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Interactive comment on "Evolution of rheologically heterogeneous salt structures: a case study from the northeast of the Netherlands" *by* A. F. Raith et al.

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General comments

This paper represents a very welcome addition to the slowly growing number of publications focused on seismic reflection analysis of intrasalt structures. As a result, the paper is timely and, as nicely articulated in the Introduction, potentially very important. As stated above, the Introduction is strong, clearly outlining the rationale and potential impact of the study. The Geological Setting and Dataset sections are generally thorough. It is in the Results and Discussion section, which represent the 'heart' of the

manuscript, where things become more problematic. In particular, a lack of figures and figure citations make some of the main descriptions very difficult if not impossible to follow, thus it is unclear how the authors' favored model is derived. Related to their model, this too is not convincingly illustrated. I also have some issues with the overall salt tectonic model the authors propose. In summary, I definitely think this paper should be published, although in my opinion it would benefit from some revisions before this happens. I provide more detailed comments below.

Specific comments (and technical corrections)

Here I provide specific comments and technical questions. Page numbers are indicated as are line numbers.

P1878

L24 and 27 – Slight mixing of terms (e.g. K-Mg salts vs. carnallite-bischofite). Try to be consistent throughout the manuscript.

P1879

I loathe to recommend my own papers. . .but in this case I think it's justified! You may wish to read Jackson et al. (2014) – GEOLOGY and Jackson et al. (2015) – JSG and, if you find them relevant, cite them in the Introduction. These studies use 3D seismic reflection data from offshore Brazil, following-up on (and challenging) the earlier work of Fiduk and Rowan (2012). You may also want to cite Dooley et al. (2015) when you mention modelling studies of intrasalt structure, not to mention the earlier centrifuge work cited therein.

L21-22 – Sentence is a little unclear. Please rephrase.

L24-25 – Sentence is a little 'wordy'. Please rephrase.

P1880

L5 – Cite Jackson et al. (2014, 2015) here too? As well as Fiduk and Rowan (2012)?

L11 – Cite Dooley et al. (2015) here too?

L13-16 – Reference required for a statement like this.

L20 – What do you mean by 'significant thickness'?

L20-23 – References required for some of these example? Or are all these basins described in the Vysotskiy reference?

P1881

L4-5 – Again, what do you mean by 'unusually thick'? Statement needs qualifying.

L10-25 – Very interesting!

L26 – Remove word 'then'.

P1882

L20-21 – I come back to this later, but I cannot believe that contraction didn't play a role in the formation of the major salt pillows.

P1883

L1-5 – Fig. 2 is way too small and 'busy'; it is almost impossible to make a link between the stratigraphy and the seismic data. Furthermore, as I will come back to later, a proper synthetic seismogram is required to really convince people of the link between rocks and reflections.

L6 – I cannot locate the ZIIIAC stringer easily on Fig. 2. Also, you need to cite a figure to support the statements made in the paragraph L6-10.

L11-30 – It is very hard to follow this text using the somewhat cramped Fig. 2. This is not helped by the fact that the intra-Zechstein stratigraphic nomenclature is a total nightmare in terms of letters, numbers, roman numerals. . ..!

L11-30 – I am a little concerned that the intra-Zechstein proportions encountered in

wells are used to derive an idea of how depositional conditions varied in the Late Permian. I guess my main concern is that post-depositional flow, as you argue for later in the manuscript, mixes up the original depositional distribution. I guess this isn't you work or, indeed, the specific aims of the present paper, but I thought I'd better mention it.

P1884

L4-6 – Comment here that the seismic-stratigraphic architecture of these units is used to constrain the evolution of the salt structures?

L13 – Use the term 'reflections' instead of 'reflectors'? Not sure it is formally defined, but a wise person once told me that a 'reflector' is a rock interface that generates the observed (and mapped) 'reflection'. . .

L20 – What do you mean by "high resolution data"?

L23 – Remove reference to specific software packages in the main body of text. You mention them in the Acknowledgements.

L15-30 – I think a synthetic seismogram is crucial in this study if you really want to link rocks to reflections and rheology to structural style. As it stands, I am very confused as to how each of the specific units you are describing (and mapping) are expressed in wells and in the seismic data. A well log, showing some wireline log data, would be very useful (and convincing). Furthermore, it would be nice to see the location of all these 136 wells on a map; this would really hammer home how robust your database is in terms of direct sampling of salt composition and thickness.

