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Interactive comment on "3D numerical modelling of the re-distribution of partially molten lower crust rocks in relay zones between faults during transtension: Implications for the Sefwi terrane, SW Ghana" *by* Xiaojun Feng et al.

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Feng and coauthors present a contribution based on ten 3-D models inspired by a shear zone corridor within the Leo-Man craton in SW Ghana. The design, computation and analysis of such highly complex models requires a very thorough approach unfortunately not mature in the present submission. A general comment points here at the major concerns arisen, before a more detailed list is given for the first part of the paper. The results are not discussed in details, as I suggest that models should be run with a more realistic set up. General comments 1- Numerous references are made

to a Feng et al., 2018 paper actually under review at Journal of Structural Geology. This makes the present manuscript non self-consistent, and prevents from checking how reliable is this reference. 2- The overall modelling strategy is not clearly stated at the beginning : What parameter is tested ? It happens that both initial geometry and boundary condition velocities are varied, but it is unclear why these parameters are the ones expected to drive the studied crustal domain behavior. What are the geological evidence that these are the parameters to test ? 3- The presentation of geology and natural data is not thorough enough and even unrelated to model design. How were the figures in the models deduced from natural data ? Especially, velocities tested imply that the finite offset of shear zones is known as well as the drection of their activation. None of this is elaborated in the manuscript as submitted. 4. The set-up of models and especially the computation of melting is probably irrelevant : constant and very low melting temperature is chosen, positive volume change has seemingly been conisdered while water-present melting implemented is expected to have a highly variable slope in PT space and therefore an opposite effect on density at least for the pressure domain explored in the models. The geological meaning of 2 km thick, newtonian light planar seeds as pre-existing "faults" is not supported by any data and not tested in terms of mechanical stability. The initial set-up with a lower crust lighter than the middle crust also would need to be explained and tested. 5. One parameter not discussed here is the velocities applied scaled to the Stokes velocity of raising lower crust. How were the velocity values chosen ? On field evidence ? On mechanical consideration ? The finite velocities (sqrt(Veˆ2+Vsˆ2)) are 2, 2.2, 2.8, 2.2 and 4.1 for the series 1 to 5 respectively ... what is the effect of this variation ? Can the results be compared ? 5. The recurrent missuse of literature (Brun et al., 994 do not include preexisting faults, Gerya et al., 2008 do not use a constant temperature melting strategy, Le Pourhiet et al., 2004 do not use a 2 km-thick fault as initial condition ...) and the approximative language when describing data make it very uncomfortable to follow the proposed approach. 6. Eventually, the figures (limited in number and data presented) do not pay tribute to the complexity of models. Strain rate invariant maps, vertical profiles, P,T paths, rheological envelopes ... would definitely improve the paper.

Detailed comments and remarks on the text: P1L12 "major" rather than "main" P1L16 "occurs" please keep present throughout the manuscript. What is the difference between "normal component of velocity boundary conditions" and "ratio of extension rate to shear rate applied at the boundaries" ? They sound the same phrased this way. P1L19 "transtension direction" transtension is a strain pattern, not a single direction. Do you mean "horizontal maximum stretching axis" ? P1L19 "low-density upper mantle" is perhaps an awkward description, unless you specify low regard to lithospheric mantle, for instance, which is actually absent in models. P1L22 "of up to amphibolite-migmatite facies rocks" P1L23 and further It is unclear how these conclusions arise from the modelling or from other data. It is not stated at this time that stretching duration or strain rate was tested so that an actual duration for the natural case can be deduced from comparison with models.

P1L29 "Pre-existing faults commonly act as preferentially re-activated sites [...] and play a dominant role in concentrating high strain" What other "sites" than faults could actually be reactivated ? And a fault is by definition a localized strain zone, I guess it was known before 2004. Rephrase, or delete this pleonasmic statement. P2L2 "fracture propagation of their surrounding rocks" Unclear, do you mean "fracturing" ? How does the network propagate if fractures do not propagate ? P2L7 "The emplacement of partially molten rocks" Do you mean emplacement of melt ? If partially molten rocks are considered here as a coherent mechanical phase, then they are "strained," they can "flow", but I would reserve emplacement for intrusion or percolation of melt through migmatites for instance. P2L9 "promoting the processes of the emplacement" What processes are meant here ? How faults can promote viscosity and density contrast mentionned above ? Please rephrase. P2L10 Feng et al, 2017 reference is not listed in the reference list. It seems to refer to Feng et al., 2018, actually under review for publication in Journal of Structural Geology. This reference seems important in the present manuscript (cited 6 times) and this is actually a major concern about 1- the ex-

