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Interactive comment on "The Subhercynian Basin: An example of an intraplate foreland basin due to a broken plate" *by* David Hindle and Jonas Kley

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se2020-185, reponse to reviewer comments 1 and 2 (Thomas Voigt and Pawel Poprawa).

We first of all thank the reviewers, Thomas Voigt and Pawel Poprawa for extremely helpful and constructive comments on the paper. Comments by these reviewers (1 and 2) overlap a little and as a result, we have dealt with them in a single response document.

We apologise in advance that this is a duplicate of the response to RC1, however, we felt with the overlap of the comments of reviewer 1 and 2, it made sense to deal

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Discussion paper

with them jointly. For consistency of reading, we therefore post the same document in response to reviewer 2.

One of the key issues identified by reviewers 1 and 2 is the exact timing and also by implication, nature and evolution of the uplift and overthrusting associated with the Harz. In general, there is a problem reconciling the different ages that are assigned to thrusting based on stratigraphic and sedimentary interpretations from basin fill and erosion-related measurements from thermochronology. Reviewer 2's suggestion that the broader spectrum of ages given by ZHe, AFT and AHe (90-60Ma) seems sensible given the inherent difficulties and uncertainties associated with thermochronology. This is indeed, the general conclusion of Von Eynatten (2019) - that the very earliest compression and uplift of the Harz began ∼90Ma. This also pertains a little to reviewer 1's question concerning the nature of the thrust or break in the lithosphere. Our model is agnostic regarding whether the plate has a "pre-existing" break or not. All our model requires is what we can observe today about the likely past behaviour of the system: that a basement thrust either developed from reactivating an earlier structure or was freshly initiated by regional compression. The basement thrust was clearly active for most of the time the Harz were growing. It may well be that it was initially a blind thrust, with what could be termed a "fault propogation fold" developed above it (which is more or less how we have modelled the evolving geometry of the HNBF in figure 8). The exact nature of the fault would make little difference for the flexure model however, so long as the fault was functionally equivalent to a large, local reduction in elastic thickness of the plate, when looked at in terms of elastic bending of the lithosphere. We think this question is well-answered by the empirical side of things, since we document in the paper a geometry of plate bending that seems to correspond very closely to one that is produced by loading an elastic plate broken into multiple segments (figure 5).

Reviewer 1 and 2 both comment on the nature of the marine incursions in the region. The earliest marine sedimentation indeed begins in the Cenomanian, and seems to be due to regional transgression, although subsequent uplift (to some degree directly related to the emergence of the Harz) has removed any direct evidence for it from regions to the south. Most importantly, it occurs independently of any local subsidence. When exactly localised basin formation begins is therefore a slightly more nuanced question. Further complicating things is the thin-skinned, syn-sedimentary deformation of the basin fill of the SCB, which is directly related to the Harz Northern Boundary Fault (HNBF), but not so much the actual mechanism of subsidence that we discuss in this paper.

I think we were a little confused ourselves in this respect. We originally were very focused on the existence of classical, "feather edges" due to progressive tilting of a developing "foreland" basin and the progradation of the infill along with possible migration of the forebulge. If we detach ourselves a little from the idea of a classical "foreland basin" (as also suggested to some degree by reviewer 2) and instead focus on the creation of a marginal basin or trough of any kind, then perhaps the broader evidence of the survival of these packets of Late Cretaceous sediment in these "marginal troughs" is enough on its own to show a localised subsidence that was more or less synchronous. Furthermore, the syn-sedimentary, thin-skinned deformation of the basin fill evidenced by the various unconformities, beginning in the Coniacian, demonstrates compression, and more importantly, shortening, which probably is associated with Harz thrusting in general. There are other pointers towards localisation of basin subsidence (Harz trough) some time from the Coniacian to the Middle Santonian. It is difficult to be absolutely certain on the basis of stratigraphic evidence exactly when this really took place though. The balance of stratigraphy and thermochronology (Voigt 2004; Eynatten, 2019) seems to suggest $\sim$90 - 87 Ma.

The complicated basin fill, with its associated, syn-sedimentary deformation, is our best (only) hope for a detailed chronology of basin development. However, in parallel, the total subsidence associated with probably the entirety of the Harz (and Haldensleben and Gardelegen) overthrusting is documented by the "passive markers" of Late Cretaceous subsidence, which we probably have not explained very well in the current draft of our paper. This responds directly to the question of reviewer 2 concerning the geometry of the Rotliegend and other Mesozoic strata. The top surface of the Rotliegend actually corresponds to part of a wider erosional unconformity (it extends hundreds of kms north and south of the SCB) sealed by the base of the Zechstein formation. This unconformity was itself regional, and originally relatively horizontal, given its cover by shallow water, Zechstein deposits. Due to its nature (an erosional surface across a variety relatively rigid basement, and older sedimentary units) it is also untouched by the thin-skinned tectonics affecting the SCB and underlying Mesozoic units, above the Zechstein evaporite detachment. The only tectonic event that has affected the unconformity in the modelled region is the Late Cretaceous "flexural" subsidence/bending asscociated with the loading of the crust due to the Harz/Flechtingen/Gardelegen faulting and any additional Late Cretaceous basin infill, of which it records (probably) everything. There is no other significant tectonic event that disturbed this unconformity. This is what we try to show in the combination of figure 3 and figures 5 and 6.

