Review Report

Frings et al., Constraining the 3-D Geometry of Fold-Thrust Belts Using Section Balancing vs. 3-D Interpolative Structural and Probabilistic Modeling, TEKTONIKA, 2023.

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1st Round of Revisions

Decision Letter

Dear authors,

We have now received two reviews of your paper, ‘Constraining the 3-D geometry of fold-thrust belts using section balancing vs. 3-D interpolative structural and probabilistic modeling’, which along with independent review by Associate Editors Adam Forte and Laura Federico, have allowed us to reach a decision. Based on evaluation by the AEs, we have elected for a decision of Request Revisions, but we emphasize in that revision process, you and your coauthors should try to fully address the core concerns of Reviewer 1.

The two reviews plus additional comments by the AEs are pasted below this letter. Reviewer 1 has also uploaded two .doc files with comments on the manuscript and figures. You'll find these files in your manuscript's record on the Tektonika web site.

Specifically, Reviewer 1 highlights in their review that significantly more detail is needed with respect to how the 2D Move cross-sections are integrated with the 3-D GemPy models and how exactly the iterative process works. They provide a variety of places where additional detail would benefit readers. We largely agree on these points, and especially on the necessity of replacing Figure 4 with a more detailed schematic workflow, i.e., a flow chart of something similar to provide tangible examples of the iterative process. Similarly, we agree that given the focus of the manuscript ultimately is the discussion of this methodology, less detail could be provided on the stratigraphy of the example study region in favor of providing more detail on how the 2D and 3D integration and iterative process works in practice. In addition to the comments from the two reviewers, we have a few minor comments on the manuscript which we include below.

We hope you will be able to submit your revision within two months, by January 23rd. This is a non-strict deadline and we will not close the evaluation process at the due date. But we recommend not delaying too much the revision process. If needed, please keep us informed about your timetable.

When uploading your revision provide us with a full rebuttal letter addressing all reviewer's comments and also a second manuscript version with all changes clearly outlined.

Best regards,

Associate editors Laura Federico, Adam Forte

Executive Editor Robin Lacassin

AE additional comments on manuscript:

L293 – What was the resolution of the SRTM dataset that you used?

Figure 1 – This map needs some a coordinate grid and could also benefit from a small location map, i.e., where in the broader Alpine system is this study area as not all readers will be as familiar with the local geology or geography.

L776-779 – Following from the comment on the topography, here you indicate that you're using topography from Google Earth? Is this accurate or are you using SRTM data directly? If you're using the underlying (relatively coarse) topography from Google Earth, is the uncertainty really that small? How did you evaluate these uncertainty values?

Comments by Reviewer 1 (Nate Eichelberger)

Frings et al. present an novel approach to quantifying uncertainties in subsurface structural interpretations. The paper demonstrates that combining 2-D balanced cross sections, 3-D implicit models, and probabilistic models opens the door to understanding how uncertainties vary across a geologic model based on the uncertainties in the input data. This in turn gives insight into alternative interpretations that are permissible based the available datasets. Importantly, the probabilistic and 3-D modeling aspects of the paper are carried out using open-source software that will make the method much more accessible to the community. Understanding that range of interpretations is a significant advancement in sub-surface geologic modeling as it is a huge blind spot in most established interpretation methods. However, the paper still needs fairly extensive revisions in order to make the workflow clearer to readers, correct issues with the 2-D balanced cross sections, and improve the organization of the article. Once these changes are made, I believe the paper will be a strong contribution towards providing an accessible method for assessing structural interpretation uncertainty.

Firstly, while the text is fairly detailed about the methods used, the actual explanation of the workflow details is somewhat limited. As I point out below in my specific comments, improving the workflow figure (Figure 4) with specific steps would help. I also think that the paper would benefit from running through the workflow on a very simple, artificial structural example where the outcome is known and the interpretation is inherently balanced and geologically consistent. The example could then show the effects of removing key swaths of “data” to show how the probability and entropy fields change. These aspects will be new to most geologists and as such, needs more support in the text on how we should view those parameters in geologic context. In the current version, there isn’t quite enough explanation that links the probabilistic outputs to the actual geology being interpreted. One way to help address this would be including some geologic annotation on the probability and entropy field cross sections, I have some detail suggestions below and in the comments in the manuscript document.

On a related note, it isn’t always clear how aspects of the 2-D model are being passed into the 3-d implicit model (see Line 531). This seems to make the implicit model at least partially dependent on the 2-D model which is subject to the interpretation biases the method is trying to overcome. A more detailed flow chart would help clarify this as well as illustrating where the 2-D interpretation is being used to constrain the 3-D model.

Secondly, the 2-D cross sections have balancing errors that may impact the interpreted fault geometries, especially at the Kirchbichl thrust. See points 5 and 6 below.

Finally, the paper needs to be revised to more consistently link the probabilistic model results to the interpreted geology. At the moment, I'm not clear how the proposed method is iterative (Line 25, Figure 4) as I don't see any discussion/demonstration of how the probability results are used to revise the 2-D interpretation or improve the 3-D implicit model fit to the data.

I do think the paper is highly original and I found the proposed workflow to be very promising. As I stated above, the paper needs significant revisions to make the method clearer and really deliver on showing how the probabilistic aspects can be concretely applied to the geologic interpretation. I do look forward to seeing this improved and strongly encourage the authors to re-submit. Please see my comments in the attached review document and annotated manuscript.

Regards, Nate Eichelberger

The reviewer has also commented on a .doc manuscript's file.

Comments by Reviewer 2 (F. Robledo Carvajal)

This manuscript is easy to read, and readers can easily extract the key points they are trying to communicate. They make the point clear. The research question is essential regarding the new challenges linked with geostorage and Energy Transition. Nowadays, much research focuses on structural interpretation uncertainty, and this contribution gives a new perspective and methodology to handle it (uncertainty visualisation and quantification). The last makes this contribution new and original. The methodologies involved are clearly outlined. Results support their interpretation and conclusions. The abstract provides a good storyline for the research done, reflecting the contents of this paper.

