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Tectonics

REPLY

10.1029/2020TC006564

This article is a reply to comment by Poli et al., <https://doi.org/10.1029/2019TC005696>.

Key Points:

- The comment on the DEM artefact is acknowledged, but it has no consequence on our conclusions about the post-Pliocene direction of maximum compression
- The comment on the use of uncalibrated ¹⁴C ages is acknowledged, leading to a shortening-rate reduction of at most 15%
- Our interpretations are in line with existing consensus on the crustal architecture and shortening-rate across the Eastern Southern Alps

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Reply to Comment by Poli et al. on “Fragmentation of the Adriatic Promontory: New Chronological Constraints From Neogene Shortening Rates Across the Southern Alps (NE Italy)”

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Abstract In a comment on our manuscript published three years ago (Moulin & Benedetti [2018] <https://doi.org/10.1029/2018TC004958>), the authors raise concern about so-called “mis-interpreted alluvial and tectonic structures,” and conclude that these issues “invalidate all the consequent interpretations on the deformation rates.” Two points raised by the comment are acknowledged, and affect our results only by slightly reducing (by at most 15%) the Late Pleistocene shortening-rate across the Eastern Southern Alps. The others issues raised by the comment are mainly dealing with the literature review, and alternative interpretations. Herein, we provide additional information to support our interpretations, and to relate our observations to existing data. Most importantly, our results do not call into question the existing consensus on both the crustal architecture of the Eastern Southern Alps, and the rate of recent shortening.

1. Introduction

In a comment on our study published almost three years ago (Moulin & Benedetti, 2018), the authors state that we misinterpreted both the alluvial chronology and some tectonic structures of the Eastern Southern Alps, concluding that our shortening rate estimates and geological cross sections are untenable.

The two points raised by the authors of the comment on the use of ¹⁴C-dates of published chronologies and on the Digital Elevation Model (DEM) artefact are acknowledged. However, they have little impact on our results (i.e., at most 15% difference with the published shortening rates). Most of the others points raised by the comment are mainly dealing with the literature review. Our active fault map is derived from geomorphic observations and agrees well with other existing tectonic maps built upon geological and geophysical data (e.g., Castellarin & Cantelli, 2000; Galadini et al., 2005; Poli et al., 2002), as well as our geological cross-section that has been balanced according to well-established methodologies widely used in other areas (Suppe, 1983). More importantly, we cannot agree with the qualification of some of our interpretation being “wrong” just because it challenges alternative interpretations.

2. DEM Artefact

We acknowledge the existence of artefacts, that we were not aware of when our study was published, caused by the imperfect juxtaposition of adjacent sheets of the topographical maps on the 5-m-DEM of the Veneto region (Mozzi, 2005). The southern branch of the Montello thrust that we mapped according to this DEM is thus attributed to this artefact. We emphasize however that, in our article, this “southern branch” was interpreted as a shallow splay of the Montello thrust, and not as a “new fault” (“we speculate that the two branches are the surface expression of two propagating thrust splays emerging from a single main thrust at depth, probably rooted at about 12- to 13-km depth (Castellarin et al., 2006; Galadini et al., 2005; Priolo et al., 2015)” in section 7.1 of Moulin & Benedetti, 2018). In addition, we only used this “southern branch” to emphasize that its apparent kinematic characteristics were very consistent with that of the northern branch (“uplift increasing westward, suggesting that the E-W section has accommodated more shortening than the NE-SW section (Benedetti et al., 2000)” in section 7.1 of Moulin & Benedetti, 2018), and then build on this common feature to infer that “the direction of Late Pleistocene to present shortening is N-S oriented, contrasting with the long-term NW-SE compression inferred from the large-scale structure of the belt” (section 7.1 of Moulin & Benedetti 2018). This latter sentence represents the main conclusion of our analysis of

the Montello thrust, and is entirely supported by the northern (undisputed) branch of the Montello thrust alone. Therefore, the comment about the DEM artefact has absolutely no impact on the conclusions drawn in our article.

