Review of **Late Holocene structural style and seismicity of highly transpressional faults in southern Haiti** by Wang et al., submitted to *Tectonics*.

In this manuscript, the authors characterize the 10-15 km wide belt of late Holocene transpressional deformation by identifying a family of en echelon thrusts along the trace of the EPGFZ. They compare on of them, the recently active Jimani thrust with the Léogâne thrust fault, which is known to be active during the 2010 Haiti devastating earthquake. Their interpretations are made thanks to new sonar data from two lakes in the south of Haiti and previous geologic and geophysical observations already published.
This is an interesting paper with new observations and data on the transpressive faulting in the south of Haiti, but there are major errors in the interpretation of the chirp sonar profiles used to make the interpretations. My suggestions and questions are listed below and in the annotated manuscript:

In general this manuscript needs reorganization. In many parts, you do interpretations and conclusions before describing the data (see the annotated file), and the figures are cited randomly. In addition I suggest making entire sentences instead of lists over entire paragraphs.

You have to end the introduction with unresolved questions that your study answered, i.e. what is the state of art in your study area and why the new data and your study are important to improve it.

You say lines 195 to line 203 that there is a strong southwestward backthrusting of the Gonâve microplate in southern Hispaniola in Haiti (southern margin) and in Dominican Republic (Muertos trench) onto the Caribbean plate. In fact, the southern peninsula of Haiti is belonging to the Caribbean plate and the Muertos trench is the boundary between the Hispaniola and Puerto Rico block and the Caribbean plate (see Calais et al. 2016).

I don't understand the differences between what you call "strike-slip" and "trans-Haitian fold and thrust" models (section 3). All studies in Haiti agree with the fact that there is a fold-and-thrust belt propagating towards the southwest between two major senestrial strike slip faults. In my sense the only one question is about the eastern termination of the EPGFZ in the Cul-de-Sac basin (see fig. 2 of Symithe et al. 2016).
Moreover, I am confused because you say lines 222 and 235 "as what we are proposing in this paper" and "as we propose in this paper" in the part describing the fold and thrust model, but you say lines 526 to 529 "Our results … support the strike-slip model … as opposed to the fold and thrust model …".

Regarding the InSAR data (figs 2, 7 and 10), you should use the quasi-vertical displacement map rather than the descending interferogram, because it's not helpful for your interpretations. You already use the quasi-vertical displacement scale so use the proper map.

You must definitely improve the interpretations of the chirp profiles (figs 6 and 8). For example in fig. 6A, you identify thrust faults south of Jimani thrust. The

thrusts are in fact backwards; the uplift part has to be on the side of the arrow. There are onlaps near the surface that help you.
On the profile 6B, a near parallel profile from 6A, thrusts located at the base of the fold have disappeared. Where is the major northeast dipping thrust that you map in the other figures and in the fig 3 CC' cross-section?
Do you have some better images for the EPGFZ fault trace in the lake Azuey? I am not convince by the profiles of the fig. 6, I do not see any deformation that can be related to a large strike-slip fault boundary.
I suggest that you compare your chirp profiles with the one of Leroy et al. (2015) that shows the EPGFZ trace westward in sea.
You also need to fix the vertical scales and add horizontal ones.

The fig. 8 has the same interpretations errors: for example the thrust in the eastern side of the line M5 fold is backward (see onlaps top of the fold). Where are located the two majors normal faults that appear on fig. 7? How did you identify them? You need to better explain the extension that is mapped in the lake because it is not well observable on the sonar profiles.

Line 472 you say that the upper 7m of the Miragoâne lake was dated to 10ka and extrapolating to the observed thickness of 30m give an age of 33ka. I found 42.8 ka, can you explain your method of calculation?

You need to explain more how you can relate the most recent rupture of the 1770 historic earthquake with the variation of the acoustic impedance shown in fig. 9 (line 490). Is it because of discontinuities in the stratigraphy or composition of the layers?

Your abstract and conclusion have paragraphs that are exactly the same. Please make an effort to reformulate your sentences.

Overall:
There are some similar superficial conjugate thrusts, oriented in the same direction, in the Gulf of Gonâve. These active thrusts have been interpreted as the termination of the Haitian fold and thrust belt, and thought to accommodate the current compressional component of the transpression along with other distributed structures over the entire plate boundary (see Corbeau et al. 2016). Moreover, the zone of compression linked to the major strike-slip fault is much wider than the narrow swath mapped in this paper. For example, the 50 km wide southern peninsula of Haiti is a zone of linked transpression to the EPGFZ. So identifying the current deformation in the few kilometers wide valley of the main strike-slip fault zone would underestimate the current distribution of the transpression.
It is not obvious that the sonar profiles in the two lakes can discriminate if the transpression in this area is partitioned by en echelon thrusts adjacent to the major strike-slip rather than distributed on the overall plate boundary. I think the paper needs supplementary discussion about the origin and the role of the identified en echelon thrusts faults.