More detailed comments, questions and suggestions:

Figure 2: It isn't clear what this figure's relevance is in the whole paper storyline. Is it essential? They show that bedding orientation change from East to West. However, this is also shown in cross-sections built with the new data and Figure 1 (map).

3.2.1. Cross-section interpretation and balancing

They said they balanced some sections by hand. What is the difference when using MOVE? What MOVE algorithms did they use to balance the other sections? How different are these two methodologies? Does this difference affect the result or add different uncertainty to the process? This difference was considered when inserting errors in GemPy?

3.2.2. Geometric steady-state modelling with GemPy

It can be relevant to add the limitations when employing co-kriging interpolation to build surfaces.

5.2.1 Data uncertainty

How was calculated the estimated error? How much contributes each variable to the total error? Do you consider any error when restoring?

Figure 11: Add letters (a, b, c).

Results: Sections restored by hand are not included.

Last suggestion: The limitations of the methodologies employed may be explained better or highlighted. What variables could not be part of the analysis but play important role? For example, stratigraphic complexity and simplifications when balancing.

At present, I think this is a significant contribution and will have a significant impact on uncertainty assessment.

Kind Regards

Authors' Reply to Reviewers

Dear Editors, dear reviewers

Thank you very much for the appreciation of the manuscript and for providing us with constructive and helpful comments. We apologize that the revisions have taken longer than expected, partly because the manuscript needed substantial restructuring, partly because timing collided with the final phase of the PhD project of K.F..

Both reviewers point out the strength of the Bayesian modeling approach, while pointing to weaknesses in the balancing part, and in particular the iterative workflow. We agree with reviewers and editors that the novelty of the manuscript lies primarily in the Bayesian approach and uncertainty estimates, which we now highlight more. Furthermore, we have included a more detailed description of the balancing approach, as requested by the reviewers.

Based on your input, the manuscript has been extensively modified. Please find below answers to all comments by reviewers and editors in green.

Comments by editors:

We have now received two reviews of your paper, 'Constraining the 3-D geometry of fold-thrust belts using section balancing vs. 3-D interpolative structural and probabilistic modeling', which along with independent review by Associate Editors Adam Forte and Laura Federico, have allowed us to reach a decision. Based on evaluation by the AEs, we have elected for a decision of Request Revisions, but we emphasize in that revision process, you and your coauthors should try to fully address the core concerns of Reviewer 1.

The two reviews plus additional comments by the AEs are pasted below this letter. Reviewer 1 has also uploaded two .doc files with comments on the manuscript and figures. You'll find these files in your manuscript's record on the Tektonika web site.

⇨ Thank you very much for the constructive criticism, which is very much appreciated. All concerns have been addressed in the updated version

Specifically, Reviewer 1 highlights in their review that significantly more detail is needed with respect to how the 2D Move cross-sections are integrated with the 3-D GemPy models and how exactly the iterative process works. They provide a variety of places where additional detail would benefit readers. We largely agree on these points, and especially on the necessity of replacing Figure 4 with a more detailed schematic workflow, i.e., a flow chart of something similar to provide tangible examples of the iterative process. Similarly, we agree that given the focus of the manuscript ultimately is the discussion of this methodology, less detail could be provided on the stratigraphy of the example study region in favor of providing more detail on how the 2D and 3D integration and iterative process works in practice. In addition to the comments from the two reviewers, we have a few minor comments on the manuscript which we include below.

- ⇨ We agree that the iterative workflow was too immature. During revising the manuscript, we realized that a full iterative workflow as requested by the reviewers would require a substantial amount of additional work, which would be a separate manuscript. This manuscript is a first step towards this approach. The new focus is to first present the revised stratigraphy, then the new kinematic analyses, and then show uncertainties.

- ⇨ We would like to make the point instead that detailed stratigraphy has been a main aspect to from the base of the Bayesian model. It was particularly challenging in the region, as facies changes quickly laterally. This is often the case, not only in the alpine Carpathian foreland, but also in other orogens in the world. However, to spare the reader the lengthy description of local stratigraphy, we moved parts of this section to a supplement, and now only present a brief version in the main text, stressing the importance of sorting out stratigraphy first.

We hope you will be able to submit your revision within two months, by January 23rd. This is a non-strict deadline and we will not close the evaluation process at the due date. But we recommend not delaying too much the revision process. If needed, please keep us informed about your timetable.

When uploading your revision provide us with a full rebuttal letter addressing all reviewer's comments and also a second manuscript version with all changes clearly outlined.

Best regards,

Associate editors Laura Federico, Adam Forte
Executive Editor Robin Lacassin

Reviewer 1 (N. Eichelberger) comments (see also .doc files uploaded by the reviewer:

Frings et al. present an novel approach to quantifying uncertainties in subsurface structural interpretations. The paper demonstrates that combining 2-D balanced cross sections, 3-D implicit models, and probabilistic models opens the door to understanding how uncertainties vary across a geologic model based on the uncertainties in the input data. This in turn gives insight into alternative interpretations that are permissible based the available datasets. Importantly, the probabilistic and 3-D modeling aspects of the paper are carried out using open-source software that will make the method much more accessible to the community. Understanding that range of interpretations is a significant advancement in sub-surface geologic modeling as it is a huge blind spot in most established interpretation methods. However, the paper still needs fairly extensive revisions in order to make the workflow clearer to readers, correct issues with the 2-D balanced cross sections, and improve the organization of the article. Once these changes are made, I believe the paper will be a strong contribution towards providing an accessible method for assessing structural interpretation uncertainty.