3. Alluvial Chronology

The shortening rates we determined in our paper are based on the cumulative deformation of the surface a1 and on its age of emplacement. Surface a1 is preserved along the course of both the Torre and Cormor rivers and is equivalent to the Late LGM (i.e. Last Glacial Maximum) surface described in Fontana et al. (2008, 2014) and dated at 18–15-ka-BP using radiocarbon dating (Fontana et al., 2008). As pointed in the comment, the ^{14}C ages presented in Fontana et al. (2008) are uncalibrated dates. If we calculate calibrated ages using the IntCal13 calibration curve (Reimer et al., 2013), radiocarbon dates of the a1 surface would range between 22 and 18 ka cal BP. Using this calibrated interval, the shortening-rate is 1.3 ± 0.3 mm/yr (see calculation details in section 6.3 of Moulin & Benedetti, 2018), and is similar within uncertainties to our initial estimate, with at most a 15% difference (1.3 ± 0.3 mm/yr vs. 1.5 ± 0.3 mm/yr).

The comment stating that, in our article, the a3 surface was erroneously “related to the LGM peak,” and that rates of deformation determined from a correct mapping would be “at least 5 times lower than that presented in Moulin and Benedetti (2018)” is not justified. We did not consider the a3 surface as a post-26.5-ka surface and, above all, no rate was quantified from the a3 surface. We acknowledge that there is a typo in the stratigraphic columns of Figures 3 and 6 of Moulin and Benedetti (2018) that might be misleading, however the a3 surface is clearly referred as “pre-LGM” (see caption of Figure 6 in Moulin & Benedetti, 2018, for example) and placed below the 26.5–21-ka moraines in the stratigraphic columns of Figures 3 and 6 of Moulin and Benedetti (2018), thus older than 26.5 ka.

Our geomorphic map, and in particular the extents of the fan and the terraces, rely on several observations made from high-resolution topographic data (identification of terrace riser, classification of surface roughness, and correlation from longitudinal profiles, and on published geological information). However, in the comment, our mapping is considered unsupported because it does not match with ^{14}C dates reported in the explanatory notes of the geological map of Gemona del Friuli (Zanferrari et al., 2013) which suggest an age of >55 ka for the terrace of the Lagna river (mapped as a “a2 [22–18 ka] surface” in our article). As pointed in several studies, only high density of sampling and reliable stratigraphic order allows placing firm chronological constraints on the termination of an aggradation event such as a2 emplacement. For example, Hippe et al. (2018) performed high-resolution ^{14}C dating of a drilling core on the Cormor megafan sourced from the Eastern Southern Alps, and show that while ^{14}C dates are in very good stratigraphic order up to 30 m deep (between 17 and 30 ka BP), significant age inversion occurs at the base of the core, with ^{14}C dates over-estimating the true age by up to 25 ka, suggesting material reworking, surface erosion, and/or inheritance. Although the core of Hippe et al. (2018) cannot be directly compared to the ^{14}C dates of the Lagna river terraces presented in Zanferrari et al. (2013), it clearly suggests that LGM sediments sourced from the Friuli belt do contain organic material characterized by ^{14}C ages scattering around the radiocarbon dating limit. This could yield to an over-estimation of the true age of the deposits by up to 25 ka (Hippe et al., 2018). Any attempt to accurately date the abandonment of alluvial surfaces in this area would thus require a careful check of the stratigraphic order of numerous ^{14}C dates. However, this is not possible with the data reported by Zanferrari et al. (2013) since the two sampling sites from which the authors of the comment argue that we “erroneously attributed” a young age to the terraces of the Lagna river, contain only five samples with no information regarding sampling depth (locality Cergneu in Table 1 of Zanferrari et al., 2013), and dispersed between two sites separated from each other by about 1 km, making any comparison between these two groups of samples very risky. We thus argue that those data are insufficient to rule out our chronology based on long-profile correlation and classification of surface roughness. It is also important to point out that the Lagna terraces were not used to quantify tectonic deformation, and their precise chronology has thus no impact on the conclusions of our article.

