Response to the response to the review by Patrice Baby

The paper of Habel et al. presents a structural study of two sites of the western Andes in Chile (20-22°S), where the authors use numerical trishear forward modelling to evaluate minimum horizontal shortening and analyse the kinematic evolution of two fault-related anticlines. Before being published, this paper must better document the structural observations, which are not always convincing (see below). These data can be used to construct in each area a balanced section to validate structural interpretations and calculate shortenings more rigorously. The authors need to better explain why they chose a fault propagation fold model rather than a tectonic inversion model in their structural interpretation.

These various issues on why we conservatively can't do much better on building cross-sections and calculating from there shortening, or the chosen structural interpretation (tectonic inversion vs. fault propagation fold) will be detailed hereafter. To summarize here in a few words:

- There are no constraints on the structure of the footwall of the faults inferred to generate the folds observed from surface geology.

This is the case in most balanced cross-sections. It is precisely the method of balanced cross-sections that makes it possible to propose viable interpretations. In figure 11, you nevertheless propose a footwall geometry !? The advantage of a balanced cross-section constructed along your entire section is that a total minimum shortening can be calculated, including that of the PT.

Given this, a more rigorous structural section and the associated shortening can't be proposed.

Not agree, see above

Because of this, we do only propose a possible interpretation and discuss its implications.
This is now better explained lines 222-225. To better separate observations from more interpretative outcomes, Figures 5c and 9c now only report field observations, with no inferences at depth - these come later, on Figure 11 when incorporating our trishear modeling results.

- We do not oppose tectonic inversion to fault-propagation folding. In fact, the investigated regions were forming basins during the Mesozoic, and these basins have been inverted during the Cenozoic to form the structures documented here along the Western flank of the Andes. This inversion most probably re-used faults that structured these basins, variably along-strike, and these inverted faults may have formed the fault propagation folds documented in our work. However, the main question relates here on whether these faults connect or not at depth onto a common detachment (as proposed by e.g., Victor et al 2004 and Armijo et al 2015), or whether they are independent from each other (as proposed in Fuentes et al 2018 and Martinez et al 2021, even though their later interpretation - in Fig 37.9 of Martinez and Fuentes 2022

- gets back to connecting these faults at depth onto an east-dipping detachment). Surface geology of the investigated field sites does not allow to choose between either one of these two models. This is now better explained in section 2.2.2 (lines 155-167) and 7.2.1 (lines 722-725). **However, from regional considerations we favor the interpretation that these faults connect at depth onto a common detachment** (see revised section 7.2.1, lines 722-731).

You have chosen a model of thin-skinned tectonics - with a detachment in the sedimentary cover - which can in no way be associated with a tectonic inversion. Generally, tectonic inversion involves the basement and results in thick-skinned tectonics as in Figure 37.9 of Martinez and Fuentes.

Finally, we recall that our proposed modeled structural interpretation and line-length balancing provide upper and lower bounds on shortening estimates, respectively, as now clarified lines 620-622.

It's just the shortening of a fold that is not representative of the cross-section shortening.

GENERAL COMMENTS:

The title must be modified. The studied areas are too small to represent the entire western Andes. It would be interesting to locate the two sites on a regional cross-section through the western Andes.

We agree with RC2 that our two areas are too small to represent the entire Western Andes - this is largely recognized throughout the manuscript. However, based on our findings from these two limited areas, we discuss the implications of these findings for shortening and kinematics of deformation at the scale of the entire Western Andes, from a regional reasoning.
The fact that we mention that our work is only "a contribution to the quantification of crustal shortening" does imply that we provide key additional data, not that we solve the problem at the scale of the entire Western Andes.
Title slightly modified to make it clearer: *A contribution to the quantification of crustal shortening and kinematics of deformation across the Western Andes (~20-22°S).*

OK

A regional general cross-section of the region added to Figure 1 would allow for structurally locating the investigated field sites. A section inspired from that shown in Figure 13 of Armijo et al 2015 would be appropriate. However, providing this section early in the manuscript may give the wrong impression that our interpretation is biased, and give room to unneeded criticism, even before we put forward our various arguments.