We thank reviewer Nate Eichelberger for the appreciation of the manuscript, as well as his constructive comments. Please find replies to every comment, and how we addressed them below.

Firstly, while the text is fairly detailed about the methods used, the actual explanation of the workflow details is somewhat limited. As I point out below in my specific comments, improving the workflow figure (Figure 4) with specific steps would help. I also think that the paper would benefit from running through the workflow on a very simple, artificial structural example where the outcome is known and the interpretation is inherently balanced and geologically consistent. The example could then show the effects of removing key swaths of "data" to show how the probability and entropy fields change. These aspects will be new to most geologists and as such, needs more support in the text on how we should view those parameters in geologic context. In the current version, there isn't quite enough explanation that links the probabilistic outputs to the actual geology being interpreted. One way to help address this would be including some geologic annotation on the probability and entropy field cross sections, I have some detail suggestions below and in the comments in the manuscript document.

In the new version of the manuscript we do not use the same workflow anymore (see also above). We agree that a more detailed description of the modeling approach using an artificial example is beneficial. Such a study has just recently been published in a region close to our study area (Brisson et al. 2023, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4274454). For details on the modeling approach we now refer to this study in our new version of the manuscript.

On a related note, it isn't always clear how aspects of the 2-D model are being passed into the 3-d implicit model (see Line 531). This seems to make the implicit model at least partially dependent on the 2-D model which is subject to the interpretation biases the method is trying to overcome. A more detailed flow chart would help clarify this as well as illustrating where the 2-D interpretation is being used to constrain the 3-D model.

A main issue of the previous version of the manuscript was the incomplete link between the two approaches (see also above). In this manuscript we now pave towards establishing the link by a comparison of the to modeling approaches.

Secondly, the 2-D cross sections have balancing errors that may impact the interpreted fault geometries, especially at the Kirchbichl thrust. See points 5 and 6 below.

This is an important point. In the Subalpine Molasse we know that we have a break-back sequence of thrusting. The classic balancing approach assumes a forward breaking sequence. Including back-breaking results in apparent balancing errors. We have added a respective statement in the balancing section, as well as a note when describing the figures.

Finally, the paper needs to be revised to more consistently link the probabilistic model results to the interpreted geology. At the moment, I'm not clear how the proposed method is iterative (Line 25, Figure 4) as I don't see any discussion/demonstration of how the probability results are used to revise the 2-D interpretation or improve the 3-D implicit model fit to the data.

See above concerning the iteration. In the new version of the manuscript we have added a more detailed section on the comparison between the two approaches.

I do think the paper is highly original and I found the proposed workflow to be very promising. As I stated above, the paper needs significant revisions to make the method clearer and really deliver on showing how the probabilistic aspects can be concretely applied to the geologic interpretation. I do look forward to seeing this improved and strongly encourage the authors to re-submit. Please see my comments in the attached review document and annotated manuscript.

Regards, Nate Eichelberger

Specific notes:

1. Stratigraphy background is overly detailed, revisit and include details specifically relevant to the paper's objectives. For example, lots of lithologic details may be unnecessary unless they pertain to mechanical stratigraphy aspects that are relevant to later parts of the paper.

One point why stratigraphy is so detailed is that it was necessary to revise the stratigraphy completely. This can be moved to a digital supplement, but should not be deleted. It will be interesting for people working on a much more local scale.

2. What about the role of uncertainties in stratigraphic thickness? As you state, stratigraphic thickness is a major input into constraining cross section geometry. Accordingly, uncertainties in stratigraphy can have a large impact on the interpreted structural geometry and by extension, shortening estimates.

Good point, and it has been one major point why we chose the Subalpine Molasse as suitable study area. Stratigraphic thicknesses are known exceptionally well, due to multiple previous studies, limiting this uncertainty to a minimum. We have added a respective statement in section 4.1 (new version of the manuscript).

3. Figure 4: This figure is too vague to really communicate what is going on with the work flow. I think a flow chart is a good idea but it needs to be more specific about the steps that are taken with each approach (2-D, 3-D, Probabilistic), what data is used, and how the results from each approach are used to inform the others. I'd suggest re-drafting and following a more traditional box-and-arrow type layout.

See above – we have deleted Fig. 4 and refocused the manuscript.

4. Line 317: When discussing uncertainty in balanced cross sections, the role of penetrative strain (layer parallel strain) can greatly affect the observed bed lengths during deformation. This can cause line length balanced cross sections to significantly over or underestimate shortening. Layer parallel strain can be quite significant an complex in folds with large extensional strains across the outer beds

and large contractional strains at the fold core. This merits more consideration here as the purpose of the paper to estimate uncertainties associated with cross sections.

Penetrative strain is important, and we included a respective paragraph in section 4.2.1 (new version of the manuscript). While in general it can be responsible for substantial amounts of shortening, in our study area, penetrative strain seems to be of minor importance, as it is a fold-thrust belt dominated by siliciclastic rocks that in our study area have not experienced substantial re-heating

Comment 1 (Introduction) "It would be helpful if you could provide a brief overview of how the method works (basic theory, data incorporated, steps taken, etc) to help orient the reader at the beginning of the paper. You could also reduce some of the background literature review in order to keep the Introduction to an appropriate length."

We have revised this section. Background literature was reduced in the introduction and now forms a separate section, including references to papers detailing the applied method. Furthermore, we have completely revised the geological introduction and moved large parts of the description of the stratigraphy into the supplementary material. We find it important to keep this information there, as otherwise our stratigraphic column cannot be used by people interested in the local geology.

Map (Figure 1) needs an inset geographic location map to show readers where the study is located regionally
Done

Caption should define "OSM" and "OMM"
Done

I can't tell from Figure 10a vs 10b which data points are being introduced from the seismic section. It is also hard to tell what the effect is on the new computed fold geometry. This figure really needs to highlight that change more. Perhaps include a figure with the seismic section to show the difference between the GemPy and interpreted cross section geometries?