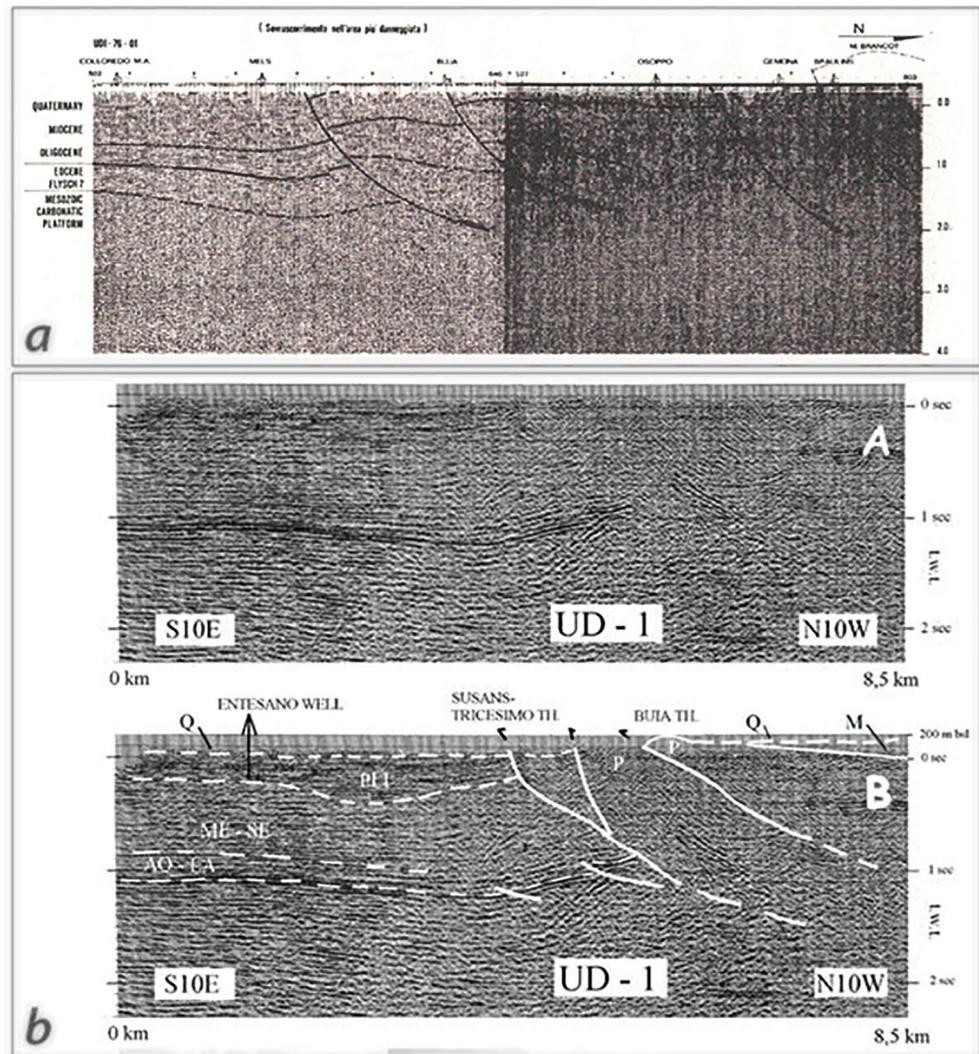


Figure 1. (a) interpreted seismic profile of Amato et al. (1976) from which the Buia thrust has been recognized and mapped. The raw data (i.e., non-interpreted profile): have not been provided by Amato et al. (1976). (b) Seismic profile (upper) and interpreted profile (lower) across the Buia and Tricesimo thrusts from Galadini et al. (2005). Note that the N10°W–S10°E direction of the profile makes an angle of about 40° relative to the ideal N30°E–S30°W direction required to cross the Tricesimo thrust orthogonally.