act content and validity of this complemental paper and 2- how original and exclusive the present contribution is. P2L13 As far as I know it, Brun et al. (1994) study did not include pre-existing faults in their analogue models, and do not show how inheritance affects MCC formation. Lavier et al., Geology, 1999 would actually be much more relevant here for instance. P2L24 This first presentation of the Sefwi domain should refer to a figure, at least for location. P2L26 "Kenyase shear zone_" and "Ketesso shear zone_" or there are more than two. P2L27 "D2 transtensional deformation stage" This D2 phase has not been presented yet, this is not suitable for an introduction. P2L28 "Ten 3D thermo-mechanical models have been constructed" Why ten ? varying what ? To test the "influence" of what parameter ? The introduction should clearly state what parameters were kept constant (based on some input natural data), what parameters were varied, and how medels were validated against some other and independant natural data. This is key for the presentation of a modeling study. P2L29 "metamorphic rock records" What is meant here ? Pressure temperature peak conditions ? PT paths ? Ages ? The lack of thoroughness in the presentation of geological context and data is another major concern in this introduction. P2L30 and further. Joining P2L28 comment, or the evolution of the KJD is known and you can use it to validate your models and elaborate on the influence of faults ... , or it is not and then the aim of your study is to further constrain it with the modelling of well-parameterized and known processes, you cannot do both. P3L2 and the whole geological setting section. What is the purpose of citing these age windows ? Please explain at the end of the paragraph what figure you deduce from these data : duration of experiments, strain episode durations ? In this paragraph one would expect to read what the extent of shear zones is, their thickness, their finite horizontal offset ... any geological data that will support the model designs. Especially the evidence that D2 is transtensional is not given, although it is of primary importance on the boundary conditions used further. Exhumation of higher grade rocks between the shear zones is not sufficient to support their transtensional nature. Please show for instance the lineation pattern inbetween shear zones to evidence this complex activation. P3L5 What is a "major" rock ? P3L5 "2850_Ma" here and throughout the whole paragraph. P3L6 Rephrase "intruded by two episodic magmatic pules" as "affected by two .. pulses" P3L9 "Several generations of main magmatic" P3L11 "The Sefwi-Sunyani-Comoéregion (Figure 1b, outlined by red box) is in South-Western Ghana and the Ivory Coast, and consists of ..." P3L11 "The Sunyani and Kumasi basins (Figure 2) are mainly" P3L25 "buried now exposed crustal rocks" P3L26 Please use GPa or any other SI unit. P3L26 "corresponding to a buried depth of about 30-40km " to be rephrased as "corresponding to a maximum burial depth about 30 to 40 km if lithostatic pressure is assumed " P3L27 " D2 deformation phase was a coeval with a decompressional stage" Decompression is not deformation. P4L8 "volcano-sedimentary upper crust" rather than " upper volcano-sediment" P4L9 Why only 18 km of mantle, which is "lithospheric" rather than "upper" ? A simple rheological envelope calculated for your boundary condition strain rate would show how the mantle strength decreases at depth, and hopefully why a 18 km layer is enough to model lithospheric mantle. P4L22 What does "YY" stand for ? P5L16 "We assume that the ðİŚĞsol and ðİŚĞliq temperatures of the lower crust do not change with pressure/depth (Gerya et al., 2008; Ganne, et al., 2014). Gerya et al, 2008 actually have Tsol varying with P, and one would expect that within the pressure range the presented models are spanning over, the melting reaction, (water present melting ?), has a strong dependance on P. Furthermore, the partial melting rate should be monitored throughout experiments to check that melting rates do not exceed the acknowledged value for melt percolation threshold, above which partially molten rocks cannot be considered as a single phase anymore (i. e. Labrousse et al., EPSL, 2015). P5L17 3 references for a linear melt calculation is perhaps excessive. P5L19 Liquidus is neither dry nor wet, it is liquidus. Why is M at liquidus taken as 0.8 ? A good definition of liquidus would be M=1. P5L21 Citing Ganne et al., 2014 for the same linear calculation is not relevant either. Gerya & Yuen, 2003 is enough here. P5L23 " In natural cases, the effective viscosity of partially molten rocks is usually 103 $\sim$ 1010 times lower than their surrounding solid rocks (Vanderhaeghe, 2001). " I don't think Vanderhaeghe (2001) shows any evidence for the "usual" viscosity contrast between solid and

