While the focus of the introduction is clearly the role of topography on the pre-stress conditions, it seems to me that it is not the case for the whole paper. The first part indeed focuses on topography, from a simple analytical perspective which I like very much, but the modeling part introduces additional complexities (rheological, geometry) which are not necessarily clearly introduced in the first place. Furthermore, the limits on the sources of pre-stress are not necessarily discussed which makes the whole description of the objectives not that clear to me. For instance, the remanence of stresses from previous earthquakes or previous cycles is not discussed while it could be of similar orders of magnitude of the other sources mentioned currently.

We have revised different parts of the manuscript to better explain the relevance and meaning of topography and pre-stress. Section 2 now addresses implications of the dynamic Coulomb wedge theory (DCWT) and we better explain that topographic relief is a prerequisite to cause stress changes by a megathrust stress drop that support failure in the upper plate (new Fig. 2). Regarding the numerical models, we have revised the discussion of the modelling results and included two paragraphs in section 5.3 that explicitly address the importance and effects of topography (lines 591-610). We have further conducted additional models to address the impact of model parameters, including the different material properties of crust and mantle and the presence/absence of topography. The detailed modelling results are included in the supplement (Figs. S5-S14). The effect of the parameters on the percentage of positively stressed aftershocks is summarized in the new Table 1 included in the main text.

We agree with the reviewer that the stress state before the earthquake will be a consequence of previous earthquakes and earthquake cycles. The calculation of the coseismic Coulomb failure stress change (DCFS) does not depend on how the pre-earthquake stress state was achieved and our models (as well as the DCWT) do not allow to assess processes in the previous interseismic period or during previous earthquake cycles. We now explain this aspect in the method section 3.2 (lines 329-333): “We thereby obtain an estimation of the total stresses in the forearc and corresponding megathrust shear stresses shortly before and after the megathrust earthquakes that is consistent with the stress-drop models and the post-mainshock fault kinematics in the forearc. The forearc stress states in the interseismic periods before and after the mainshock are not determined and do not influence the calculation of the coseismic Coulomb failure stress change.”

The revised description of the DCWT further includes a simplified statement (adopted from Wang & Hu, 2006) that briefly explains how the interseismic period conditions the immediate pre-earthquake stress state (lines 125-128): “Over the interseismic period, the shear stress on the seismogenic megathrust increases progressively such that the maximum compression of the wedge occurs toward the end of the earthquake cycle. During megathrust earthquakes, the shear stress on the plate interface decreases abruptly due to coseismic weakening processes (e.g., Kanamori and Brodsky, 2004; Scholz 1998; Wang and Hu, 2006).”

Finally, we explain in section 3.1 that the pre-stress applied in the numerical models in step 1 is the lithostatic stress and is only applied to ease the computation of total stresses.

There is no comparison between the potential to explain the location of aftershocks using the classic coulomb approach (semi-elastic half space following King et al 1994) with the various refinements proposed here. While the authors state that

topography is a “first order” contribution, there no elements in the paper supporting that assertion. The first order contribution to aftershocks is not the pre-stress but the earthquake itself and it would be great to be able to quantify the effect of each of the improvements brought to the calculations. One way would be to simply turn off gravity in the models, but also see what happens without the rheological heterogeneities and quantify the performance of the different models. If the parameterization proposed by the authors is indeed of importance, it should definitely greatly improve the overlap between the positive CFS regions and the aftershock distributions. I am asking this because simple CFS seems to work to some extent for strike slip earthquakes where topography does not play a major role, compared to subduction zones.
We now better explain the importance of topography in sections 2 and 5 as described in our previous response. We have also carried out models without topographic stresses, i.e. models with a flat surface and without water loads (turning gravity off is not an option in the force-balance models because without gravity there is no stress, as all the stresses result from gravity). Without topography, the Coulomb failure stress change is negative almost everywhere in the forearc (Figs. S12-S14 in the Supplement). The reason for this and the difference of our DCFS calculation for optimal failure planes to the common approach of calculating DCFS for faults of specified orientation is explained in section 2.
We have further added to the discussion a comparison of our modelling results with the outcome of previous models (section 5.4, lines 642-673). We address that the percentages of positively stressed aftershocks are higher than in previous models (≥97% vs. 60-70% for Japan, and 64-87% vs. <50% for Chile). We also address similarities and differences between the modelling approaches.

I havent found in the paper the notion of attribution of the earthquakes to a sequence of aftershocks. I guess the authors have carefully taken care of this aspect but are all the events used here really aftershocks and how is the selection performed? Is it simply over a given time period after the mainshocks and if so, is the duration of that period of importance? I know that there is no time dependent processes in the CFS calculation but there is a time dependency in the aftershock distribution and over a long time, some “interseismic seismicity” should show up in the dataset, polluting the interpretation in this case.
We now provide arguments for considering the seismicity as aftershocks (lines 583-593, Figs. S3-S4 in Supplement). The arguments include: 1) increased seismicity rates following the mainshock, 2) the investigated seismicity appears in areas that were inactive before or shows little seismicity, 2) the seismicity rates are highest immediately after the mainshock and decay at a power-law like rate afterwards, 3) the location of seismicity remains similar over time.

Minor points:
There is two sections 2.2
We have corrected the section numbering.

Point P1 (figure 2) is not indicated on figure 1
The Coulomb wedge model and the sketch is scale independent; we therefore prefer to do not indicate the point. More importantly, we realized that it is more accurate to state that these are “stresses at 10 km depth” rather than at a coordinate X,Y

because the stress sigma_x does not depend on horizontal distance (equation A1a). We have adjusted the text and figures accordingly.

I am not convinced by the need for figure 4 since it simply explains what CFS is. It certainly is a nice textbook figure, but I don't see how it brings new elements to the discussion. In general, removing elements that are known or separating them clearly from what is new would certainly help clarifying the objectives of the paper.
We have removed the figure.

Line 515: Some words are missing in this sentence
We have simplified the entire section and the sentence has been removed.

Line 525: Some words are also missing here.
We have simplified the entire section and the sentence has been removed.

The first sentence of the conclusion is quite awkward. The stress change does not depend on the initial stress since it only depends on the stress drop. If you mean the CFS, then yes, but please clarify.
We have removed the sentence and other misleading formulations in the manuscript.