4. Tricesimo Thrust

The comment argues that the nomenclature that we used for the frontal Tricesimo thrust, and its relation with the 1976 Friuli earthquake is not appropriate. In particular, they state that we misinterpreted the Tricesimo thrust because we considered it as the active frontal thrust of the Eastern Southern Alps, whereas Cheloni et al. (2012) suggest the active frontal structure is instead the Buia thrust based on modeling of geodetic data set re-measured after the 1976 Friuli earthquake.

We first emphasize that our study is the first that has systematically mapped the geomorphic surface expression of active thrusting at the front of the eastern Southern Alps, whereas until then thrusts were mostly mapped from subsurface evidences based on seismic profiles (Amato et al., 1976; Galadini et al., 2005; see Figure 1). Seismic profiles allow constraining the geometry of faults along vertical slices, but unless a dense series of parallel profiles are available, which, to our knowledge, is not the case at the front of the Eastern Southern Alps, the fault geometry as seen on a map is only inferred. In the fault database used by Cheloni et al. (2012), both the Buia and Tricesimo faults have been mapped from seismic profiles (Figure 2), and

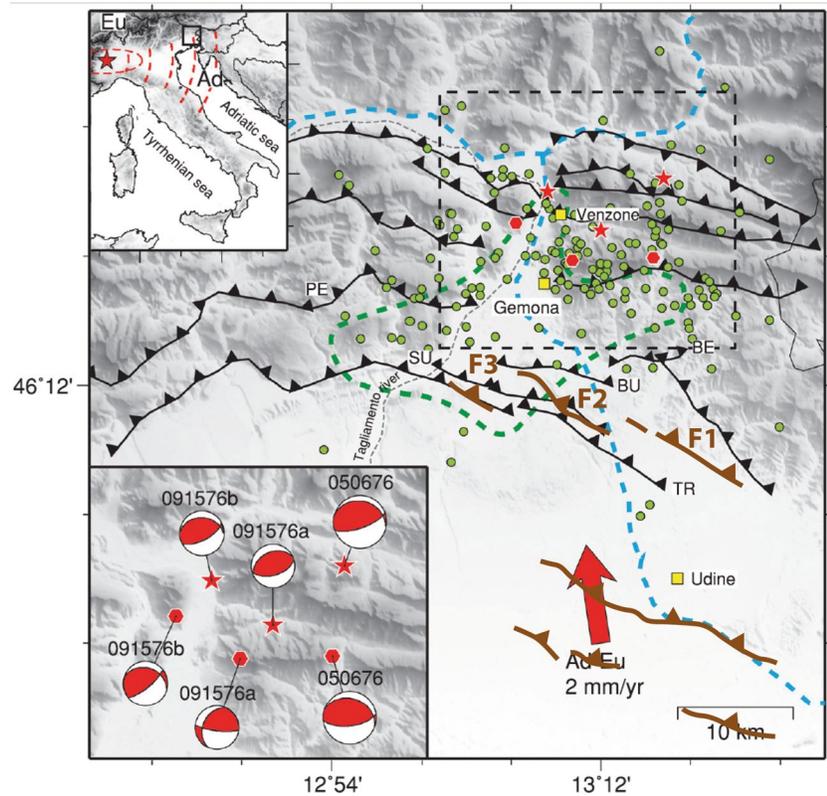


Figure 2. The active fault mapping (in brown) as in Moulin and Benedetti (2018) is superposed on the tectonic map of Cheloni et al. (2012). Note how the frontal active faults we have mapped (three strands labeled from 1 to 3) match with the different structures of Cheloni et al. (2012) (TR = Tricesimo thrust; BU = Buia thrust) when moving from ESE to WNW.

their surface projection thus results from the interpolation of very few discrete observations. As a matter of fact, these two thrusts are mapped as blind thrusts (dashed lines) in the Geological maps of Udine and of Gemona del Friuli (Zanferrari et al., 2008, 2013), because the faults have not been observed at the surface.