partially molten rocks. Please refer to a paper with actual data, such as Rosenberg and Handy, JMG, 2005, which actually gives limited strength contrasts for the metatexites at least. P6L5 "alpha is the coefficient of thermal expansion" not beta ... P6L6 What is the actual beta value taken in the present study ? In Rey et al., 2009, beta is taken positive (0.13) because water-absent melting, the only reaction considered in their study, is acknowledged to have a positive volume change (Clemens and Droop, Lithos, 1998). It seems that the reaction considered here is water-present melting, which has a dramatic change of slope in the considered pressure range, and hence imply beta values varying from negative to positive. I cannot tell whether models with a constant and positive beta factor for water-present melting have the least geological relevance. With the figures given in the tables, I can estimate M(Moho), melt fraction at the Moho = 0.28 (this would be called a diatexite) and rho(Moho), density at Moho temperature, =2530 kg/m3 for the lower crust, so lighter than the "fault material" in the same conditions ... I guess what is shown in the models is actually driven at first order by these exagerated density contrasts. P6L9 "to explore the influence of pre-existing faults on re-distribution of partially molten lower crust during extension and transtension" Actually, the models presented here evaluate the influence of horizontal velocity field on gravitational instabilities development through a layered heterogeneous media ... models with and without faults would better answer the phrased statement. P6L12 and further and table 1 : Why are series 1, 2 and 3 designed with more orthogonal stretching than shear while field evidence (Figure 2) points at strike-slip shear zones ? Actually series 1 is developing two symetrical core-complexes that would elongate in the N-S direction if further run. It cannot be compared to anything real here. Actually series 4 and 5 are the most interesting, and could possibly be relevant with more realistic figures used. P7L21 to 25 These statements seem somehow obvious, or I don't really understand. Is "occurred" to be replaced by "occurring" ? P7L19. Indeed, as stated in Block and Royden, Tectonics, 1990 for instance, Moho remains flat under core complexes ... P7L26 "Surface relied" Although I agree that relief is a first order signal of processes occurring at depth, I wonder whether this is one to be specifically discussed in this paper, since the field example has no memory of the topography at the time the shear zones were active ! PT paths and strain patterns would be much more appropriate synthetics to look at. I am not sure I should further comment on the results and their relevance to the Ghanaian case study, since the relevance of the numbers used in the models cannot be assessed. Series 4 and 5 could actually be interestingly commented, but I am not sure the series A vs B can be used unless completed with C, D ... with overlap/spacing ratios decreased until the two shear zones do not see one another anymore. No data on a possible lateral propagation of the shear zones is actually available, so this is perhaps not a lead to follow. Comments on the figures and tables Table 1 : Columns 1 and 2 don not give "extension rates" or "shear rates" which would express in .s-1, but boundary normal and tangential velocities. Table 2 : The lower crust is 300 kg.m-3 less dense than the middle crust ... it is even lighter than the volcano-sedimentary upper crust. On what evidence is this very unstable initial setting designed ? The "faults" are lighter than their surrounding rocks ... what does it mean ? This initial set-up is unlikely to be realistic or even mechanically stable. Has a run been perfomed without strain, to check that Rayleigh-taylor instabilities actually do not initiate ? 636°C is a very low melting temperature for the lower crust. On what evidence is such an extreme choice made ? What does the second value for Drucker-Prager criterion stand for. P5L5 only mentions one value. Units are unconsistent use / or -1, but do not mix, same for Table 3 kJ/mol instead of KJ/mol same for latent heat, and Table 3 Table 3 : "Quartzdiorite" instead of "Quartzodiorite", Pa.s for "faults" Figure 2 : there are migmatites out of the shear zones corridor ? How did they exhume ? Do they have the same age as the ones from the KJD ? Figure 3 : " Two interacting vertical faults with a thickness of 2 km (Le Pourhiet et al., 2004)" A 2 km thick structure is not a fault, it might be a fault zone, or a preexisting shear zone, but not a fault. The 1.5 km thick structure in Le Pourhiet et al., 2004 is actually a nappe ... certainly not a fault. For what reasons were the "faults" not continued to the (x,0,z) and (x,300,z) edges of the models ? How were their length scaled regard to the overall model sizes ? Figure 4 : Black and red arrows do not change size from a model to another, aren't they supposed to do so according to

Table 1 ? All snapshots are not extracted at the same time and hence finite strain, this limits their comparison. Besides, the color scales stipulates >= 10 km of "relief" .. What is the actual maximal "relief" produced ?

As a conclusion, the idea of testing the impact of a relay zone in a shear system on exhumation pattern is one to be addressed with 3D numerical modelling as attempted here. The tool used here and the case study proposed definitely can bring some insight on the question, but the used constants and the varied parameters have to be much more thoroughly chosen and presented. How are they chosen based on natural data ? What other natural data are kept for validation against synthetics post-processed from the models ? Focusing on models 4 and 5 and adding preliminary tests (with null velocities, with no density changes for instance) and more post-processing of outputs (finite strain patterns, strain rates invariant maps and profiles, PTt paths ...) would probably make a very convincing contribution.

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