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Interactive comment on "The Ogooue Fan (Gabon): a modern example of deep-sea system on a complex sea-floor topography" *by* Salomé Mignard et al.

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We are thankful to Dr Hogdson for his interest in our paper and for his helpful and constructive comments. We have made efforts to clarify the parts of the manuscript that, in light of the reviewer's comment, needed to be more accurate and more detailed, escpecially by giving much more quantitative information. Here, we report below the reviewers' comments answers concerning the main issues.

General comments: The manuscript by Mignard et al. presents recently collected bathymetry, sidescan, and core data from the understudied Ogooue Fan, offshore Gabon. These data permit the authors to present an interesting case study clearly.

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Discussion paper

They demonstrate that changes in seabed topography above a stepped slope, formed by volcanic seamounts and mud diapirs/volcanoes, strongly influence the location of erosion and sediment bypass, and deposition, and therefore the distribution of faices and environments of deposition. There is nothing particularly new here – several modern/recent, ancient subsurface, and exhumed systems have shown similar patterns that indicate how sensitive turbidite systems are to subtle gradient changes. Nonetheless, more case studies will help the community advance understanding of threshold controls (e.g. gradient and confinement). With that in mind, and given the quality of data, there needs to be much more quantitative information on gradients, gradient changes, and dimensions of the erosional and depositional feature reported. Also, even though this is a case study, it does add to generic aspects of these system types, however the introduction is too parochial in scope. Specific comments:

1. Abstract: there is a random sentence to fix – wrong place? But the abstract can be more – more numbers, and clearer on what is novel here, beyond more knowledge of this particular system. What are the wider implications? 2. The introduction needs a rewrite. This is far too parochial in scope, and focused on the geological setting of the system, rather than providing a context for the analysis of stepped submarine slope systems in general. What is the 'gap' that can be addressed, or at least contribute to? There are several assertions or facts made where supporting references should be cited. 3. Suggest that a wider set of papers that deal with stepped slopes be cited and compared. What are the similarities and differences?

The abstract the introduction and the conclusion have been rewritten in the manuscript. The discussion has been expanded and reorganized and four new figures have been added. As suggested by both reviewers, most information given in the introduction is now placed in the "Context" section, whereas the introduction focuses on the role of complex topographic slopes on depositional processes and fan architecture. More literature concerning this topic is now cited and we are very thankful to the reviewer for providing us references that we could use. In this context, the study of the Ogooue fan

provides a good example of what impacts can very subtle gradient changes have of sedimentary systems construction. Indeed, the gradient change between the “ramps” and the “steps” of the slope found in the distal part of the slope is less than 0.4° (steps : <0.6°, ramps <0.2°). These gradients are very low compared to the ones found in the literature for modern fans (4° and 1° offshore Niger Delta (Barton, (2012)) or ≈1° and ≈0.6° for the Benin minor canyons (Deptuck et al., (2012)). This new data could be useful to study the gradient change threshold that is required to provoke sedimentation or bypass/erosion.

4. Please check for accuracy of use of turbidite (deposit) and turbidity currents (flow). Turbidity current deposit is fine, turbidite flow is not, turbidite deposit is superfluous.

In all the manuscript, we have been more careful about the use of “turbidite” and “turbidity currents”. We have removed, wherever they appear, the formulations “turbidite flow” and “turbidite deposit”.

5. In several places meandering is used to describe a channel form. You should use sinuous which is descriptive. Meandering is an interpretation, and a controversial one in deep-water, so should be avoided without supporting evidence.

We agree with Dr. Hogdson about our misuse of “meandering” for description of the shape of the channels. This has been corrected in the revised manuscript and “meandering” has been replaced by “sinuous” in the “Results” and “Discussion” sections. The sinuosity of Channel D has also been calculated in order to provide more details about the downslope evolution of channels. Sinuosity was measured at 2 km intervals because this length appeared to be the most adapted to the meanders with short wavelength and short amplitude found along the channel path. The same length has been used in Babonneau et al. (2002) to calculate along the present day Zaire Canyon/Channel.