On the contrary, our fault mapping underlines geomorphic evidences that are spatially continuous. Therefore, the surface segmentation of the fault evidenced by our work represents a level of detail, which cannot be attained by geophysical investigations. Indeed, the frontal thrust is composed of three left-stepping fault strands (labeled from F1 to F3 in Figure 2). While F1 does not appear in the map of Cheloni et al. (2012), its recent activity was recently confirmed based on seismic lines and paleoseismological investigations in Falcucci et al. (2018, named Colle-Villano thrust in the study). The two others fault strands we mapped are essentially consistent with the structure named Tricesimo thrust in Cheloni et al. (2012) (F2 and F3 in Figure 2).

We stress that the leveling data used by Cheloni et al. (2012) to model the source of the 1976 earthquake are not inconsistent with the Tricesimo thrust being the causative source. Indeed, the most frontal leveling benchmark which revealed significant co-seismic uplift, is located immediately S of the Buia thrust (red arrow in Figure 3), while the next benchmark is located immediately S of the fault strand F2 of our Tricesimo thrust (Figure 3). In between, there is no additional data (Figure 3). Thus, considering that between those two fault strands, the one exhibiting geomorphic evidence of reactivation might represent the surface expression of the 1976 source is an interpretation followed in many similar situations worldwide and not in disagreement with Cheloni et al. (2012) data set.

In addition, and considering (a) the poor quality of existing seismic profiles (Figure 1), (b) the small magnitude of the Buia thrust displacement inferred on those profiles (e.g., Galadini et al., 2005; Merlini et al., 2002; Poli et al., 2002), and (c) the fact that a ramp-decollement geometry leads to a pattern of folding broadly

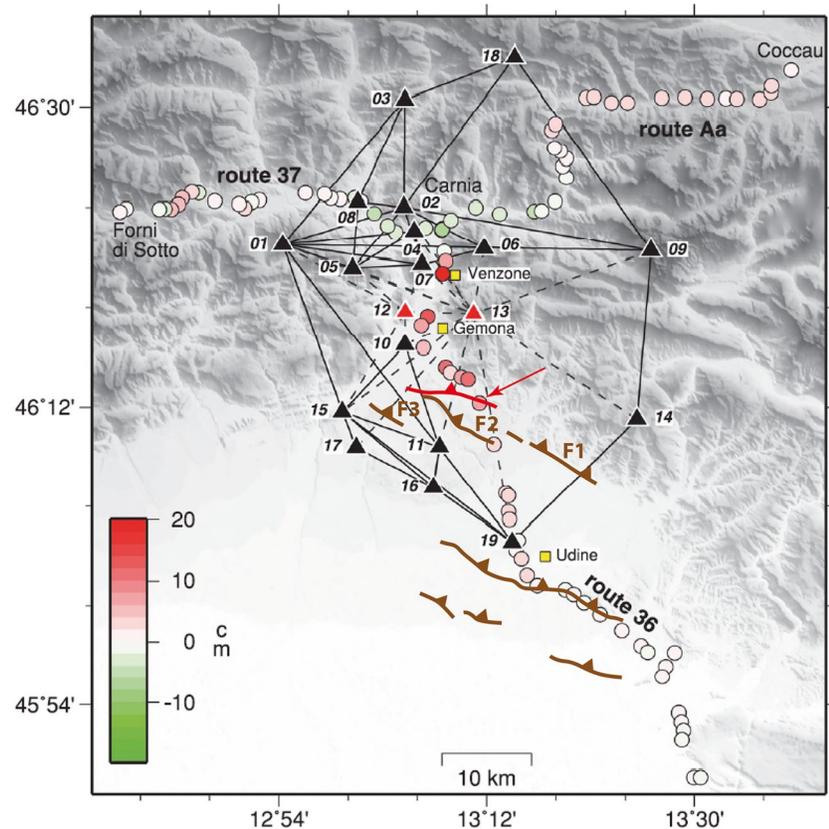


Figure 3. The active fault mapping of Moulin and Benedetti (2018) (in brown) has been superposed to the leveling data map of Cheloni et al. (2012) (circles are color-coded as a function of 1976 “co-seismic” vertical displacement: see scale on the left-hand side). The red arrow shows the most frontal leveling benchmark which revealed significant vertical motion (see also Figure 8 of Cheloni et al., 2012). The red thrust reports the geometry of the Buia thrust of Cheloni et al. (2012) (see also Figure 1).