We have specified the changes in the text and the figure caption. With GemPy is it so far not possible to highlight special data points and additionally, the view on data points gets blocked by the modeled plains. Post-processing with a differnt image procesing tool might be possible but more inaccurate. We suggest that the specifications in the text are the better choice.

The maps (especially the probabilistic maps) should probably be much larger and/or even shown in a separate figure so we can see larger versions. With the current format, I can't really see much detail in Figure 12g. The cross sections could be shown in separate figure.
Done

In general, I think it would help to have some kind of geographic or geologic reference on both the cross sections and maps in order to spatially orient the readers (e.g. topographic profile, data point locations, mapped surface contacts, etc). Right now, its kind of hard to tell how each of the probability plots fit together into what one would recognize as a geologic map or cross section. Right now, you have to look at each piece separately to try to mentally stack them together.

Thank you, we see the point. Problematically, topogroaphy is just out of the box, data points are in 3-D out of the plain and projected points would not help as structures are not cylindric. We considered random markers at fixed x-y positions, but decided these would not solve the problem. However, the entropy plot can also be regarded as a stacked model of the three different sub-models and consequently helps orientation.

All other comments in the annotated manuscript have been revised as suggested. We'd like to reply here to one particularly important point: "It's not clear to me where or how much of the data from the interpreted cross section is being incorporated into the implicit model. Based on my reading, it seems like this makes the implicit model much less independent of the interpreted cross section. Couldn't this basically allow someone to create an implicit model that just reproduces their interpreted cross section with all of the inherent biases?", and later "Circling back to my comment on line 531, the 3-D model and interpreted 2-D cross sections are not completely independent as "data" from the 2-D sections are incorporated into the 3-D model. The text needs to provide more details on this aspect of the 3-D model generation. This may not be an issue where fold geometries are well studied (such as the case here) but may certainly be a limitation that should be recognized in areas with greater uncertainty."

The performed uncertainty modeling focuses very much on the data itself, and within the study area we can highlight regions of high uncertainty while the general structural framework is well established and allows only for little conceptual bias. In other regions, where data coverage is worse, conceptual bias may be a problem, as also noted by the reviewer. A main point we wanted to achieve with this study is linking uncertainty modeling with kinematic restorations. Kinematic restorations are highly susceptible to bias introduced by the interpreter, as also discussed in the manuscript. This is hard to overcome, but GemPy modeling may show areas where kinematic models are most vulnerable to such errors. In the new version of the manuscript, we added a respective section in the discussion on this.

Reviewer 2 (F. Robledo Carvajal) comments:

This manuscript is easy to read, and readers can easily extract the key points they are trying to communicate. They make the point clear. The research question is essential regarding the new challenges linked with geostorage and Energy Transition. Nowadays, much research focuses on structural interpretation uncertainty, and this contribution gives a new perspective and methodology to handle it (uncertainty visualisation and quantification). The last makes this contribution new and original. The methodologies involved are clearly outlined. Results support their interpretation and conclusions. The abstract provides a good storyline for the research done, reflecting the contents of this paper.

We thank the reviewer for the appreciation of the manuscript and constructive criticism.

More detailed comments, questions and suggestions:
Figure 2: It isn't clear what this figure's relevance is in the whole paper storyline. Is it essential? They show that bedding orientation change from East to West. However, this is also shown in cross-sections built with the new data and Figure 1 (map).

It is true that all models include these data. However, we keep the visualization of the new raw data to illustrate the unbiased base for conceptual modeling.

3.2.1. Cross-section interpretation and balancing
They said they balanced some sections by hand. What is the difference when using MOVE? What MOVE algorithms did they use to balance the other sections? How different are the Ese two methodologies? Does this difference affect the result or add different uncertainty to the process? This difference was considered when inserting errors in GemPy?

Using the kinematic restoration tools of the MOVE software it is not possible to balance overturned structures. Therefore, we had to do it manually, using standard approaches. The goal of this manuscript was to compare a model with geometric uncertainty with a kinematic model. Quantifying uncertainties in balanced cross sections remains a challenge. We have clarified this in the introduction, citing the respective references focusing on uncertainties in balanced sections (Judge & Allmendinger, Witte & Oncken). As of now, we cannot fully link GemPy to kinematic restorations, as previously thought. Our study paves towards this link, raising a hope that it may become possible in the near future. During the revision of the manuscript we found that possibly software like PyNoddy may be more suitable than MOVE to achieve this goal.

3.2.2. Geometric steady-state modelling with GemPy
It can be relevant to add the limitations when employing co-kriging interpolation to build surfaces.

The strength of GemPy is that it allows for uncertainty modeling of a model using a data-driven approach. A commonly raised limitation of cokriging is that data have to be normally distributed. We consider this a valid assumption, as we use structural data from field measurements. We assess the uncertainty of this data in section 4.3.2 new version of the manuscript, formerly 3.2.3

5.2.1 Data uncertainty
How was calculated the estimated error? How much contributes each variable to the total error? Do you consider any error when restoring?

Good point – the error was not calculated but estimated based on the fact that topographic data exists at high resolution. For our km-scale models, positioning errors of data must be negligible. We have revised the respective sentences and deleted the semi-quantitative estimate of "50 m".

Figure 11: Add letters (a, b, c).

Done and caption adjusted accordingly.

Results: Sections restored by hand are not included.

The presented sections are the result of a combination of MOVE & manual restorations. We have clarified this in the method section

Last suggestion: The limitations of the methodologies employed may be explained better or highlighted. What variables could not be part of the analysis but play important role? For example, stratigraphic complexity and simplifications when balancing.

