in the manuscript (including the abstract) that we are discussing about "down-dip fault bends".

2) The assumption of "constant along fault displacement" is clearly stated, but I wonder what the implications of this assumption being incorrect would mean for the conclusions of the paper? For example, Wesnousky, 2008 presents a compilation of historical earthquake ruptures, including normal faulting earthquakes, that show that along a fault the coseismic displacement (and thereby probably the long term displacement) is highly variable. However, there may be confusion on my part given my point in the paragraph above – I think the authors are referring to "constant down-dip fault displacement" rather than along strike displacement?
It is absolutely correct that fault displacements vary both along strike and down dip with a tendency of the maximum displacement to be located towards the centre of an ideally planar fault surface and its systematic gradual decrease towards the fault tip-lines (e.g. Barnett et al., 1987). In this manuscript, the assumption of "constant along fault displacement" is only a simplification that implicitly excludes displacement variations due to fault propagation related folding and only concentrates on fault-bend folding. Of course, fault displacement variations are expected (e.g. Walsh et al., 1988) and this doesn't affect the conclusions for this paper. To illustrate that our deformation algorithm is also valid for the case of variable displacement, the figure bellow is the equivalent of Figure 2 but, in this case, the displacement systematically decreases upwards with a constant gradient

that is not affected by the fault bends. The text has been modified accordingly to clarify this assumption (lines 80-81).



3) Line 37 – "normal faults are often approximately planar" can you provide a reference for this? There are many active normal faults in Italy, Greece and Basin and Range that are not planar (although obviously it depends on the length scale of observation).
Yes, faults are rarely planar. However, in this occasion, the authors refer to the normal faults to be approximately planar in comparison to the ramp-flat geometries in thrust systems. The text has been modified accordingly for clarity (lines 37-38).

4) Line 43 – others have investigated strain partitioning and variations in throw along-strike of non-planar normal faults e.g. Faure Walker et al., 2009; Iezzi et al., 2018 – and discussed the implications particularly for seismic hazard.
Thanks for bringing these references to our attention.

5) Line 239 – 243 – this is very similar to the conclusion in Iezzi et al., 2018, wherein they looked at the spread of data in the Wells and Coppersmith 1994 data set. Could the observations/models presented in this paper also explain the scatter in fault scaling relationships? Or is this less applicable given the different scale of observation?
Thanks for highlighting this aspect. The presented model can be applied to a wide range of scales, with fault lengths and displacements ranging from a few centimetres up to hundreds of meters (i.e. Figures 5 and 6) and, therefore, we believe that fault throw variations due to down dip fault bend geometries can also provide an explanation in the scatter in fault scaling relationships (e.g. Wells & Coppersmith, 1994). New text has been added to highlight this aspect (lines 278-284).

6) Figure 3 – I mostly like this figure, but I am curious about the dots plotted on the graph that refer to the examples presented in the later figures. I'm assuming the dots are plotted according to the dips of the lower and upper fault segment – and then a percentage can be read off the graph. How do these predicted percentages compare to the actual measured percentages from the seismics/outcrops? This information/analysis seems to be missing from the paper. I think this would be a valuable addition to demonstrate that your simple (but effective!) geometric model works.
The dots in Figure 3 are plotted according to the dips of the lower and upper fault segments to highlight which areas in this plot represent realistic fault-bend geometries. Providing a quantitative comparison between the predicted values and the actual measures, Figure 5b includes the predicted values of the throw components (dashed lines) together with the measured ones (continuous lines).

References

Barnett, J.A., Mortimer, J., Rippon, J.H., Walsh, J.J. and Watterson, J. Displacement geometry in the volume containing a single normal fault. AAPG Bulletin, 71(8), pp.925-937, 1987.

Fossen, H.: Structural geology. Cambridge University Press, 2016.

Peacock, D.C.P., Knipe, R.J. and Sanderson, D.J. Glossary of normal faults. Journal of Structural Geology, 22(3), pp.291-305, 2000.

Suppe, J.: Geometry and kinematics of fault-bend folding, American Journal of science, 283(7), pp.684-721, 1983.

Walsh, J.J. and Watterson, J. Analysis of the relationship between displacements and dimensions of faults. Journal of Structural geology, 10(3), pp.239-247, 1988.

Wells, D.L. and Coppersmith, K.J. New empirical relationships among magnitude, rupture length, rupture width, rupture area, and surface displacement. Bulletin of the seismological Society of America, 84(4), pp.974-1002, 1994.