similar to a blind-thrust (Figure 4), we have proposed in our article that the Buia anticline might be related to a deep ramp of the Tricesimo thrust (labeled R'1' in Figure 9 of Moulin & Benedetti, 2018), and explicitly mentioned that alternative interpretations are possible (last paragraph of section 6.3 of our article).

5. Udine Thrust

The comment states the Udine-Buttrio thrust is not a splay of the Pozzuolo thrust as we interpreted. Among the four articles cited by the comment to support their assertion, only one actually contains an image of a seismic profile in the area of interest (Figure 5 in Zanferrari et al., 2013). This seismic profile makes an angle of about 70° with the trend of our cross-section, and hence runs at very low angle with the strike of the thrust. We thus consider that this seismic profile is not appropriate to place firm constraints on the geometry of the Udine thrust, particularly since the finite displacement associated to it is very small.

Moreover, the aspect-ratio of the folds constrained in our study (Figures 4 and 8 in Moulin & Benedetti, 2018) strongly limits the range of plausible fault geometries, following classical widely employed methods, for example, in the Himalaya-Tibet system (Lavé & Avouac, 2000; Meyer et al., 1998). In addition, the comment suggests that we did not consider the Palmanova thrust in constructing our fault geometry, which is a mistake since our fault geometry has been explicitly constrained by the shape of the Palmanova anticline (section 6.2 in Moulin & Benedetti, 2018).

