Dear Editor,

Thank you for your helpful comments on the manuscript se-2020-30. We have addressed all the issues raised by you in our replies below and in most cases have modified the manuscript accordingly.

Editor's comment: *1 - Please remove the new text about KIC, which is inappropriate to the study case. First this rationale implies too much speculation about the initial crack length, which is not observed or at least not presented. Second, you try to estimate the stress with a LEFM equation in which you have two unknowns (Y and S3). Third, you give phenocryst size to define a potential length of initial crack (which is very speculative) to use this equation which is for homogeneous elastic properties. In you rationale, the crack length is controlled by the penocrist and host heterogeneity and therfore not controlled by the elastic stresses. Then the rational is not suitable.*

Authors' response: Accepted and deleted from the manuscript.

Editor's comment: *You can let the following text and mention briefly how S3 has been estimated if the estimation is proper (LOT ?): « It may also be noted that, previous studies by Mondal and Acharyya, 2018, conducted in Chitradurga Granite, in close vicinity of the study area also regarded the magnitude of σ3 ~10 MPa, to be a good estimation. Combining these estimations with the results obtained from the present studies, we constrained the value for σ3 ~12MPa. »*

Authors' response: Accepted (The estimation was based on the fracture toughness values).

Editor's comment: 2 - *Please clearly answer to the following reviewer#2 main comment that you have not considered in your revision:*
Reviewer #2 mentionned : « In page 14 (lines 273-274), it is mentioned: "Tensile strength of metabasalt (∼12MPa; obtained from BTS studies) indicates that the minimum principal stress (sigma3)has to be sigma3≥12 MPa." Thereafter, this value is selected as magnitude for theminimum principal stress for the stress reconstructions (e.g. Fig. 7). I am particularly puzzled by the reasoning. The condition for tensile fracturing is sigma3 – pore pressureless or equal to the negative value of tensile strength. That is if sigma3 = 12 MPa thenPf has to be equal or higher than 24 MPa... Starting from there could the authorsexplain how they constrain sigma3? »
I completely agree with reviewer 2, there is again a problem of rationale, or unclear/incomplete explanation. First, I do not understand how you use the BTS to estimate the minimum principal stress. The BTS gives you an estimate of the strength (a negative stress value) but this value alone can not give you the full minimum principal stress (Sigma 3, which must be positive at depth). Then BTS gives you T0 = -12 MPa. If you manage to estimate S3 (how ?) as equal to 12 MPa (then positive), then Pf must be higher or equal to S3+(-T0), then 24 MPa as suggested by the reviewer.

Authors' response: We are extremely grateful to you for pointing out this sections that we failed to communicate in a better way. We feel there has been some drawbacks from our side that we failed to explain our viewpoint. In the following we have tried to clarify your concerns regarding the manuscript.

The equation that we could not justify $\sigma_3 - P_f \geq T$ (after Gudmundsson, 2011) explains the formation of hydro-fractures at any depth. In our study, we have shown the role of pre-existing anisotropy and fractures/faults in channelizing fluid flow. Fluid plays a vital role in reactivating the pre-existing fractures, but the formation of fractures is driven by the tectonic stresses. The P_f is negligible when the fracture system consisting of the riedel shear components, P, Y, R, T, X and R′ shears are formed due to the sinistral movement along CSZ, that acted as the shear boundary under a WNW-ESE directed D3 compression. Therefore, the condition that we presumed, satisfies the equation $\sigma_3 \geq T$ (You, 2015). The studies by Ishii (2015) have shown that the laboratory-measured tensile strength is almost similar to the tensile crack initiation stress (σ_3). Eidelman and Reches (1992) have also shown that the minimum principal stress required for the generation of fractures in stiff inclusions (such as pebbles) may be considered to be identical to the laboratory measured tensile strength of the rock. Therefore, we have considered that the minimum principal stress (σ_3) required for fracture initiation in the metabasalt of the study area are equivalent to its tensile strength. In the present study, we used BTS to quantify the tensile strength of the metabasalts (T=12 MPa). It may be noted that, for quantifying σ_3, we only considered the magnitude of the tensile strength here. We agree that the magnitude of σ_3 could be greater than the one we considered, but as we are dealing with the limiting condition this is the minimum magnitude of σ_3 required to overcome the tensile strength of the rock and generate a fracture in the rocks.
In the revised manuscript the changes are marked in blue color in line number 296-305.