P1885

L1-2 – Rephrase. You are mapping stratigraphic tops or horizons and not stratigraphic units. I'm being picky, but it makes no sense to say 'stratigraphic units' on L1 and then to list a series of 'tops' in the sentences that follow. Furthermore, some of the tops have age information (e.g. Top Upper Rot (Triassic)) whereas others do not (e.g. top

Rupel Formation (North Sea Group)); i.e. what age is the top Rupel Formation?

L7 – Not sure this sentence is needed.

L7-9 – I would rephrase this sentence. Although the isopachs tell you about thickness and depocentre location, you don't know if all depocentres are really like Trusheim's primary peripheral sinks, which have a very specific temporal connotation in terms of timing of formation relative to salt structure development. For example, some depocentres might be due to cover stretching-related graben formation above reactive diapirs; these depocentres are NOT primary peripheral sinks.

L9-11 – You don't know this for sure; some relief may have developed during salt movement. Not all relief needs to have been .levelled off'.

L20 – Label salt structures on Fig. 1.

L24 – Maps too small and the layout is a little old (i.e. put the maps more clearly in age order?). In fact, the seismic profile is so small, I cannot make out the phases described in the text. This is a big problem, as the cover seismic-stratigraphy is critical to the model you later describe. This can be fixed though by making the figure larger and clearer.

L25-26 – Sentence needs rephrasing; e.g. The Veendam Pillow strikes SW-NE and is 10 km long in the NE-SW direction and. . .

P1886

L1-3 – Sentence a little unclear. Please rephrase.

L3-5 – It would be useful to see a depth map of a key overburden horizon at some point in the manuscript. Some of the fault geometries at that level are important to your story, but you do not convincingly provide primary data to illustrate their map-view geometry.

L8 – Cite lower profile in Fig. 3? L9 – Cite upper profile in Fig. 3? All in all, please be more specific in your figure call-outs.

L11-12 – Do you mean salt rollers rather than salt anticlines (sensu Hudec and Jackson, 2011).

L14 – Cite figure to illustrate the "bigger faults".

L20 – Structural trend (i.e. EEN-WWS) needs changing.

L19-23 – Are the fold upright, recumbent, etc? How are they distributed across the study area, especially in relation to the main salt structures? I get very little sense of the true geometry of the folds, especially because you provide no zoomed-in seismic images or maps showing the range of fold styles. I feel that this is the section that really needs more work to convince the reader of the styles of deformation and their distribution. Without this, I find it hard to be fully convinced of the structural model you later propose. In Jackson et al. (2015) we provide detailed maps of the intrasalt structural styles and their distribution across the study area; you have excellent data that, I think, lends itself to similarly detailed structural mapping.

L26 – Lower bit of Fig. 7 doesn't really add anything. As stated above, some basic maps and sections would be far more valuable.

P1887

P1-18 – This is a super-critical section. . .but I find it very hard to visualize what you are describing, largely due to a lack of data (e.g. seismic profiles, maps, etc) that clearly illustrate the main structures and their spatial relationships. To keep on top of the slightly unwieldy intra-Zechstein nomenclature, I kept going back to Fig. 2, but this didn't really help much because of the aforementioned figure size issues.

L23 – Which bit of Fig. 5 are you referring to?

L24 - Rephrase; e.g. ". . .can be as little as 200 m thick. . .".

L23-26 – Again, I have to work extremely hard to see these thickness relationships/changes in the regional map(s) shown in Fig. 5. It would be especially powerful

to some zoomed-in bits of the maps, with complimentary seismic profiles, clearly showing some of the key relationships that underpin your main arguments. At the moment, I don't think this is done as well as it could be.

P1888

L2 – What is the orientation of the seismic profile in Fig. 5?

L1-2 – Would be nice to cite a figure to illustrate this relationship in cross-section.

L3-22 – This section is a little 'list-like' and very descriptive. I wonder if some of the material in this section could come later, in Section 5.2, when you describe the overall salt tectonic model. In this way, the description and interpretation (still in separate paragraphs) would be more closely coupled. Furthermore, the Bundsandstein looks rather tabular to me in Fig. 2, at least about the Veendam Pillow, but I don't get a sense of this at all from the description you provide here and on the preceding page.

