We thank the reviewer for his/her insightful comments. This is most appreciated and will help improve the revised version of the MS.

The key concern is about the constraints of the input parameters in our model. Specifically, concerns are raised about the inferred tectonic stress from the fracture orientation without properly describing the fractures. We acknowledge that providing a comprehensive description of the fractures would increase our understanding of the rationale behind our tectonic stress inference. We will include this aspect in the revised version. However, as part of this project, another paper dealt with the background fractures at the Latemar Carbonate Platform (N. Italy). There, we comprehensively described the fracture geometries, kinematics, driving factors, and connectivity. This paper is presently under revision.

Specific comments/questions	Response
The presented investigation uses the outcropping network geometry as input for geomechanical and flow models, that is fair, but a key question could regard the timing of fractures formation. In my opinion the authors must clarify and show the evidence that relate the fracture formation to time. 1. what is the evidence that exclude very late (i.e. during exhumation) formation of some of the observed fractures? 2. Since no crosscutting relationship are analysed, why should we think that all fractures experienced both tectonic stress regimes?	We used the outcropping network geometry as input for geomechanical and flow models. We agree that capturing the timing of the fracture formation is essential. However, for this study, our model did not consider the formation and/or growth timing of new fractures in the study area. Instead, it considered that the already-developed fractures were either sheared, opened, and/or closed during the tectonic episodes. In the Latemar, the two major tectonic events are associated with subsidence-related deformation in the Late Triassic and Early Jurassic times, shortly after the fractures were formed. This means that most of the fractures analysed were affected by this tectonic episode. In addition, a later Alpine compression during the Neogene overprinted the whole fracture network. This means new fractures may have formed during and/or after these tectonic episodes, including during the late exhumation. However, the formation and growth of these new fractures were not mimicked in our model and are clearly beyond our model and study scope. We only focused on already-formed fractures, as they are today. Detailed crosscutting relationships of the fractures were analysed in Igbokwe et al. (2022), and we will reflect some of this analysis in the revised version.
Line 255 "the arrangements orientations and the stress fields during the development of the fractures are documented". Stress inversion technique must be explained here.	We will explain the stress inversion technique.
Line 257 "In the Valsorda Valley (Fig. 3), carbonate outcrops are affected by minor reverse	

conjugate faults dipping at low angle (< 30o) to bedding." I strongly suggest the authors to clearly show displacements and kinematic indicators supporting this.	We agree and will show the displacement and kinematic indicators relating to the minor reverse conjugate faults.
Line 262 "On the other side, at the flat-topped Latemar, on the sub-horizontal (pavement) outcrops, fractures also form conjugate patterns, exhibiting dextral and sinistral displacements (Figs. 3 and 5)." I think that authors should highlight this evidence in figure 3 and 5. Line 286 "Overall, two deformation phases were observed and documented" I think that authors must better document this.	Yes, that is a good point; we will highlight the evidence and document further the overall two deformation phases observed in the study area.
Line 298 "Although stylolite tends to hinder fluid flow (Boersma et al., 2019), observations in figures 3, 4 and 5 show they can enhance fluid movement". Even if I do think that stylolite generally hinder fluid flow I would suggest reading the paper by Heap et al., 2018 were it is proposed that stylolites can be considered as conduits for flow. I would also ask the authors to clearly describe and show the observations in figures 3, 4 and 5 proving that they can enhance fluid movement.	We agree with Heap et al. (2018) that stylolites can serve as fluid conduits. Stylolites and fluid flow will be detailed in Figures 3 through 5. However, for the geomechanical modelling (or numerical workflow), we treated all modelled features as reactivated fractures that control the fluid flow. In section 5.3 (Implication) of our work, we advanced the argument that "because of the effects of weathering and exhumation, the distinction between open fractures, veins, stylolites and shear fractures was not made in the model. Making these distinctions in our modelling workflow will complicate the whole process and is beyond the scope of this study.
Line 321 "Slight modifications and/or extrapolations of the fracture's original pattern were implemented to maintain the fracture topological connectivity" I would ask the authors to be more specific .	The revised section will provide more detail. Thank you for pointing this out.
Line 338 "When the fracture is in a closure condition, and sufficient loading is acting in the tangential direction, the fracture may slip. The slip and stick conditions of the fracture are determined based on the classical Coulomb's friction law". Coulomb friction depends on several parameters and it can greatly vary form host rock to host rock (see Collettini et al., 2019). It would be appropriate to have more information about the adopted mechanical properties.	Yes, that is a good point. That must be detailed in the revised version of this paper.
Line 411 "We have adopted the matrix permeability value of 2 x 10-15 m "Are there any	

other constrain eventually based of data from actual samples of the Latemar area or from similar lithologies?	We adopted the matrix permeability value of 2 x 10^{-15} m from the results of Whitaker et al. (2014) based on carbonate samples from Latemar. Generally, carbonate rocks have very low matrix permeability with average values ranging from 2 to 4 x 10^{-15} m^2. We will detail the rationale based on other similar lithologies and explain why we chose the matrix permeability value we used
Line 437 "These tectonic episodes are constrained to the NW-SE and N-S shortening (compressive) directions with an assumed maximum magnitude of 50 and 160 MPa for the subsidence-related and Alpine deformation stages, respectively." I can't really see how authors constrain these stress values. Authors successively state that they refer to the The World Stress Map database, however as far as I know that paper is more focused on stress orientation that stress magnitude, moreover here there is no mention about the assumed depth so I found these stress values not clear. An explanation is given only at line 625, I think that it would be better to report and exlpain here the used input values.	We will move the explanation regarding the input parameters from line 625 to line 437. The paper is focused more on the impact of the far-field stress orientation rather than stress magnitude values, which also affect the final effective permeability calculations. However, the stress values mentioned here only indicate approximations from similar study areas with the same geodynamic conditions. This means that changing these values can change the final calculation of the effective permeability. Still, the workability of the model remains stable and robust and can compute any given matter. Secondly, the numerical analysis (or modelling) is in the 2-D, constraining the effect of lateral expansion resulting from the overburden stress of the fracture domain. Considering depth implicates overburden stress. We advanced this argument in lines 828 – 839.
Line 445 "The constitutive parameter values are Youngs modulus 25 GPa and Poisson ration 0.30 (see Table 1 for detailed parameters)." Once again I would like the authors to specify where these input data come from. Moreover, if perfect elasticity is supposed in the model and dynamic parameters are used, they can differ a lot from static ones in particular for dolostone samples (see Trippetta et al., 2013). In any case a reference for such used values must be given.	We agree with the reviewers and will specify where the input parameters came from with relevant citations.
Technical corections Scales are absent in figures 1d and e I have a problem also with the scales in figures 3 A and B. According to the shown scales outcrops are ~ 500 m long each. It is correct?	Consider it done. Needs to be verified.

Moreover the legend must be improved, I can't really distinguish between Tectonic styliolites and Perp. fractures Line 255 and sub-horizontal (pavement) outcrops. Maybe "that" is missing here	Yes, thank you, we will do.