Anyway, it is clear that your regional interpretation is guided by the work of Armijo et al.

Later, this section would only be schematic as we only have field data from limited outcrops. Building a new section of the western Andean flank would

need more observations and discussions that are beyond the scope of the present paper which is focused on the frontal basement thrust (ABT) and structures just west of it. Our paper is already quite long and detailed. As such, we decided not to add and discuss such a large scale section.

This is true and this is the problem. You draw important conclusions from little data.

The stratigraphic and geologic background (2.2.1) needs a figure with a synthetic stratigraphic column.

This is already schematically provided in the legend of Figure 1, which corresponds to this part of the main text. As this is a very general regional overview, we do not see the need for more at this stage.

Not agree. Difficult to have a regional view. No regional cross-section, no synthetic stratigraphic column!

Structural and kinematic context (2.2.2):

In their last paper, Martinez and Fuentes (2022)(https://doiorg.insu.bib.cnrs.fr/10.1016/B978-0-323-85175-6.00037-7) show the importance of tectonic inversion of the Jurassic rift in this region. The analysed seismic sections are just west of the study areas of this paper and must be taken into consideration and discussed.

We already discussed the work and results of the group of Martinez and Fuentes, by citing their original work and data (Martinez et al 2021 and Fuentes et al 2018) rather than this later summarizing paper that does not provide any new interpretation of the seismic profiles.

This paper is a Chapter of the Elsevier Book “Andean Structural Styles. A Seismic Atlas”, where seismic interpretations have been validated by experts. The authors oh this Chapter write: “The inversion of these east-dipping faults produced the uplift of the hanging wall fault blocks and the development of west-verging asymmetrical anticlines with short and steeply dipping frontal limbs and large and gently dipping backlimbs (Figs. 37.6 and 37.7)”. This is very similar to the anticlines in Fig. 11, especially the one in Fig. 11a which propagates on a steeply dipping fault.

As mentioned earlier, tectonic inversion is not in opposition with our interpretation. In fact, the continental and marine Mesozoic series described in our field sites were deposited in basins, that were lately inverted during Cenozoic compression to form the structures of the Western flank of the Andes.

You have chosen a model of thin-skinned tectonics - with a detachment in the sedimentary cover - which can in no way be associated with a tectonic inversion.

The two main previous opposing views of these structures were about their geometries at depth. On one hand, Martinez et al 2021 and Fuentes et al 2018 propose that they are single planar faults not connected to each other at depth, minimizing shortening estimates. On the other hand, Victor et al 2004 and Armijo et al 2015 propose that these faults form a thrust system connected at depth onto a common decollement, dipping eastward beneath the Andes.

You cannot compare with the deep geometries that are done on a regional scale and involve the basement.

Seismic profiles to the west of our field sites are too poorly resolved at depth - let's be frank on this! –

Not agree. Seismic profiles clearly show tectonic inversions.

and field observations are too sparse and local to favor one or the other model.
This is now clarified in the revised version of the manuscript (lines 155-167).

However, as already discussed in Victor et al 2004, and considered by Armijo et al 2015, only the thrust system model is able to provide a satisfactory structural framework for the large-scale structural organisation (deep basement to the east, shallower Mesozoic and Cenozoic units to the west) and topography (high to the east, low to the west, with a westward dipping continuous slope) of the Western Andes. Interestingly, in their latest interpretation, Martinez and Fuentes 2022 propose to connect these various faults at depth onto an east-dipping detachment (See their figure 37.9). To summarize, only from regional considerations can we favor the interpretation of a thrust system, as indicated in our final discussion (section 7.2.1, slightly modified to clarify this reasoning, and separate local observations from regional interpretations).

I don't really understand your reasoning. At the mountain scale, inverted faults are part of the thrust system and often control deep basement structure uplifts.

