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Interactive comment on "Turning the tide: comparison of tidal flow by periodic sealevel fluctuation and by periodic bed tilting in the Metronome tidal facility" *by* Maarten G. Kleinhans et al.

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We thank the reviewer for critical comments that help us to clarify the work. Minor suggestions will be followed or taken as an indication for the need of textual improvement.

The main comment is that the reviewer understood this to be a methods paper and therefore misses important elements in the methods and considers this journal the wrong outlet for this paper. We will make more clear in the future submission what the scientific issue is and analyse in more detail what aspects of tidal flow are reproduced

in the Metronome and why it goes wrong in the classic setup. We agree with the reviewer that the tilting principle is indeed not new and the morphology produced in small tilting flumes as published earlier is promising, but as a reviewer of one of those earlier papers rightfully remarked, there is no evidence that this morphology happens for the right reason. Moreover, it remained unclear why the classic experiments by Osborne Reynolds could not work even though he initiated tidal flow as in nature. So, whether tilting produces periodic flows that are sufficiently similar to tidal currents in nature for the purpose of landscape experiments is therefore still an open scientific question, and why the flows driven by periodic sealevel fluctuation, as in nature, are not sufficient for experiments is not fully clear. In the manuscript we target this question by direct flow measurement, allowing comparison to well-known properties of tidal flow, and by modelling, allowing identification of the most important terms in the physics describing tidal flow. This will benefit selection of proper settings in tidal flumes that are being built, it will answer the inevitable reiteration of the reviewer question about tidal flow in future submissions and therefore it will be cited in future papers. Our main result is that flow in the experiments is much more friction-dominated than in nature, meaning that the tidal wave generated at the seaward boundary dampens out too fast in experiments to cause sediment transport in the estuary, unlike the tilting setup where the pressure gradient is enforced along the estuary by the periodic tilting.

We also note that the problem of novelty perceived by the reviewer in relation to the closing sentences in the discussion is countered by Reviewer Comment 2: RC2 requires more, rather than less, elaboration of the possibilities of the present setup. This shows a need for brief discussion that couples the present paper to our earlier papers on small-scale experiments with tidal morphology. Moreover, for the resubmission we will conduct modelling at a range of scales from experiment to nature to explore at what scales the tilting setup ceases to perform well and the Reynolds setup begins to perform well.

The reviewer suggests we should cite different, more fundamental papers on the use of laboratory experiments rather than our 2010 HESS paper, but provides no specific references. We chose this reference because it explains fundamental complementarity with modelling and field data analyses, all of which we use. This reference also refers to such more fundamental papers in earth science and in philosophy of science, but rehashing these in this paper as we did in our extensive ESR paper would take much space that we prefer to use for improved debate about the fundamental science questions. We will add some additional references though.

Here we explain the Particle Imaging Velocimetry (PIV) method better and we will add this to the paper. The method is the Large Scale Surface Particle Image Velocimetry (LS-PIV) developed by Uijttewaal et al. (2001) as also used in Blanckaert et al. (2012). This means that the particles are floating on the water surface and are lighted and repeatedly imaged from above as described in our manuscript. The image processing then proceeds as with classical PIV, in our case by peak cross-correlation, which is a windowing operation that does not identify individual particles as would be the case in Particle Tracking Velocimetry. Problems specific for the tilting flume are that the distance to the cameras changes over time, and that the flow conditions change rapidly. This led to the chosen measurement frequency, which is more than sufficient for data reduction to width-averaged flow velocity.

The choice of artificial soccer grass as the channel boundary needs more explanation and illustration. Two observations in past experiments led to this choice. First, channels get attracted to flume walls in many experiments, and perhaps also to valley walls in nature. This is due to two effects: first, the lower roughness of the sidewall compared to an alluviated bed with bedforms, and second, and lack of slope-driven sediment transport from a rocky sidewall sidewall that would potentially make a channel migrate towards the middle. The usual methods to prevent this are to use wider flumes and to add high banks of sediment, but even one locality of scour onto a smooth wall may self-amplify to strongly affect the large-scale morphology. Michal Tal and Chris Paola did an unpublished braided river experiment with groynes to try and push the chan-

nels out of the flume wall, but, as observed in nature, this caused local scours at each groyne that seemed to attract the flow rather than repel it. This is probably because the scale of the groyne spacing and scour holes approaches that of the channel width. The artificial grass, on the other hand, provides large roughness at a much smaller scale that prevents scour holes (Kleinhans et al. 2017). It is quite uniform and dense with rather rigid plastic stems that did not bend noticeably in the strongest experimental flows tested. As such, it also provided a large uniform roughness for the clear-water flow experiments reported in our manuscripts, it makes a gradual transition from alluviated sand-bed to a nearly fixated rough bed that is a more realistic bottom boundary than smooth steel because that would be much smoother than the sediment. Finally, it provides a soft protection for the thin steel floor of the flume. We therefore strongly recommend such artificial grass for landscape and bedform experimentation and will clarify this better in the manuscript.

Finally, we do not agree with the reviewer that papers with a methodological component do not fit in this journal. We consider this paper to have a strong methods component, but we target the science. At this moment, other papers considered for this journal report on rather methodological issues such as automated laser scanning for change detection, seismic signal analysis for rockfall dynamics measurement, measurements on geometry with an R-package, validation of digital elevation models, and so on and so forth. Clearly, there is a need to assess and publish whether and how these methods perform for specific and potentially more general subjects in the Earth Surface Dynamics and we believe this to be valid for our case as well. We think that the tilting flume is not a ‘specific piece of hardware for a very specific purpose’ as the reviewer suggests; rather, it is a technically simple but scientifically novel functionality added to a type of flume used worldwide for a great many different types of experiments of landscapes and geology, opening up new alleys for experimental biogeomorphology. Two new metronomes have already been built outside our institute for education and outreach purposes in the Netherlands and one metronome is being built in the United States for research on washovers. By providing information about the technical setup of the Metronome we disseminate the idea such that others can use it, and the evidence in the present paper shows to what degree it simulates tidal flows.

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