Editor's comment: *3 - You mention « CORRECTED and INCORPORATED the suggestion » for the following comment I mentionned: « Provide more justification in the discussion about the deformation mechanism related to the magnetic fabric. Can we interpret the magnetic fabric as non-coaxial (simple shear) or multi episodic deformation (2 poles on the stereogram), and then having a shortening oblique to the foliation ? »*
Please, clearly indicate in which lines you did the explanation, I do not find dit in the Ms.

Authors' response: We would like to thank you for raising this issue and giving us an opportunity to revise the same. Yes, we extremely apologize for this inadvertent error. We had added some words that somehow got deleted while editing.

The NNW-SSE to NW-SE oriented magnetic fabric has developed during regional D1/D2 deformation on account of ~NE-SW directed shortening and that is characterized as a co-axial deformation. Recent studies by Mondal (2018) also suggest that, the same ~NW-SE oriented magnetic fabric in the adjacent Chitradurga granite is a product of co-axial deformation (D1/D2) manifested by NE-SW shortening. Since the magnetic fabric has a range of strike orientation (NW-SE to NNW-SSE), thus producing a variation (spreading) of its pole in the lower hemisphere equal area projection.
The line number 384-391 in section 5.1 of the revised manuscript reads as follows: "This implies that the fabric in metabasalts of the study area must have been controlled by the regional D1/D2 deformation under ~NE-SW directed shortening that generated the field foliation in the meta-sedimentary sequences. Recently Mondal and Mamtani (2014) and Mondal (2018) interpreted that, the ~NW-SE oriented magnetic fabric in the adjacent younger granites of the study area are the result of co-axial (pure shear) deformation. Since