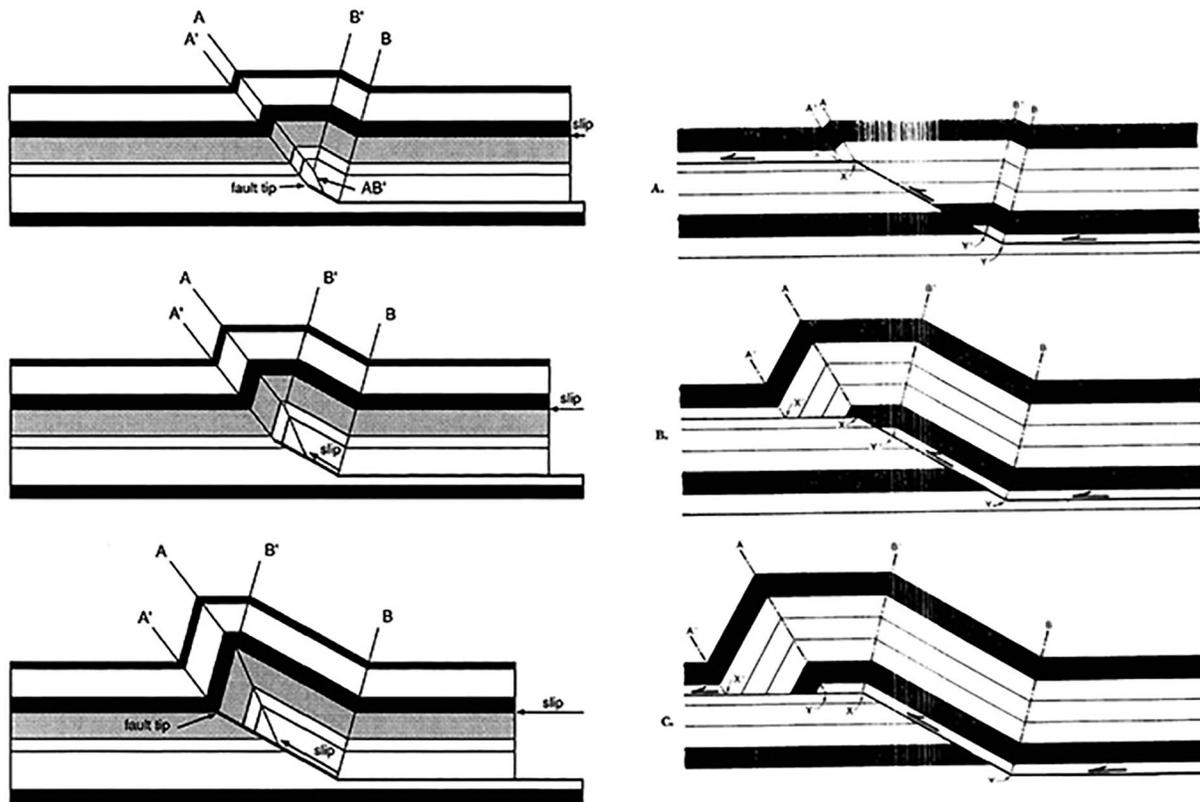


Figure 4. sketches showing the similarity between the anticline geometry produced by slip on a blind thrust (left-hand side: Suppe & Medwedeff, 1990) versus slip across a ramp-flat (right-hand-side: Suppe, 1983).

6. Geological Cross-Section and Strike-Slip Faulting

The comment argues that the Idrija-Ampezzo strike-slip fault system should have been incorporated in our cross-section. The objective of our cross-section was to provide an estimate of the total shortening absorbed across the Eastern Southern Alps, based on the insights of our short-term analysis (see section 6.4 of Moulin & Benedetti, 2018 for more details). In that purpose, we have voluntarily ignored what we have considered as “minor strike-slip faults” (caption of Figure 9 in Moulin & Benedetti, 2018). In particular, we have considered the Idrija-Ampezzo as “minor” for two reasons.

First, the Idrija-Ampezzo system has been inferred to be the continuation of the Dinaric Idrija fault (Poli & Zanferrari, 2018). In Slovenia, this fault accommodates about 5 km of total geological displacement (Moulin et al., 2016), which is quite small when compared to the about 40 km of total shortening absorbed across the Eastern Southern Alps. Thus, few kilometers of strike-slip displacement in a direction parallel to the main thrusts would significantly affect the determination of finite shortening only if huge lateral gradients of crustal shortening would exist. Such gradients are not documented in the Eastern Southern Alps, this is why we choose to neglect the effect of strike-slip faulting when determining the total shortening.

Second, strike-slip faults in this part of the Eastern Southern Alps have always been considered as minor, the consensus being that the crustal architecture is thrust-dominated and composed of low-angle N-dipping structures across the entire width of the belt (e.g., Galadini et al., 2005; Merlini et al., 2002; Poli et al., 2002; Schmid et al., 2004). Our cross-section (Figure 9 in Moulin & Benedetti, 2018) is in line with this consensus. By contrast, the cross-section proposed by the authors of the comment (their Figure 5), first published in Poli and Zanferrari (2018), clearly calls into question this consensus, since the Idrija-Ampezzo strike-slip fault is drawn as a sub-vertical structure going down to depth of at least 25 km and interrupting the overall architecture. The only argument used by Poli and Zanferrari (2018) to “hypothesize” this huge depth of the Idrija-Ampezzo system is a 3 km vertical throw of the magnetic basement observed beneath the

Tagliamento valley. Yet, it is not necessary to introduce a crustal-scale strike-slip fault to reproduce this throw: in our cross-section for example, this throw is accounted for by the total displacement accumulated on the ramp R3 (Figure 9 in Moulin & Benedetti, 2018). It should be noted that, in their comment, the authors do not provide additional arguments to call the “thrust-dominated consensus” into question.