Data and structural observations:

It is important to better document the field data. For example, it is necessary to localise the field structural data in the structural map of Figure 4 to validate the cross-sections construction and structural interpretations.

All field data are reported on the map of Figure 4 and on the field sections of Figure 5. We cannot place on the map all measurements for obvious readability reasons, this is why we represented (some of) them along the field sections of Figure 5 - a section that is located on the map. We remind here that additional field observations are located on the map of Figure S14.
We now report some of our structural measurements (either from field measurements or extracted from 3D mapping) to the structural map of Figure 4. Those now reported are meant to illustrate the asymmetry of the documented folds.

We would like also to underline the fact that field data do not resume, to our sense, only to numerous analytical field measurements of strike and dip angles, but also encompass large-scale landscape observations - illustrated in the various field pictures provided in the main manuscript and in its supplement.

Field pictures interpretations must be also validated by field data. These field data, as structural dip measurements, must be placed on the pictures. I am not at all convinced by the structural interpretation of the picture in Figure 7b. I can't really see the axis of the anticline of the Quebrada Tambillo, which is a key element of the structural interpretation. This picture interpretation must be absolutely validated by field measurements.

**We chose to show field pictures to depict the various observed structures, for instance with the general trend in dip angles on either side of axial planes. This is to our sense much more meaningful here than numerous local field measurements for our interpretations at the scale of the field sites. Landscape views are true field data, even though qualitative. In figure 7b for instance, the axis of the anticline is defined by the change in the dip orientation of layers, with layers dipping to the east (left of the figure)
on one side and layers dipping to the west (right on the figure) on the other side, whatever the absolute values of the dip angles, and whatever the number of dip angle measurements.**

We are in the 21st century, and structural cross-sections are no longer constructed from landscapes, but from robust quantitative structural data, and using 3D software.

We added additional information on the field pictures, whenever appropriate and meaningful, either from direct field measurements, or from the projection of mapped 3D layers on satellite images and DEMs.

The structural map of the Quebrada Blanca zone shows structural dips values, which is not the case for the structural map of the Pinchal area. I understand that strike and dip measurements are extracted from 3D mapping. These 3D mapping and data extraction must be documented with some detailed illustrations.

See our previous answers about representing structural measurements on the map of Figure 4 (Pinchal Zone). A detailed illustration of the structural inferences that can be made from 3D maps is already provided in the surface section of Figure 5a.

Structural interpretations:

I don't understand why the authors didn't try to construct balanced cross-sections, the best way for thrust system modelling and calculation of shortening. The proposed interpretations are not geometrically validated. The footwalls of the thrusts have not been constructed (?).

The footwalls of the thrusts are not drawn in our sections of Figures 5 and 9, as we recognize not to have any indication on their structure. The structure used to build our trishear models is just a proposition, as indicated in the text. As such no definitive balanced section can be honestly proposed just from our field observations.

You could have made the same proposition using the balanced cross-section method, but at a more regional scale, which would have allowed you to validate it or not.

This is now better explained in the text (lines 222-225, 531-532, 553-554)

The authors propose a model of fault propagation fold (or fault bend fold (?)) for each section. Why? Why not a tectonic inversion? How do you explain such a steep frontal ramp in the cross-section of Figure 9C? Are there lithologies compatible with the levels of detachment?

As already indicated, tectonic inversion is not to be opposed to fault-propagation folding (see previous answers). We agree that structural inheritance from the earlier Andean Basins may have played an important role in localizing the thrusts at depth, or in controlling their steep geometry. This said, the observed folding is most simply explained by fault-related folding. Fault propagation folding is favored here as the faults do not reach the surface, and as small scale folds at the front of the western anticlines are possibly indicative of disharmonic folding (and therefore internal deformation) at the tip of an upward propagating ramp.

These are not valid arguments.

As of the lithologies at depth that would favor a decollement level, as can be deduced from our cross-sections (Figures 5c, 9c and 11), these stratigraphic levels do not crop out in the investigated areas and we cannot tell more on the subject.