L25 – I can't tell green from grey in Fig. 4. Also, the green merges into the green areas on the underlying depth map.

L24 – Is the bischofite really abundant only in the southern area? It looks more widespread than that to me, only really being absent on the crest of the Slochteren Pillow. Maybe it's my mistake, seeing as I can't make out the well colours on Fig. 4 very well. . .

P1889

L7-10 – What evidence do you have for this statement? Can you cite a figure?

L11-17 – I'm a little unsure whether cumulative thicknesses really are that useful. Something could be relatively thick but, at that specific location, still represent only a small % of the total evaporite thickness. In this way, the unit may then not be that rheologically significant, even if it is locally thick, with the bulk rheology being controlled by the volumetrically significant other units.

L21-24 – Could the same general statement not also hold true for halite and, in fact, pretty much any other layer contained in the salt structure; i.e. thickness variations may be depositional of tectonics?

P1890

L1-5 – I like this idea, but why wouldn't the top salt become flat after subsalt faulting? Is the salt bulk viscosity that high that it can 'sustain' relief generated by subsalt faulting? Is this realistic? You may also want to cite the original pod-intepod paper by Hodgson et al. (1992), which advocates a similar thing (without providing any physical basis for it).

L12 – See earlier comment; put seismic-stratigraphic description of overburden units at the relevant points in section 5.2 to more closely link descriptions and interpretations? Just a suggestion to tighten things up. . .

L22 – Label salt structures on cross-sections and maps in Fig. 8, otherwise it is difficult to link the text with the figure. N.B. See also Dooley et al. (2015), who show thickening of low-viscosity layers into the crest of inflating salt diapirs during initial rise.

L22-24 – I am confused here. Above the Schloreten Pillow the Bundsandstein layer seems to thin by truncation at its top surface (in Fig. 2) rather than by onlap onto the salt. In fact, it seems to have a concordant lower contact with the salt. Furthermore, lower down the flanks of the Schloreten Pillow the unit looks tabular, like it does across all of the Veendam Pillow (Fig. 3). Thus, I see no reason why salt movement occurred at this time. In fact, as I argue below, the Bundsandstein layer pre- rather than syn-kinematic to me. . .

P1891

L5-6 – Rephrase sentence; e.g. ". . .the Lower Cretaceous interval is 50-140 m thick and does not thicken. . .".

L9 – Based on: (i) the overburden geometries; (ii) the shape of the salt pillows; and (iii)

the nature of the contact between the salt and overburden, isn't it more likely that the salt structures grew in response to contraction rather than, as I think you are advocating, passive diapirism? I see no inward-dipping 'fan' of normal faults detaching into the crest of the pillows and structurally thinning the overburden, hence reactive diapirism also seem an unlikely trigger. Instead, the faults higher in the overburden could be due to outer-arc being during contraction. In fact, wasn't the Late Cretaceous a time of Alpine-related shortening, hence you have a regional trigger to play with. Whatever you think the trigger is, I think you need to argue more strongly for your preferred model.

L17-24 – I find this model a little contrived. Couldn't the overburden faults simply be due to outer-arc bending during contraction and salt pillow growth? Also, this model implies that the sub-salt faults were active in the tertiary; do you have any independent evidence for this? You cite Lewis et al. (2013), but the fault geometries, relationship to the salt, salt thickness, etc, are very, very different in that example, thus I am not entirely sure how relevant the analogue is. In that paper we could independently constrain movement on the sub-salt faults, and thus demonstrate their age-equivalence to the overburden faults. I don't think you can do this here. You've got way more salt between the fault populations.

P1892

Section 5.3 – I found this all a little hard to follow, mainly because, as stated earlier, I don't think some of the key thickness relationships, structural styles, etc, are adequately illustrated or described. Furthermore, I think this model needs to be illustrated in another summary cartoon, focused on the details of what is going on inside the salt; I don't think Fig. 8 really is detailed enough in this respect.

L14 – Not sure what you mean by "passively or actively deform". Please clarify.

L23-25 – Cite a supporting figure.

I hope the authors (and other reviewers) find these comments useful. I hope to be able

to discuss them further via the online forum!

Speak soon!

Chris

Interactive comment on Solid Earth Discuss., 7, 1877, 2015.