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**Analogue experiments of salt flow and pillow growth due to basement faulting and differential loading**

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## General comments

I congratulate the authors on the significant advance in salt tectonics provided by the sophisticated and beautifully illustrated dynamically scaled generic experiments documented in their paper. However, I consider this presentation does not do justice to what I assume was the brilliantly conceived and consciously perverse timing imposed on these models.

In the world before the commendable transparency of Solid Earth or Solid Earth Discussion, my confidential review to the authors and editor(s) would have rejected this script with the aim of giving the authors the chance to recast their presentation to do justice to their excellent work by adding a valuable point not made in this version.

Authors and topic editors beware! The opportunities for authors to correct simple mistakes in the privacy that was normal in the past has been lost in the harsh new world of transparent publications like Solid Earth. Authors and editors must take more care than was necessary in the traditional journals of the past to ensure that potentially embarrassing mistakes are not cruelly exposed.

This work (doi:10.5194/sed-6-1625-2014) is currently presented as a study of the effects of displacements of normal faults in the basement and sedimentary downbuilding on the growth of salt pillows in a buried salt layer. The current emphasis on faulting raises several questions not addressed in this version. There is no explanation for introducing to the experiments a pre-kinematic layer at the base of the cover sequence. There is no explanation for artificially separating realistic natural histories of overlapping or contemporaneous faulting and sedimentation into sudden pulses of rapid faulting followed by long phases of cover sedimentation. There is also an unexpected insistence that a hiatus is required between faulting and sedimentation to produce salt pillows.

Whether the experimental strategy was consciously designed to distinguish the effects of elevation and pressure heads or not, it could be recast as such. Recasting the introduction to something like that below would immediately explain all the apparent perversities in the timing of the experiments mentioned above:

Salt behaves like a linear viscous fluid and flows “down” hydraulic pressure gradients that are the sum of two components of pressure: elevation head and pressure head (Keble, 1988; Hudec and Jackson, 2007). A hydraulic head can be imposed on the salt layer by (amongst many other scenarios) differential vertical displacement of the salt layer due to displacements along old or new normal faults in a laterally extending basement. An additional pressure head is induced on the salt layer by any differential loading due to lateral changes in thickness of the sediments accumulating on the irregular topography of the faulted surface. Natural processes of sedimentation and thick-skinned extension along basement faults usually overlap in time relationships that are so intimate that ii is seldom practical to distinguish the theoretically different effects of elevation and pressure heads.

As a result, the experiments documented here were carefully designed to separate the development of the elevation and pressure heads in time so as to distinguish their different kinematics. To this end, an elevation head was induced in a layer of model “salt” by a rapid pulse of lateral extension that activated normal faults built into the model basement. Deformation driven by the resulting elevation head was given time to develop by artificially delaying downbuilding of a “sedimentary overburden” until after a hiatus of 15 minutes. A thin layer of pre-kinematic “cover” was added to hinder any potential flow of surficial “salt” downslope into the graben. In fact syn-sedimentary recumbent lobes of saturated salt dribbled off the crests of at least one salt diapir (e.g.

Talbot, 2008). After the pattern of deformation induced by the elevation head was clearly established, a pressure head was imposed by a long phase of slow sedimentation of a distinctive post-kinematic "overburden".

The models were successful in artificially separating the development of the elevation and pressure heads thus allowing the distinction of the different kinematics imposed by each drive. Lessons learned from our models allowed recognition of the subtleties of different timings of the two drives that overlap to various degrees in seismic profiles of natural salt tectonics.

**A few specific questions or comments**

1. Does the complex pattern of crosscutting zones of extension in the footwall peripheral sink of the left-hand primary pillow shown on Fig. 7a merely indicate paths along which the overburden was sieved?
2. The authors are to be congratulated on developing a granulate-and-silicone model "overburden" with a density only slightly higher than the natural salt-cover density contrast. However, I look forward to future work telling us the depth at which this new model overburden compacts to a density that exceeds that of the model "salt".
3. The authors claim that buoyancy forces were not overestimated in their models. However, was any buoyancy of the "salt" required at all for pillows to form? Lateral thickness changes in an overburden initially less dense than the "salt" would still have moved the "salt" into passive downbuilding of reactive "salt" structures.
4. The author's first conclusion is that there must be a thin pre-kinematic overburden layer for minor basement deformation to induce considerable structural relief in the top of "salt". But surely experiments showing what happens when no pre-kinematic overburden is present are required to justify this claim. Even Warsitzka, Kley and Kukowski (2013) lacked such

experiments. Surely differential sedimentation of overburden initially less dense than salt without any pre-kinematic overburden would still downbuild reactive salt structures.

5. The author's second conclusion claims that a phase of tectonic quiescence is required to trigger the development of salt pillows after basement extension. I consider that more experiments (without any such quiescence) would be required to demonstrate this claim.
6. The authors refer to the faulting they applied to their models as "minor". Their short pulse of basement faulting involved a model displacement of 7 mm (equivalent in the real world to ~ 700 m) over 0.3 to 10 h (that the authors equate to ~ 0.3 to 11 Ma in the real world). I calculate the author's fastest rate of extension (of 700 m in 333,333 years as 0.002 m/a= 2 $cmyr^{-1}$). I do not consider a fault offset of 700 m as minor, particularly at a rate of ~2 $cm/yr^{-1}$.
7. It is probably inevitable that we are all strongly influenced by the first salt structures we learned about. I first learned about salt tectonics in and around the Zagros Mountains where Neoproterozoic faults in the Panafrican basement strongly controlled the depositional facies and thicknesses of the Precambrian component of the Hormoz salt and Cambrian graben controlled the facies and thicknesses of its Cambrian component (Talbot and Alavi, 1996). Little is known about the Paleozoic cover that may have been >3km thick before the salt is thought to have first started to move in Triassic times. Nevertheless, the Hormoz salt appears to have first moved into (Triassic) pillows elongate N-S above old faults reactivated in the basement by throws of km long before the southern advance of the Zagros deformation front squeezed successively younger diapirs from the initial pillows in a process that continues to this day (Talbot and Alavi, 1996). This background has long made me wonder about all those tiny faults drawn in the top of the Zechstein basement by German geologists. Such faults strike me as much too small to have triggered the major sat structures above them. In other words, there are plenty of similar problems for these authors to work on in the future.

**References**


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