However, we have emphasised only the Rotliegend basins (in their entirety) which are laterally discontinuous, instead of the regionally extensive unconformity marked by base Zechstein. We now recognise a better and clearer approach is both to highlight the unconformity across the whole of figure 3 and also replace the Rotliegend basins with the unconformity itself in figures 5 and 6. We would also suggest changing the text in section 2 to reflect new emphasis on the unconformity itself as a marker. But it is most important to emphasize that the geometry of the base Zechstein unconformity today, will mostly reflect the flexure and subsidence of lithosphere due to Late Cretaceous loading, and certainly won't be influenced by the folding of the Mesozoic sedimentary cover above the Zechstein detachment. As such, the line traced by the base Zechstein unconformity should correspond quite closely to a "flexure curve" described by a 1d flexure model, a little bit like ocean bathymetry is used today to describe lithospheric bending due to volcanic islands.

More generally, reviewer 1 asks whether "foreland basin" is really the correct name for the phenomenon we describe. Reviewer 1 also asks about a discussion of earlier ideas for basin formation in such settings. We propose to make the "intraplate foreland" distinct from a classical "foreland" by adding a cartoon figure and some additional text, explaining the evolution of subsidence of the SCB and adjoining basins as a system of more or less "rigid" tilted blocks and basement uplifts which form a segmented lithosphere (this also responds to questions raised by reviewer 3).

Turning to more specific questions of reviewers 1 and 2.

Does something like a forebulge develop (reviewer 1)?

We had not really thought about this at the time we wrote the paper. However, it is clear that a "classical" forebulge is increasingly difficult to generate as the fault-bounded "segments" of lithosphere get shorter. The shorter a segment, the less it can bend. In the case of the Harz and SCB, the likely segment length would be constrained by the location of the Huy anticline, Flechtingen High and Haldensleben Fault. This issue has also been raised by reviewer 3 (see later). In any case, segments in this system are so short that they probably preclude development of a "normal" forebulge.

Nielsen and Hansen (reviewer 1)

Nielsen and Hansen (various iterations from 2000-2006) used a visco-elastic model, but critically, with an "elastic break" incorporated into it. This is very similar in some ways to what we have done. However, our model is simpler (it is purely elastic and does not consider time-dependent evolution of any kind). At the same time, our model is entirely focused on the equivalent elastic behaviour (and properties) of the lithosphere, and as a result, we find some different insights into the behaviour of the system than Nielsen and Hansen. Their work mostly focused on time-dependent, stress relaxation processes. These may indeed exist. Our model cannot say anything about them by its very nature. Our model instead emphasises the importance of mutliple lithospheric breaks to account for a quite complicated Late Cretaceous subsidence pattern. Nielsen and Hansen's work is, in general, at a much larger scale and misses this aspect of the system. Nielsen and Hansen's work also has an implicit "thrust" wherever there is an elastic break imposed on the system. However, at the scale of their experiments, it will not be easily comparable to our results. But overall thickening in their model can probably be considered somewhat equivalent to the thrust load we impose in ours.

Pre-existing fault (reviewer 1)

See above

Density of mantle etc. (reviewer 1)

Within the range of possible density variations of mantle, crust, and infill, there will be little significant difference to model results. Mantle density is likely to be within 5% of the true value. Crustal load and infill densities will also probably be similar.

Figures (reviewer 1)

We will revise figures 1 and 2 using the digitised and georeferenced fault networks from the GIS layers available from the BGR from the 1:200000 sheets. These are at least "standardised" and replicable for anyone else (since they are available for public download in georeferenced form).

Figure 6 (reviewer 2)

The modification to the model for figure 6 was rather ad hoc to try to better match the geometry of the flexural marker horizon (base Zechstein unconformity). On reflection, we now recognise that the region we have arbitrarily "pulled" upwards, which is by necessity elastic in our model, probably corresponds to the hanging wall "damage" zone of the Haldensleben Fault (cf figure 4d). The geometry of the unconformity surface in this region is therefore due to displacement above a thrust ramp, and is effectively "geometric", not elastic. It is therefore inappropriate to attempt to model this region in any way with a purely elastic flexure model. It suffices to place the elastic break separating the dipping (elastic) segment below the SCB from the hanging wall of the Haldensleben Fault, correctly. We will thus remove figure 6 and make a more complete explanation of the discrepancy between the unconformity surface and the elastic model in this region in figure 5.

Figure 7 (reviewer 2).

Using the model to illustrate the point with, as the reviewer correctly states, a deliberately non-valid alternative, is, on reflection, probably not a good idea. We would propose instead to make a simple, generic, cartoon to explain the principle. We nevertheless think it is important to highlight the difference in response of a broken versus unbroken plate to loading.

Page 4 Line 11 (reviewer 2)

indeed, 4e is an error. We have corrected this. Fig 1 and 2 (reviewer 2)

We will add an inlay with the location of the area within Europe to figure 2. We will also add the relevant abbreviations to the figure caption. We will swap the order of the figures 1 and 2 in the paper.

Page 7 line 27 (reviewer 2)

The reference to figure 6 is indeed a mistake. We propose to remove figure 6 entirely for the reasons given above.

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