You could have deduced this from the regional sedimentary thicknesses.

Our manuscript has been re-organized so as to better clarify these points. We now better separate field observations (revised sections 4 and 5, and updated figures 5c and 9c), modeling and local kinematic considerations (section 6, with Figure 11) and our choices of interpretations from regional considerations (in particular in section 7.2). But in any case this does not question our final conclusions and the field data provided.

The calculation of shortening is confusing ("Folding" + "Folding + thrusting"(?)).
We moved to section 6 all the structural reasoning leading to shortening calculations. By having all this together in one section, we hope that these various results are much more understandable now.
The discussion would require an integration of results in a regional cross-section through the Western Andes.

This has already been discussed previously.

If you don't want to introduce the regional cross-section at the beginning, you could have at least put it at the end.

My detailed comments are highlighted in the attached pdf version.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-629/egusphere-2022-6 29-
RC2-supplement.pdf
We hereafter copy the various comments found throughout the reviewer's annotated PDF.
Please note that some text sections were highlighted but not commented by the reviewer.

Line 1: The studied areas are too small to represent the entire western Andes unit
See previous answer to the comment on the title
Modified to ""investigated field sites" (line 14)

Line 17: They are not really restored
Right, since we do not model the entire sections.
This has been corrected to "From our interpreted sections " (line 17)

Lines 47-49: Add: Baby et al., 1997
https://doi.org/10.1130/0091-7613(1997)025<0883:NSCTCT>2.3.CO;2
and Rochat et al., 1999
https://www.researchgate.net/publication/233713934_Crustal_balance_and_control_of_the_erosive_and_sedimentary_processes_on_the_Altiplano_formation
Lines 101-102: Baby et al. (1997):
https://doi.org/10.1130/0091-7613(1997)025<0883:NSCTCT>2.3.CO;2
Lines 104-106: Baby et al. (1997):
https://doi.org/10.1130/0091-7613(1997)025<0883:NSCTCT>2.3.CO;2
We recall that RC1 recommended to limit here the citations only to the few most significant ones among the numerous possible publications from the extensive literature on the subject of Andean shortening across the whole range!

It would have been interesting to diversify the authors for once.

Figure 2: It would be appropriate to represent the Pinchal thrust on both images.

We agree that this could be a good idea, even though the aim of this figure is to present first-order observations on the stratigraphic organization at the scale of the landscape. We tried to add the Pinchal Thrust but we did not find a satisfactory solution that would not load too much the figure - in particular when only keeping only one of the two field pictures as suggested by RC1. Therefore, this suggestion was not implemented.

As with my other suggestions!

Figure 3: It is hard to imagine that there are no more precise ages in the bibliography with all these fossils and volcanic levels. It would be appropriate to indicate on the figure more precise ages such as 27-29 Ma at the base of the Cenozoic, or the Triassic and Jurassic (Majala o Chacarilla Fm?).

We recall that the Pinchal zone is really remote and not easily accessible. The only geological map of the area dates back from the early 1980's (Skarmeta and Marinovic, 1981) and we believe from our field experience of this site that their stratigraphic correlation was done from an a priori knowledge of the regional stratigraphy rather by actual dating of the local layers.

Precise dating remains to be done here and we agree that there is a good potential for this. The SERNAGEOMIN (Geological and Mining service of Chile) will be working on this most probably over the next years. This is out of the scope of this manuscript.

Figure 3: Locate the décollement of the cross-section in Figure 5c?

This decollement is a structural interpretation proposed from our field observations, not a field observation and should therefore not be placed on the stratigraphic column of Figure 3.

Also, as suggested by our sections (former Figure 5c, now Figure 11ab-c**), the proposed decollement level does not crop out- and cannot therefore be represented on the log where only the series observed in the field are reported.**

We corrected the caption of Figure 3 to emphasize the fact that only field observations (at the surface) are reported.