We have added an assessment of stratigraphic complexity, as well as the uncertainty of the stratigraphic thickness, as well as a statement on balancing limitations

At present, I think this is a significant contribution and will have a significant impact on uncertainty assessment.

Kind Regards

AE additional comments on manuscript:

L293 – What was the resolution of the SRTM dataset that you used?

90 m resolution, added in the text

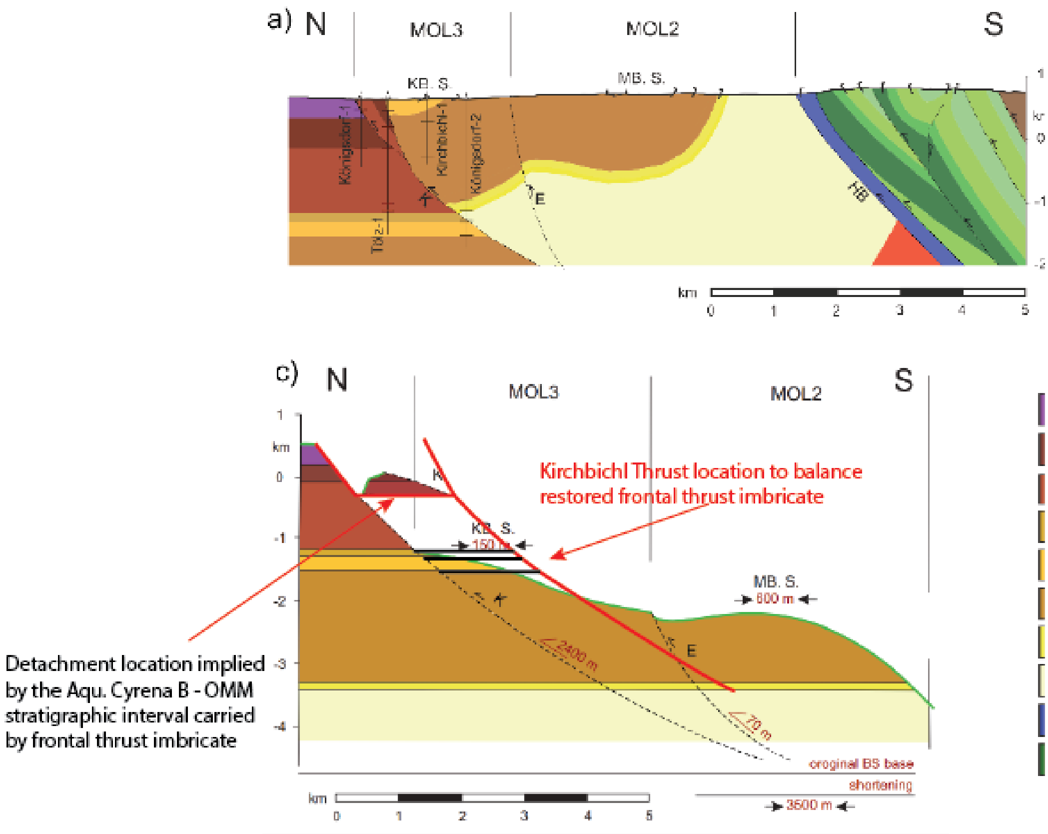
Figure 1 – This map needs some a coordinate grid and could also benefit from a small location map, i.e., where in the broader Alpine system is this study area as not all readers will be as familiar with the local geology or geography.

Done

L776-779 – Following from the comment on the topography, here you indicate that you're using topography from Google Earth? Is this accurate or are you using SRTM data directly? If you're using the underlying (relatively coarse) topography from Google Earth, is the uncertainty really that small? How did you evaluate these uncertainty values?

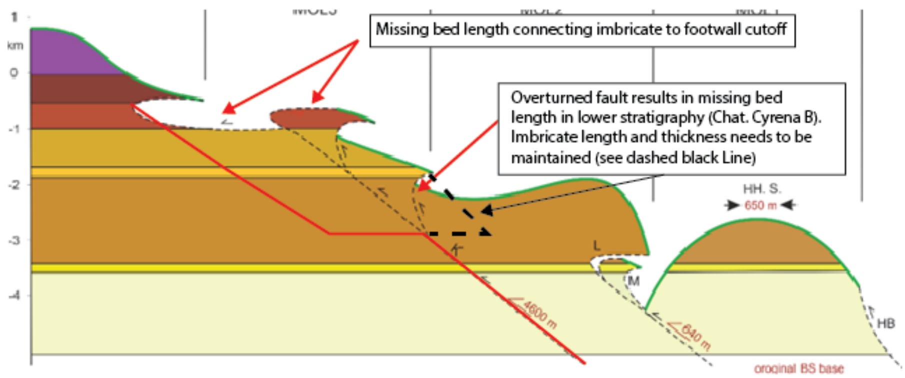
See comment above – we did use the SRTM data. We have deleted the estimate of 50 m. Regardless of the exact value, for a km-scale model, the positioning error of data on the map is negligible.

5. The cross section in 5a is not correctly balanced at the western edge. The interpreted frontal thrust imbricate at the Kirchbichl fault would require a detachment within the Aqu. Cyrena B formation of equivalent length to the thrust sheet. Line lengths may have been preserved in the restoration but the interpreted structure is not balanced with the underlying stratigraphy. This is why the small thrust is “floating” in the restored section, indicating a lack of structural/stratigraphic balance. The whole reason behind balancing a cross section is to constrain issues like balancing repeated stratigraphic sections in thrust imbricates with the underlying stratigraphy not involved in said imbricates. Dahlstrom’s original objective was to limit thrust belt interpretations in the Canadian Rockies to those that were structurally and stratigraphically consistent.



