We hereafter respond to the various comments and questions addressed by Patrice Baby (RC2) in his review. His review is entirely reported in black, and our responses in bold blue.

The suggested revisions do not question our results and conclusions, but will clearly help improve our manuscript by clarifying our arguments and their presentation. We thank Patrice Baby for this discussion, as well as for his various comments and suggestions.

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. Given this, a more rigorous structural section and the associated shortening can't be proposed. Because of this, we do only propose a possible interpretation and discuss its implications.

- 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. However, from regional considerations we prefer the interpretation that these faults connect at depth onto a common detachment (see section 7.2.1).

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.

Adding a regional general cross-section of the region is a good idea, in particular in Figure 1, to structurally locate 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 interpretations are biased, and give room to unneeded criticism, even before we put forward our various arguments. As such, we'd rather avoid to revise the manuscript accordingly.

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.

Structural and kinematic context (2.2.2):

In their last paper, Martinez and Fuentes (2022)(https://doi-org.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.

In fact, 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.

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 initially in basins, that were lately inverted during Cenozoic compression to form the structures forming the Western flank of the Andes. We will clarify this in our manuscript.

In fact 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 relate to the inversion of previous steep normal faults, not connected to each other, 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. Seismic profiles to the west of our field sites are too poorly resolved at depth - let's be frank on this! - and field observations are too sparse and local to favor one or the other model. 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 west-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 also favor the interpretation of a thrust system, as indicated in our final discussion (section 7.2), and the implications of this choice in terms of shortening were also discussed. We will revise our manuscript to further emphasize these points and the reasons for our final interpretations. In particular if parts of the manuscript are re-organized according to the recommendations of RC1, with a clearer separation between observations and structural interpretations, this can be easily done.

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 will try to add some of our structural measurements - as long as the figure keeps readable and these additions are meaningful. We remind here that additional field observations were located on the map of Figure S14

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.

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 detailed field measurements for our interpretations at the scale of the field sites. 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.

However, we will try to add 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 - as long as the figures keep readable.

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 were 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. We will make sure this point is clear when revising the manuscript.

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 Basin may have played an important role in localizing the thrusts at depth, or in controlling their steep geometry. This will be clarified in our revised manuscript.

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.

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

Our manuscript will be re-organized so as to better clarify these points. We propose to better separate field observations, and our choices of interpretations. 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 propose to move to section 6 all the structural reasoning leading to shortening calculations. By having all this together in one section, these various results should be clarified.

The discussion would require an integration of results in a regional cross-section through the Western Andes.

This has already been discussed previously

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
Line 17: They are not really restored
Right, since we do not model the entire sections. This will be suppressed, thanks for the correction.
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 from the extensive literature on the subject of Andean shortening across the whole range!

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 fear that adding the Pinchal Thrust could load too much the figure, we will however try to add this information and keep the figure readable.

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.

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

We would have liked to have more indications on why RC2 is not convinced by the existence of this anticline. As shown in Figure 7b, the axial plane and the two fold limbs re well defined at the landscape scale, with layers dipping eastward to the east and layers

dipping westward to the west. In the absence of any field indication of inverted series... we get an anticline!

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 propose to better separate field observations, sub-surface and deeper interpretations

As already mentioned, we have no 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 will be further clarified.

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

We can add 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. We rather choose not to do so.

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

We will try to add some dip angles, either measured in the field or deduced from 3D mapping, but will do so as long as the figure keeps readable. The objective of this figure is to offer a landscape view of the folds, dip angles are provided along the various field sections of Figure 5 where we feel they are more appropriate.

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.

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 propose to better separate in our revised manuscript surface observations and sub-surface (folds) deductions from deeper interpretations (thrust geometries).

See previous answers about tectonic inversion.

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.

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.

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.

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 will add a few words on this in the revised version of the manuscript.