Figure 5c: I am not convinced by this anticline. Field data are lacking

See comment above about field data and lanscape views.

See my response

Figure 5c: This thrust geometry is very arbitrary. The footwall of the thrust must be constructed to geometrically validate the interpretation. Is there a lithology compatible with this level of décollement? Why not tectonic inversion?

To clarify our point, we now better separate field observations, sub-surface and deeper interpretations. Here, we now only report sub-surface observations on Figure 5c, and leave deeper interpretations for Figure 11.

As already mentioned, we have no direct clue about the structure of the footwall from field geology or even geophysics - this is why we had left it blank. Also, we already explained earlier why the tectonic inversion hypothesis is not to be opposed to fault related folding. This has been further clarified in the text

Figure 6: You must indicate values of the structural dips collected on the field and show them on the photo. Scale?

We added an approximate scale on Figure 6a **- there is already a scale (Swiss knife) on the picture of Figure 6b). Dip measurements could be added on Figure 6a, but would not be meaningful here, at this scale, in the highly deformed footwall of the Pinchal Thrust**.

This is not satisfactory

We only added this information on Figure 6b.

Figure 7: You must indicate values of the structural dips collected on the field and show them on the photo. Scale?

We added dip angles, either measured in the field or deduced from 3D mapping on Figure 7. A scale is also now reported, it is approximative because of perspective effects (indicated in figure caption).

Figure 9c: I'm not really convinced by this part. Lack of field data!

Well... the small-scale folds are illustrated in the field picture of Figure 10b.... as indicated in the boxes of Figures 9a-b. To our sense, not much to be added here without a more precise comment.

Lack of quantitative field data!

Figure 9c: This thrust geometry is very arbitrary. The footwall of the thrust must be constructed to geometrically validate the interpretation. Are there lithologies compatible with these level of detachment? Why not tectonic inversion? How do you explain such a steep ramp?

As indicated in our previous responses, we better separate in our revised manuscript surface observations and sub-surface (folds) deductions - as reported here, in particular in revised figure 9c - from deeper interpretations (thrust geometries) - as now only reported in Figure 11.

See previous answers about tectonic inversion and the geometry of faults at depth.

How do you explain such a steep ramp?

Figure 10: I'm not really convinced by this part. Lack of field data!

Same answer as for the comments on Figures 5c and 7 above, about the frontal anticline of the Pinchal zone. Also this anticline (Chacarilla anticline) has already been documented in previous published (cited) work: Blanco and Tomlinson 2001, Armijo et al 2015, Fuentes et al 2018.

We added some structural measurements, following comments on other field pictures.

Figure 11a: How do you explain such a steep ramp?

Steep ramps are commonly found, in particular in the case of inherited previous structures. This is probably the case here as Mesozoic layers were deposited initially in extensional basins. Here "steep" is only ~40-45°, as reported in table S3... so not that extremely steep. Such dip angle is derived from the geometry of the folded layers of the eastern limb of the Chacarilla anticline, and because of this should be here taken as a minimum.

I don't understand the argument

Figure 11c: It is a fault bend fold!

We do not agree: it is a fault-propagation fold, as the fault has not yet reached the surface and deformation is disharmonic with layer thickening (and folding) at the tip of the ramp.
But we agree that this fault-propagation fold is not far from becoming a fault-bend fold with additional incremental slip and fault propagation.

The staircase trajectory of the fault is typical of a fault bend fold

Figure 11: How do you explain the difference in the depth of the detachment between the two cross-sections?

The two sections are ~70-80 km away from each other (Figure 1), with clearly differing stratigraphies, most probably controlled by the details in the earlier local Andean Basin (structural and stratigraphic inheritance). Also the Pinchal section is located in the immediate footwall of the Pinchal Thrust (Figure 5c), and we cannot rule out the possibility that the thrusts generating the observed folding of the Mesozoic layers are here shallower splays of the Pinchal thrust.
We added a few words on this (lines 609-615).