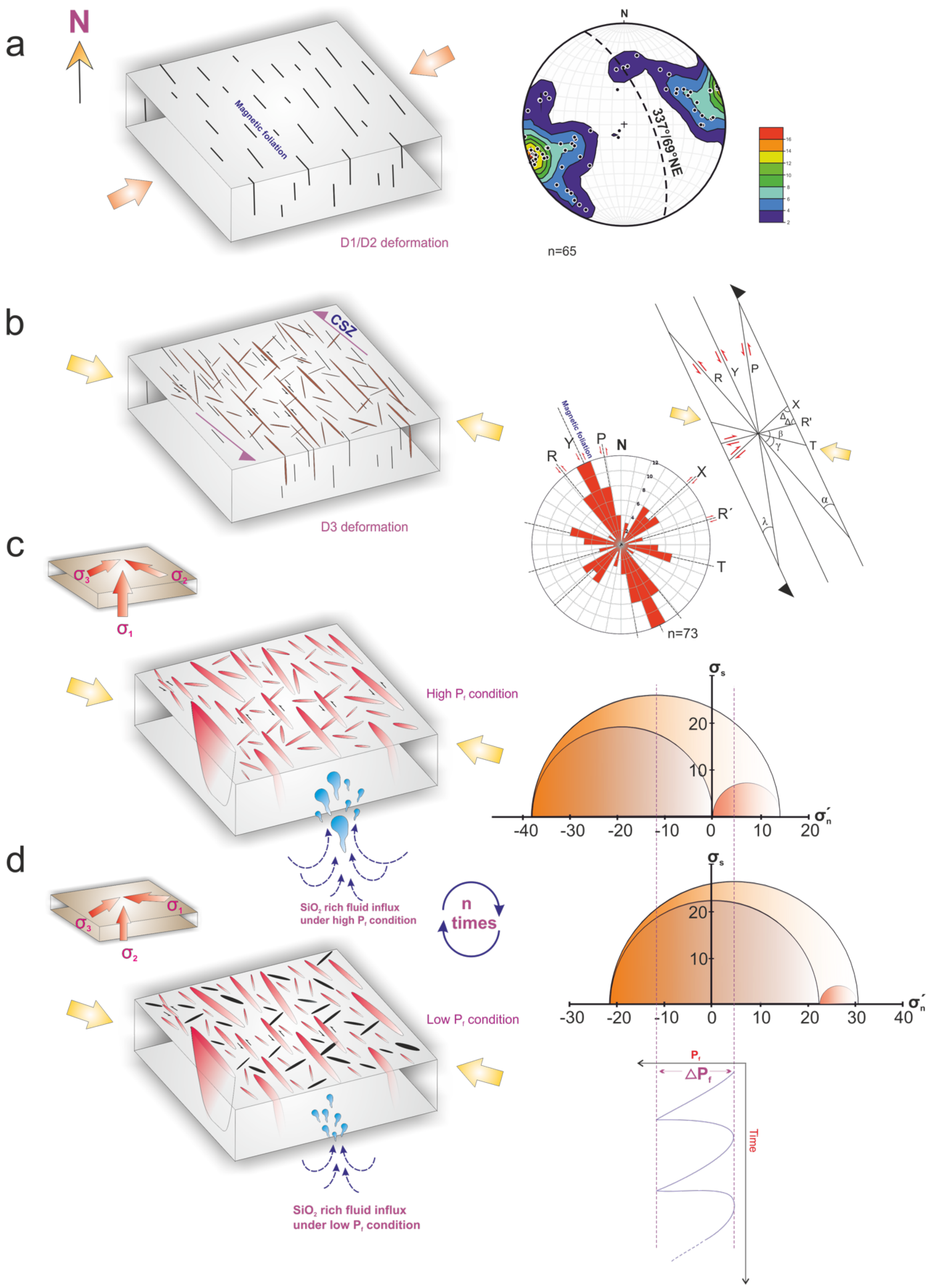
these fabric in metabasalts and adjacent younger granites are found to be parallel and manifested by the same NE-SW shortening, it is inferred that magnetic fabric in the metabasalts of the study area are also the result of co-axial deformation."

Editor's comment: *4 - The wing crack geometry has not been corrected in the block diagrams of the last figure although you mention that you did the revision. The wing crack geometry you drawn is still kinematically inconsistent with the sense of shear on the reactivated veins in the three schemes and at different places in a same sheme. The wing cracks you drawn around the reactivated veins suggest right lateral shear, which is inconsistent with the far field stress. Please revise the wing crack position in all the diagrams to adapt them to sinistral shear on the slipped veins, and to be consistent with the remote stresses.*

Authors' response: You have also raised your concern regarding the wing cracks in Fig. 11 of the revised manuscript. However, we would like to mention here that all the cracks that you have mentioned are not wing cracks. Moreover, these are the various components of riedel shear system that are reactivated during vein emplacement. In accordance with your recommendations again we have modified the same (see below) for the clarity of the manuscript.

With the above revisions we hope that all the questions have been addressed and the revised version of manuscript is to your satisfaction.

Thanking you
Yours sincerely
Tridib Kumar Mondal
(Corresponding author)


a
N
Magnetic foliation
D1/D2 deformation
N
337°/69°NE
n=65
b
CSZ
D3 deformation
R
Y
P
X
R'
T
α
λ
β
γ
Magnetic foliation
N
R
Y
P
X
R´
T
n=73
c
σ3
σ2
σ1
High Pf condition
SiO2 rich fluid influx
under high Pf condition
σs
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-40
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Low Pf condition
SiO2 rich fluid influx
under low Pf condition
σs
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-30
-20
-10
0
10
20
30
40
Pf
ΔPf
Time

Figure: 11