A limitation of cross section balancing as proposed by Dahlstrom originally is that the sequence has to be forward breaking. In the case of the Subalpine Molasse it has been shown that the sequence is breaking backwards, which then results in seemingly inconsistent solutions. We have added a respective paragraph in the section on balancing and in the discussion

6. Similar structural balance issues arise in Figure 6 (Section 2), 7 (Section 3), and 8 (Section 4). In these cases the frontal imbricate at the Northern edge of the Kirchbichl thrust should restore to the footwall cutoff at the OMM and Aqu. Cyrena B. levels. The thicker stratigraphic package in these imbricates implies a flat detachment somewhere in the Chat. Cyrena B. level. Additionally, the apparently overturned faults in the restored section are not geologically realistic, it looks like the kinematic restoration algorithm (vertical shear?) you are using is also being applied to the fault planes which is not accurate. Generally, cross sections keep fault geometries consistent from deformed to restored states unless they are involved in re-folding due to motion on underlying faults and stacking of imbricates. The balance issues in all four sections may have significant implications for how the structures are interpreted. Correcting them may lead to different results with respect to the 3-D and probabilistic models.



7. Given that this is a new approach to geologic mapping and cross sections, I think it would really be helpful to start with a very simple, artificial example that can help readers really understand the geologic implications behind the probability maps and cross sections. It's great to show this new method applied to a relatively well controlled, natural example but the concept of geologic probability maps and cross sections will not be familiar to most readers. Having a very simple example where the outcome is already known could help demonstrate you workflow and also educate the readers on how to interpret the results for the thrust belt case study.

This is a great suggestion, and as stated above since first submission of this manuscript a respective study focusing on the method only has been published (Brisson et al.). We now can refer to this study. Furthermore, we now do not propose anymore the iterative workflow, but rather pave towards one.

8. Overall, any discussion of probability and entropy results needs to directly address how they relate to the geology. Lines 625 – 630 are a great example of presenting the entropy field for a structure, discussing it's relationship to the probability field, and relevance to the geology. This needs to come earlier and be maintained throughout the results and discussion section

We have added an explanation of entropy in order to make this more clear.

9. 5.1.3 Strain Distribution in the Study Area: Any discussion of strain variation based on cross section shortening estimates should acknowledge the possibility that mechanisms such as fracturing, pressure solution, and cleavage development can accommodate strain in ways that is not accounted for by line-length restoration. Discussing regional changes in mechanical stratigraphy could shed some light on this.

We have added a paragraph on penetrative / distributed strain in the methods, and refer to it in the discussion

10. Section 5.2: Much of the material in this section reads like it should be at the beginning of the paper in a more detailed discussion about the methods and how they are integrated. In the current version, this information comes to the reader too late.

That is true, we have moved the section on uncertainty of input data to the methods section.

Summary of changes to further Figures:

Figure 1: we inserted a map of the wider region for orintation and inserted a grid. Also, we did some minor improvements of the design.

Figures 5, 6, 7: Design changes: not moddeled areas in a) are cropped and in b, the parts over the topography are cropped.

Figures 11, 12: Probabilistic model results are split into two figures as suggested by the reviewers, allowing the single panels to be enlarged.

2nd Round of Revisions

Decision Letter

Dear Kevin and co-authors,

Your revised manuscript has now been carefully evaluated by one of the former reviewers (Nate Eichelberger) and by our associate editors (Adam Forte and Laura Federico). They all agree that your work deserves to be published in Tektonika, but needs further revision. You'll find their comments and recommendations below. The reviewer asked for quite extensive revision, which would require significant additional work. I recommend to follow as much as possible their propositions and comments. However, as it will be a second revision for your manuscript, and to avoid too long delay, the associate editors offer a compromise solution that clarifies the minimum to be done before your manuscript can possibly be accepted for publication. I hope you will be able to rapidly submit a revised version (together with a rebuttal letter), if possible within one month. I suggest a due date by the beginning of July for submitting this revision, although not mandatory (it may be extended on request).

Best regards,

Robin Lacassin, Tektonika Executive Editor

1/ Associate Editors' (Adam Forte and Laura Federico) recommendations:

The reviewer makes some important points with regards to what this paper ultimately delivers, or does not, and we generally agree that at present, the paper feels somewhat incomplete. We suggest carefully considering the reviewers main point, but offer a compromise solution compared to what is suggested by the reviewer. Ultimately, following through on the reviewers suggestion, i.e., to fully realize the suggestion laid out in section 6.2.1 of iteratively using a balanced cross section to constrain a GemPy reconstruction and then using the uncertainties from the model to revisit the balanced sections, would greatly improve this paper. At the same time, we recognize this is effectively asking for significant new work in a clearly already substantial effort. As an alternative, focusing on section 6.2 more broadly and trying to lay out in more detail, ideally with a figure, of hypothetically how such an iterative process would work. Both the reviewer and we recognize that some version of what is being asked for was in the first draft, but the challenge there was that the graphic and description around it were not particularly effective in conveying the point. Thus, the path forward may be to revisit some of the things excised between the original version and the revision, but to focus on how to make these as clear and specific as possible.

We would additionally encourage you to consider some of the more technical points in the reviewers comments and also to address the few minor wording or phrasing issues we highlight below. We look forward to receiving a revision of this manuscript!

Some rephrasing suggestions:

L77 - "ultimately paving the way toward"

L113 - "paving the way toward"

L121 - "on top of"

L215 - "conducted our own field work"

L238 - "choosing the Subalpine Molasse, and our study area in particular, we"

L257 - "as the main scope"

L342 - "we removed 17 adjacent data" (?)

L767 - "Projection errors grow with increasing distance"

Comments by Reviewer 1 (Nate Eichelberger)

I appreciate that the authors have made some extensive revisions to the paper and addressed many of my points from the initial review. But in removing the iterative model development to incorporate uncertainties into geologic cross sections, the paper now seems incomplete.