6. Some numbers are used, but mainly for gradients. Overall, however, I would like to see much more quantitative information stated, with gradients, changes in gradients,

widths and depths/thicknesses of erosional and depositional features, sinuosity. This would really help elevate the paper, and allow worker to compare different systems, and consider thresholds etc., and to quantify relatively long, deep, shallow, etc.

We understand the reviewer request and the manuscript now provides much more quantitative information about the Ogooue Fan. All the morphological features referred in the text are described more precisely with numbers (depth, width, length and volume when possible). Moreover, following the request of the second reviewer, three new figures showing details of some morphological features of the system (The Cape Lopez lobe, the canyons ramp and the mid-system area with scours) have been prepared and will allow the reader to have more details on the Ogooue Fan. All the gradients are now expressed in “degree” rather than “in percent” to allow an easier comparison with previously published literature.

Also, on figure 5, we need more methodological information on how the levee height and channel depth (check spelling) are derived. Which levee, as seem asymmetric? How is base levee defined/identified? Using seismic data? Is depth measured from base levee?

Seismic data show that the channels incise the seafloor below the depth of the associated levees which are poorly developed and sometimes asymmetric. These features are similar with the morphology of the recent Congo channel-levee system. On figure 5, a small drawing is added to present the definition of the morphologic parameters measured from bathymetric data.

7. Cape Lopez lobe: This is not really ponded, according to your interpretations, as some of the flows are able to escape the confinement and pass downdip. This is a more confined step – and intraslope or perched basin, similar to several on the African margin (e.g. Jobe et al., 2017, and at outcrop, e.g. Spychala et al., 2015

We agree on our misuse of the expression “ponded lobe” for the Cape Lopez lobe located north of the Mount Loiret. This sediment body with one knickpoint and an exit

channel corresponds in fact to an intraslope sandy lobe which has developed on a confined step. This lobe is very similar with the “X fan” described in Jobe et al., (2017) and is in the same size range as the intraslope complexes studied in the Karoo Basin by Spychala et al. (2015): 8 km x 8 km, 76 km^2 for the X fan (Jobe et al., (2017)), 6 to 10 km wide and 15 to 25 km long for the lobe complexes in the Karoo basin (Spychala et al. (2015)) and 6 km x 16 km, and 106 km^2 for the Cape Lopez lobe. These morphological details have been added to the manuscript as well as a new figure showing the details of the lobe as requested by the second reviewer.

8. The echofacies interpretations are not done in isolation. You are using the interpretations of other studies, including your groups, which are sometimes calibrated with core. Therefore, need to cite supporting literature of these echofacies interpretation (as with sedimentary and seismic facies).

The previous studies that have been used to calibrate our echofacies when no cores were available have been more carefully cited. Among them, there are the studies of Damuth, (1975 and 1980), Loncke et al. (2009), Praston and Laine (1989), Gaullier and Bellaiche (1998), Piper et al. (1995) and Keynon et al. (1995).

Technical corrections: Several nomenclatural points to be consistent on: Seabed, seafloor, and sea-floor used. Stick to one.

Only “seafloor” is used in the manuscript in order to remain consistent.

Suggest use external levee, and internal levee or terrace deposit to be clear.

The description of the channel-levees systems has been clarified to be more accurate.

Also, fan, system, apron, are used, with various descriptors, turbidite, deep-sea. Be consistent.

We have removed from our manuscript the expression “deep-sea turbidite system”, “apron” and replaced them by the term “fan”.

Numerous suggested changes in grammar, spelling, and sentence structure are contained on the attached annotated pdf file. Supporting references for terms, such as sediment bypass, lobe complexes, etc.

We are very grateful to Dr Hodgson for its careful and detailed reading of the manuscript. All the changes that are suggested on the annotated pdf file will be found in the revised manuscript.

All the reference cited here can be found in the revised manuscript.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-99/se-2018-99-AC1-supplement.pdf

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