The comment also suggests we have underestimated the importance of the Fella-Sava fault in our geological cross-section. East of our studied area, in Slovenia, the Sava fault represents a large displacement structure, with a total right-lateral displacement estimated between 30 and 60 km (Fodor et al., 1998). The timing of activity is post-mid-Miocene (Vrabec & Fodor, 2006), therefore in a period where active thrusting was already ongoing in the Eastern Southern Alps. According to Vrabec and Fodor (2006), the role of the Sava fault changes from East to West: it is part of a strike-slip duplex within the Periadriatic fault system in Slovenia, whereas it becomes a right-lateral back-thrust (namely the Fella-Sava fault) in the Eastern Southern Alps. Therefore, the importance, total displacement, and geometry of the Fella-Sava fault cannot be compared when moving from Slovenia to the Eastern Southern Alps. In particular, we are not aware of any strike-slip displacement estimate along the Fella-Sava fault at the longitude of the Eastern Southern Alps. Do the authors of the Comment consider that 30–60 km of post-mid-Miocene strike-slip displacement have accrued along the Fella-Sava fault in the Eastern Southern Alps? If so, they should have explain on which data this assertion is based. Moreover the geometry of the Fella-Sava fault displayed in our cross-section is very similar to the one of Poli et al. (2002), Galadini et al. (2005), or Merlini et al. (2002) along the same transect. To our knowledge, these publications have not been subjected to comments. Thus concerning the Fella-Sava fault, our cross-section is fully consistent with the existing literature.

The comment finally asserts that the Periadriatic fault, “*represents a polyphased back-stop* (Castellarin et al., 1992, 2006; Schmid et al., 1996)” making the fault geometry we assumed (Figure 9 in Moulin & Benedetti, 2018) unsupported. However, Schmid et al. (1996) and see also Rosenberg and Kissling, (2013) suggest that the Periadriatic fault is no longer a back-stop since the Middle Miocene, when the Adriatic lower crust started to indent into the crust of the Central Alps. This indentation necessarily produced a deformation of the deep sections of the Periadriatic fault, the latter flattening/bending because of northward entrainment by the Adriatic lower-crustal wedge (see cross-sections by Rosenberg & Kissling, (2013); Schmid et al., 1996, 2004, for example). In such models, thrusting in the Southern Alps is kinematically related to lower-crustal wedging (Schmid et al., 1996), and hence to the deep deformation of the Periadriatic fault. The data presented in our article are far from providing insights into this debate, which is why we explicitly stated that “*how the three main thrusts connect with the Periadriatic fault at depth is not discussed in the present study*” (caption of Figure 9 in Moulin & Benedetti, 2018). In any case, the amount of total shortening derived from our cross-section (at least 43 km) is consistent with previous interpretations (e.g., 40–55 km according to Castellarin et al., 2006), and thus does not impact the general conclusions of our article.

Finally, we emphasize that our cross-section includes the axial surfaces of folds, from which anyone can check the consistency between surface structures and the proposed fault geometry at depth (Suppe, 1983). To our knowledge, none of the geological cross-sections that have been published so far across the Eastern Southern Alps (including the one presented in Figure 5 of the comment) has included those constraints, and most of them are affected by kinematic inconsistencies (in particular, the relation between cutoff angles and ramp angles is rarely respected). In addition, the main assumptions underlying our cross-section have been explicated in details (section 6 in Moulin & Benedetti, 2018), in particular regarding the lithologic controls on the ramp-flat geometry.

7. Conclusions

In conclusions, the misinterpreted DEM artefact and the use of uncalibrated ^{14}C dates in our paper affect our results by decreasing of at most 15% the shortening-rates initially constrained (1.3 ± 0.3 mm/yr vs. 1.5 ± 0.3 mm/yr). This has a very limited impact and does not affect the general conclusions drawn in our article.

All the others critics raised in the Comment about the interpretations and the geological cross-section presented in our study, which were made with explicit assumptions, are unjustified. None of the statements in the Comment discredit our inferences. As science advances, and new data are acquired, it is expected that

inferences made by scientists are supported or, on the contrary, abandoned, however the right way to do it, in our opinion, is under the form of new publications presenting new data set.

Acknowledgments

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