It is now much clearer how the interpreted cross sections are used to create geologically realistic implicit 3D models and probabilistic cross sections. However, the snag is that the authors only discuss the how uncertainty insights from the probabilistic cross sections could be used to revise and improve the interpreted geologic cross sections. However, they don't develop alternative interpretations that are permissible within the computed uncertainties.

I think would be very compelling to see something like the following workflow. The initial balanced cross sections are used to create the 3D probabilistic models. Then, from those computed uncertainties, modify the initial cross sections to create end-member geologic scenarios that express the range of possible solutions. So only one iteration, but it would at least close the loop on incorporating statistical uncertainties into geologic cross sections.

The authors were clear in the paper's introduction that the study is meant to "pave the way" for this sort of approach but it seems incomplete to just present cross sections and the 3D probablisitic models without really integrating them in a concrete way. In the present form, the paper is in limbo between a regional geologic study and a methodology paper.

I also have a few specific points below but they may be less pertinent following further revisions. A secondary but important issue is that I still disagree with the authors about how they have represented the restored out-of-sequence structures. However, this does tie in with the uncertainties associated with the frontal thrust structure and perhaps something that could be addressed by integrating the probabilistic results with the interpreted geology.

- Line 81: Clearly define "steady state uncertainty modeling" in the introduction, especially in terms of application to geologic problems.

- Line 201: "… conceptual biases of the model can be excluded": This is a bold statement even if there are significant existing datasets available. As you say in the prior sentence, the terminal triangle zone is poorly constrained. So conceptual biases could be minimized but there is always plenty of room for conceptual bias in interpreting said data. You make a similar point in section 6.2.1 .
Even using balanced cross section balancing is a form of conceptual bias since it holds bed lengths constant and therefore imposes specific kinematic

assumptions/biases. I'd consider just deleting this or being very specific about the conceptual biases that are being minimized.

- Line 462: While back-breaking or out-of-sequence thrusts can create "unusual" fault geometries in restored sections, they can still be restored by line length balancing. Boyer and Elliot 1983 does not state that forward breaking sequences are required, just that restored out of sequence thrusts will cut earlier faults at high angles or over turned (see page 77, Boyer and Elliott, 1983). Line length balancing is simply a way of following mass conservation when restoring a section. The most important thing about any restored cross section is that the restoration follows the observations that make up the deformed section. This can lead to complex restored fault paths but as Boyer and Elliott state: "As long as the fault blocks can be fit back together and the appropriate lengths and areas in the two sections are equal, then balance has been achieved".

As such, there is still missing stratigraphy in your restored section that would need to be accounted for even if it was eroded as part of the "floating imbricate" being cut by later formation of the Kirchbichl thrust. This missing stratigraphy means there is missing area in the restored section, creating balance issues. This could ultimately affect shortening estimates which become a discussion point later in the paper.

I think that this complexity at the frontal structure highlights the utility of the probabilistic approach you are developing but it is not quite realized within this paper.

I do think the material here deserves to be published because practical methods for understanding uncertainties in geologic sections really is a huge blind spot for the science. But at this point, the paper still seems incomplete as far making a meaningful scientific contribution. I would again encourage the authors to resubmit after finding a way to more tightly integrate the geologic interpretations with the statistical insights from the 3D models.

Regards, Nate Eichelberger

Authors' Reply to Reviewers

Dear Editors, dear reviewer

Thank you very much for the appreciation of the revised manuscript, the very appealing idea of a compromise and further helpful comments.

Editors and reviewer agree that a full implementation of an iterative approach would improve the manuscript, while the editors acknowledge that this would require major remodeling and the manuscript as such already provides substantial steps forward. We agree with the reviewer and editors in all these points and appreciate the compromise offered to describe and discuss the proposed iterative workflow more precisely. Additionally, we point out how the state of the tools limits implementation of the full iterative approach by now and highlight how we nonetheless show that the approach will work once the tools can handle all requirements.

The manuscript has been modified accordingly. Please find below answers to all comments by the reviewer and the editors in green.

Comments by editors:

Dear Kevin and co-authors,

Your revised manuscript has now been carefully evaluated by one of the former reviewers (Nate Eichelberger) and by our associate editors (Adam Forte and Laura Federico). They all agree that your work deserves to be published in Tektonika, but needs further revision. You'll find their comments and recommendations below. The reviewer asked for quite extensive revision, which would require significant additional work. I recommend to follow as much as possible their propositions and comments. However, as it will be a second revision for your manuscript, and to avoid too long delay, the associate editors offer a compromise solution that clarifies the minimum to be done before your manuscript can possibly be accepted for publication. I hope you will be able to rapidly submit a revised version (together with a rebuttal letter), if possible within one month. I suggest a due date by the beginning of July for submitting this revision, although not mandatory (it may be extended on request).

Best regards,

Robin Lacassin, Tektonika Executive Editor

1/ Associate Editors' (Adam Forte and Laura Federico) recommendations:

The reviewer makes some important points with regards to what this paper ultimately delivers, or does not, and we generally agree that at present, the paper feels somewhat incomplete. We suggest carefully considering the reviewers main point, but offer a compromise solution compared to what is suggested by the reviewer. Ultimately, following through on the reviewers suggestion, i.e., to fully realize the suggestion laid out in section 6.2.1 of iteratively using a balanced cross section to constrain a GemPy reconstruction and then using the uncertainties from the model to revisit the balanced sections, would greatly improve this paper. At the same time, we recognize this is effectively asking for significant new work in a clearly already substantial effort. As an alternative, focusing on section 6.2 more broadly and trying to lay out in more detail, ideally with a figure, of hypothetically how such an iterative process would work. Both the reviewer and we recognize that some version of what is being asked for was in the first draft, but the challenge there was that the graphic and description around it were not particularly effective in conveying the point. Thus, the path forward may be to revisit some of the things excised between the original version and the revision, but to focus on how to make these as clear and specific as possible.

