First reviewer (Thomas Voigt)

Question 1 and 2 are well explained by the modified version of the forward modelled cross section (Figure 5) and the new explanation in the text (page 4). We show an evolving fault-related folding for the forelimb of the Harz, which undergoes breakthrough only after several kms of shortening. This is a plausible (if not unique) structural solution to the Harz shortening geometry and its evolution over time. It suggests that, indeed, the initial Harz uplift, when some basin subsidence would already be taking place according to our model, would be associated with blind thrusting.

Question 3. As we have modified the paper to include the idea of "tilted rigid block" basins, although we hope we have made it clear that there are still conceptual differences between our formulation of the problem and the one originally used by McQueen and Beaumont, it should be clear that "forebulges" in the strict sense cannot develop, because, as we point out, the length of individual segements of lithosphere is too short for flexural folding, and hence forebulge developement. Of course, the uplifted corner of a "tilted rigid block" may be interpreted as a forebulge, but strictly speaking, it isn't one, since a forebulge is strictly defined as being due to the flexural bending of the plate under loading. We have given a reason for sticking to the "foreland basin" name at the end of the paper (page 9, line 21-23).

Question 5. As we state, because of how the model is derived, a "load" can be due a multitude of different things, including thickened crust. The load in these flexural models is simply an applied, vertical force, which may even be the resultant of a horizontal force across a fault plane for example. Any thickening of similar magnitude to the actual load thicknesses applied in the model would produce a similar amount of subsidence. In general, Nielsen and Hansen's approach is much more complicated, time dependent and hence not as focused on the specific, elastic aspect of the problem of subsidence in these basins. It is also at a much larger scale, and ultimately was used to explain later, Paleogene subsidence as a consequence of viscous relaxation. We do not consider it particularly relevant to what we are doing in this paper, and hence have chosen not to discuss it any further. Regarding our numerical model, we have also given a reference to our submitted paper in this journal (se2021-36). Viscosity of the mantle has no effect on our model as it is a simple, elastic flexure model where the mantle is considered an inviscid fluid substrate. This is a widely accepted practice in flexural modelling of foreland basins. The assumed values have small uncertainties, and generally aren't discussed beyond highly specialist literature. We will refer to some of this in our "theory paper" (se2021-36) when we revise it. We feel that is the better venue for technical discussions.

Regarding supplementary comments, please note we have incorporated fully Guido Meinhold's section across the SCB into our revised forward model (Fig. 5 - it is the third component of this composite section, and has been directly retrodeformed in order to model the forward evolution of the basin structures).

Second reviewer (Pawel Poprawa)

We have modified the reference to the thermochronological data of Von Eynatten et al. and reference the broader and more compatible, age spectrum, as suggested. page 1, line 21

Regarding onlap and geometric evolution of foreland basins, for the SCB-Harz, we have mentioned that the load is effectively static (with some uncetainty surrounding its ultimate magnitude and whether or not it may have achieved some sort of steady state with erosion). Some of this is also explained in the new figure 6.

We have replaced the old figures 5 and 6 and 7 (three numerical experiments) with 2 new experiments. All are carried out with higher resolution grids (100m spacing) using the new code described in se2021-36 (modified to allow quadruple precision variables).
The new figure 7 is effectively the same as the experiment in the old figure 5. The new figure 8 is an experiment corresponding to the suggestion made by Brian Horton concerning the flexure of the SCB beyond the lateral termination of the basement structure of the Huy-Fallstein-Hakel anticline. Hence, the old figure 6 which was somewhat unclear in its purpose, has been replaced by a different mechanical model. The old figure 7 has been removed entirely, as we agree with reviewer 2 that it was a bit confusing. We have to some degree covered some aspects of it in the new cartoons in the new figure 6. Please also note we have used the base of Zechstein unconformity surface as the marker flexural subsidence in the basins.

Regarding editorial remarks. We have corrected the caption for figure 4, reversed the order of figures 1 and 2, added an inset map of Europe to show the location of the new figure 1 (formerly figure 2) and added the abbreviation explanations to the figure captions.

Third reviewer (Brian Horton)

We have modelled the case of an SCB without a basement fault associated with the H-F-H anticline. This does indeed, give a wider SCB, and seems to fit well with the change in width of the basin at this position. This new model is included as figure 8. We note here that a very interesting future project would be a 2 dimensional, "thin sheet" elastic model of the Harz and SCB incoporating the weakness zones associated with laterally terminating faults. However, this is a separate project and paper.

Regarding timing of thrusting and fault geometries and in particular the H-F-H anticline. Our modified, composite, modelled cross section (Figure 5) shows what is possible or likely in this regard. It suggests the most important components of basin deformation are shear and fault induced rotation of the fault-adjacent footwall cover, and that this requires a fairly early breakthrough of the main HNBF to fit both the footwall geometry and the thermochronological estimates of total uplift of the hanging wall. The flexural models in turn, suggest that the HFH associated basement thrust is necessary in the narrow portion of the basin, but only as an elastic weakness. We cannot model in this respect, dipping elastic weaknesses in a 1d model. However, we suggest the empirical fit to the data suggests that the most important effect on subsidence is the effect of elastic weaknesses per se and these can be well-represented in 1d models. The flexure models in figures 7 and 8 in particular, illustrate clearly how important an elastic weakness is for determining subsidence geometry. We also note here, the reference to our new submission (se2021-36) where some of these topics are discussed in more detail.

Finally, we would like to thank the reviewers for their careful considerations of our paper which we feel have proven invaluable in improving it.

on behalf of the authors.

David Hindle