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Interactive comment on "Inversion tectonics: a brief petroleum industry perspective" *by* Gábor Tari et al.

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General Comments

This paper is a well-written review of those aspects of inversion tectonics most relevant to the petroleum industry. As such, it is not intended as an exhaustive review, but instead highlights certain aspects with some specific examples. The paper succeeds in this effort, providing a clear summary and important insights. The examples used are not the highest-quality seismic images of inversion structures, but they were chosen to support the message offered in this paper and are more than adequate. A welcome addition is the authors' emphasis on just why it is valuable for industry to properly identify prospects and fields that have an inversion origin, and why a more quantitative

and systematic approach is needed in the future.

Having said that, there are some relatively minor issues that, if addressed in the paper, would only add to the quality and applicability. These are itemized in the specific comments below.

Specific Comments

1. There is an almost exclusive focus on subsurface data in identifying inversion structures (although regional understanding of the basin evolution is also emphasized). Even in the petroleum industry, surface observations can provide important clues. For example, significant thickness and facies changes between the footwalls and hanging walls of thrusts can provide clear evidence of preexisting extensional faults. Classic examples include the southern Pyrenees of Spain and the Eastern Cordillera of Colombia (which is a major petroleum province). In the latter case, two papers had very different interpretations of essentially the same cross section: whereas Dengo and Covey (1993) interpreted most of the structures as thin-skinned deformation, Cooper et al. (1995) used, among other criteria, dramatic facies and thickness changes within synrift fill to demonstrate the preeminence of inversion tectonics. It would be good to add some comments about this additional tool in recognizing inversion.

2. It would also be beneficial, especially because Bally's figure includes it, to cover briefly some of the effects of salt during inversion. One is the decoupling of deformation above and below (e.g., Letouzey et al., 1995), which can complicate using observed large-scale geometries to recognize inversion or lead to secondary, intrabasinal traps (Stewart, 2014). Another is the varying impact of prerift, synrift, or postrift salt on the petroleum system, given its tendency to act as a barrier to upward migration.

3. Lines 80-81. Is this really a common problem? Forced folds are monoclines, with one side below regional, whereas inversion structures are anticlines that are above regional.

4. Lines 112-113. It's not clear just what data support this statement that "inversion" is used quite loosely in exploration. You have used the data mining to show that traps are rarely termed inversion structures, but this is just a statement. It's the same on Line 116 when you state that inversion tectonics appears to be overrated in exploration. Please clarify.

5. Section 4, first paragraph. Can you explain or speculate on what is generating the two different wavelengths of inversion structures? Distinguishing between regional and local inversion was highlighted in the introduction, so what is causing the differences in this case? Also, it's not clear how the map view pattern of the shorter-wavelength structures suggests an eastward propagation. Only timing data can tell you this. And finally, what are the orientations of the stresses you cite in the last sentence?

6. Lines 168-169 and Fig 6. It's very difficult to see the asymmetry with so much vertical exaggeration – I suggest redrafting with much less distortion. Also, it seems that the best indication that this is an inversion structure is not the asymmetry, but the thickness pattern in the Badenian and older strata. This should also be cited in the explanation around Line 175 and in the figure caption.

7. Lines 197-198. Salt-detached structures can still be part of inversion structures. So is there no basement inversion in this (Syrian) part of the Syrian Arc? Or could it be decoupled inversion due to the influence of salt? Also relevant to the caption for Fig 8.

8. Line 201. It says that the Syrian Arc includes part of the Western Desert, but Line 187 says from Sinai to the Palmyrides. Needs to be consistent. Also, it would be helpful to mention the time of rifting in the paragraph starting on Line 196. There's a mention on Line 207, but it's not clear whether this applies to the whole area or not.

9. Lines 212-215. The Syrian Arc I structures are asymmetric, but what is used to identify inversion on the more symmetric Syrian Arc II structures?

10. Lines 219-220 and Fig. 10. The text says that the NW limb is steeper, but the

figure shows what looks like a shorter, steeper SE limb (especially if Pliocene tilting is removed). Some might say that it's symmetric, so what criteria are being used to say this is in inversion structure? Is it the regional setting? If so, you have also cited the presence of folds detached on Triassic salt, so how can you distinguish between them?

11. Section 5.2. The geometries in Fig 11a are inconsistent in terms of clear inversion. This may be beyond the scope of this paper, but: Leviathan is the most obvious inversion structure, with a rift hanging wall inverted above regional; the prominent basement highs on either side of Tamar are bounded by uninverted rift basins and are themselves long-lived highs; and Tamar itself is enigmatic, with no clear underlying rift-bounding fault. So it really comes down to what you say in the last sentence, namely that an inversion interpretation is based by regional timing. You might add the similar timing of formation to known inversion structures.

12. Lines 262-265. This seems a little contradictory and thus maybe should be reworded. First it says that the source rocks in the hanging wall are critical. But then it says that hydrocarbons from here might migrate away from the trap, and that it is the footwall source rocks that are more important (as shown in Fig 12a and b). There is a similar problem in the conclusions (Line 337).

13. Lines 314-315. Figs. 7 and 11 are cited as examples where the source rocks are in the postrift fill, but don't actually show anything about the stratigraphic level of the source rocks. Either indicate these levels on the figures or remove this from the text.

14. Please indicate the locations of the Budafa and Lovászi fields on Fig 3.

15. There are a few places (e.g., Fig 13) with redundancy between the text and the figure caption. This should ideally be removed.

Technical Corrections

Marked-up manuscript with minor corrections sent to author separately.

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