⇨ We highly appreciate the offered compromise and revised especially section 6.2 accordingly. Thereby, we reinserted the improved respective figure and discussed the proposed approach much more precisely and clearly.

We would additionally encourage you to consider some of the more technical points in the reviewers comments and also to address the few minor wording or phrasing issues we highlight below. We look forward to receiving a revision of this manuscript!

Some rephrasing suggestions:

L77 - "ultimately paving the way toward"

Done

L113 - "paving the way toward"

Done

L121 - "on top of"

Thank you, corrected

L215 - "conducted our own field work"

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Done

L257 - "as the main scope"

Done

L342 - "we removed 17 adjacent data" (?)

thank you, revised

L767 - "Projection errors grow with increasing distance"

done

2/ Reviewer's (Nate Eichelberger) evaluation on revised manuscript:

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It is now much clearer how the interpreted cross sections are used to create geologically realistic implicit 3D models and probabilistic cross sections. However, the snag is that the authors only discuss the how uncertainty insights from the probabilistic cross sections could be used to revise and improve the interpreted geologic cross sections. However, they don't develop alternative interpretations that are permissible within the computed uncertainties.

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⇨ We appreciated the remarks and agree that closing the loop is the ultimate goal. See the new solution in 6.2, following the compromise offered by the editors and some more thorough explanation as provided below.

I also have a few specific points below but they may be less pertinent following further revisions. A secondary but important issue is that I still disagree with the authors about how they have represented the restored out-of-sequence structures. However, this does tie in with the uncertainties associated with the frontal thrust structure and perhaps something that could be addressed by integrating the probabilistic results with the interpreted geology.

- Line 81: Clearly define "steady state uncertainty modeling" in the introduction, especially in terms of application to geologic problems.

⇨ We acknowledge that this approach is somewhat knew to many readers and some might request further information on it. We introduced steady state and steady state uncertainty modeling, including reference to the literature on this topic in lines 98 – 104 of the new version. For our specific application, we describe what we do and what the modeling tool does in a detailed way (4.2.3).

- Line 201: "... conceptual biases of the model can be excluded": This is a bold statement even if there are significant existing datasets available. As you say in the prior sentence, the terminal triangle zone is poorly constrained. So conceptual biases could be minimized but there is always plenty of room for conceptual bias in interpreting said data. You make a similar point in section 6.2.1 . Even using balanced cross section balancing is a form of conceptual bias since it holds bed lengths constant and therefore imposes specific kinematic assumptions/biases. I'd consider just deleting this or being very specific about the conceptual biases that are being minimized.

⇨ We agree and specified what we wanted to express:

⇨ „[...]. This, together with the poorly constrained strike-slip faults provides the main structural uncertainty of the study area. However, the large existing structural and stratigraphic data set allows to test modeling approaches as the influence of conceptual biases is decreased and models are much more data driven."

(l. 200 – 204, new version)

- Line 462: While back-breaking or out-of-sequence thrusts can create "unusual" fault geometries in restored sections, they can still be restored by line length balancing. Boyer and Elliot 1983 does not state that forward breaking sequences are required, just that restored out of sequence thrusts will cut earlier faults at high angles or over turned (see page 77, Boyer and Elliott, 1983). Line length balancing is simply a way of following mass conservation when restoring a section. The most important thing about any restored cross section is that the restoration follows the observations that make up the deformed section. This can lead to complex restored fault paths but as Boyer and Elliott state: "As long as the fault blocks can be fit back together and the appropriate lengths and areas in the two sections are equal, then balance has been achieved".

As such, there is still missing stratigraphy in your restored section that would need to be accounted for even if it was eroded as part of the "floating imbricate" being cut by later formation of the Kirchbichl thrust. This missing stratigraphy means there is missing area in the restored section, creating balance issues. This could ultimately affect shortening estimates which become a discussion point later in the paper.

I think that this complexity at the frontal structure highlights the utility of the probabilistic approach you are developing but it is not quite realized within this paper.

- ⇨ We technically agree and the discussion point outlined by the reviewer is also one of the main motivations to communicate our ideas in form of this paper. However, for fully realizing this approach already in this paper, the software has some limitation as of now. Especially the fact that terminations of faults (at other faults, fault braching or just decreasing of the offset to zero) cannot be modelled until now make it impossible to adress the critical volume affected by the Kirchbichl Thrust. Thus, for now, we have to stick with discussing the approach and showing parts oft he full cycle, before improvement of the algorithms allows for a practical realization. We inserted such an explanation of these limitations in the new section 6.2.2

I do think the material here deserves to be published because practical methods for understanding uncertainties in geologic sections really is a huge blind spot for the science. But at this point, the paper still seems incomplete as far making a meaningful scientific contribution. I would again encourage the authors to resubmit after finding a way to more tightly integrate the geologic interpretations with the statistical insights from the 3D models.

Regards, Nate Eichelberger

Summary of changes to Figures:

New Figure 13: as part of the compromise we provide a new figure illustrating the links creating the full iterative modeling loop.

Acceptance Letter

Dear Kevin and co-authors,

The second revision of your manuscript, "Constraining the 3-D geometry of fold-thrust belts using section balancing vs. 3-D interpolative structural and probabilistic modeling," has now been carefully evaluated by our associate editors (Adam Forte and Laura Federico) and executive editor (Robin Lacassin). We are all in agreement that this latest round of revisions sufficiently addresses the core concerns that we and the reviewers had, and so we are pleased to accept your article for publication in Tektonika. Messages will follow in the coming days with regards to production.

Best regards,

Adam Forte, Tektonika Associate Editor on behalf of

Robin Lacassin, Tektonika Executive Editor