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Interactive comment on "Geomorphic implications of gravity currents created by changing initial conditions" *by* Jessica Zordan et al.

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Many lock-box exchange experiments with gravity currents are performed with horizontal flume floors. While gravity currents over horizontal surfaces do exist, many gravity currents flow down a slope. In such cases the slope-parallel component of the gravitational force acting on the excess density acts to propel the flow down the slope, and a steady flow may be generated when the gravitational forces are balanced by friction with the bed and the ambient fluid. Many lock-box studies do not address the question whether the two hydrodynamic structures on flat beds and sloping beds are fundamentally different. This manuscript explicitly studies the consequences of the differences in hydrodynamic structures between gravity currents on horizontal beds and gravity currents flowing down a slope. It thereby addresses a question that I have long wondered about when reading papers about lock-box exchange experiments. I applaud this effort, and think it is a significant contribution to the literature.

However, I think considerable improvements are needed in the presentation of the results and clarification of the text.

The results look like intricate quantitative analyses of the measurements, but are presented in a way that makes it very difficult to assess the answer to the research question. The main results from figures 4-7 are difficult to read because they are so overwhelming with dominating fluctuations. These results are in need of syntheses and parameterisation, such that the reader can interpret the effect of slope on the flow structure. I would suggest to present a selection of key runs in full page width figures; and to leave out the detailed measurements for the other runs. Figure 8 is a good start, but I would suggest to first plot the shape variables (Lb,S) against the slope, such that the effect of slope on flow size can be assessed (and similar plots for Hb and the time averaged velocity and bed and interfacial shear stress).

My second main concern about the manuscript relates to the reliance on Zordan et al under review a, and Zordan et al. under review b, which are not available at this stage. The postulation of new entrainment variables in sections 4.2 and 4.3 does not seem justified without publication of those papers. Furhtermore, I find it problematic to discuss bottom erosion by flows in section 4.3, based on experiments that did not include erodible beds. This seems to be a step back from the seminal gravity current erosion experiments of Garcia and Parker (Garcia & Parker, 1991, 1993). Why is the entrainment relation arising from their work (or any other entrainment rate, e.g. Dorrell et al. 2018, GRL) not sufficient? Likewise, for the water entrainment, the new parameter in section 4.2 is not explained in sufficient detail for me to evaluate its merit. Why not use the available Ri-dependent criteria? I suggest to cut sections 4.2 and 4.3 from the manuscript.

Below I have formulated my further comments to the manuscript. I hope the authors find these useful and constructive.

Yours Sincerely,

Joris Eggenhuisen.

Main Comments: - The postulation of new entrainment variables in sections 4.2 and 4.3 does not seem justified without publication of Zordan et al under review a, and Zordan et al. under review b, which are not available at this stage. Furhtermore, I find it problematic to discuss bottom erosion by flows in section 4.3, based on experiments that did not include erodible beds. This seems to be a step back from the seminal erosion experiments of Garcia and Parker (Garcia & Parker, 1991, 1993). Why is the entrainment relation arising from their work (or any other entrainment rate, e.g. Dorrell et al. 2018, GRL) not sufficient? Likewise, for the water entrainment, the new parameter in section 4.2 is not explained in sufficient detail for me to evaluate its merit. Why not use the available Ri-dependent criteria? I suggest to cut sections 4.2 and 4.3 from the manuscript.

-The structure of many sentences could be improved to increase the readability of the English text. I am clearly not a fluent native English writer myself, so I may not be the right person to point to style mistakes. An example of a sentence that I might suggest to be recast is the third sentence of the Introduction, which improves if the subject of the sentence is brought to the front: “Katabatic winds or sea breezes are examples of gravity currents in the atmosphere, in which the density gradients are caused by temperature inhomogeneities.” Let me be clear to state that this comment does not reflect on the quality of the science presented in the paper.

-P2L19-24 I am not sure I understand the authors’ perspective on the two flow dynamics states. In my understanding there are flows on horizontal planes that can be called momentum driven, these will always dissipate under action of friction as the flow spreads over the flat floor and finds a hydrostatic equilibrium with density gradients only in the vertical orientation. And there are flows down a gravitational gradient. If these

are steady, this implies a balance between gravitational driving force and friction on the flow, which is divided over the top and bottom boundaries (see for instance the definition sketch of the force balance in Konsoer et al (Konsoer, Zinger, & Parker, 2013). The dichotomy between friction-governed and gravitationally-governed is confusing to me.

- The frequency data in Figure 2 is not relevant to the results. It could be in the methods section, but I find the filtering and decomposition is already dealt with sufficiently there.

-P7 Figure 3. The variable H [mˆ2/s] is confusing me, please explain in more detail what it means, and how it is relevant to the research question of the manuscript. P8-12 Figs 4-7. The main results from figures 4-7 are difficult to read because they are so overwhelming with dominating fluctuations. These results are in need of syntheses and parameterisation, such that the reader can interpret the effect of slope on the flow structure. Figure 8 is a good start, but I would suggest to first plot the shape variables (Lb,S) against the slope, such that the effect of slope on flow size can be assessed (and similar plots for Hb and the time averaged velocity and bed and interfacial shear stress).

-P10 Fitting the logarithmic law of the wall to very thin gravity current boundary layers is notoriously difficult. Please report the precise approach taken. If I understand the workflow correctly z0 was the second free parameter in a two-parameter linear regression (the other being u*). Please report z0 and confirm that it had the correct size for hydraulically smooth flow.

-P11 Figure 6. The data in this figure is very difficult to read. Would it be possible to determine time-averaged shear stress values, and plot these against slope? Such a synthesis might be clearer than an accumulated set of panels showing all of the time series.

-P12L6 The definition of a novel ambient water entrainment variable is not clearly justified, and relies on the companion paper Zordan et al. under review a, which is not available to the readers of Earth Surface Dynamics at this time.

Minor comments: -The title is not clear; are the gravity currents created by changing conditions, or is the topic of the paper the geomorphic implication of changing initial conditions.

-P2L3 Traer et al. (Traer, Hilley, Fildani, & McHargue, 2012)is also a suitable reference here.

-P2L14 The initial trigger is emphasised to be of particular importance, but the experiments do not address the trigger of the flow. I suggest to rephrase this emphasis to align the statement better with the experiments and analyses performed.

-P2L22 The authors describe the common experimental observation that the head of gravity currents on steep slopes is fed by by the steady current in the body; Azpiroz-Zabala et al. (Azpiroz-zabala et al., 2017) have recently argued that this is a small scale experimental artefact and that real world turbidity currents in submarine canyons have a different structure.

Textual: -P1L15 not “confer” -P2L8 “Niño and Garcia”? -P2L16 Not clear, rephrase.

References used in this review: Azpiroz-zabala, M., Cartigny, M. J. B., Talling, P. J., Parsons, D. R., Sumner, E. J., Clare, M. A., . . . Pope, E. L. (2017). Newly recognized turbidity current structure can explain prolonged flushing of submarine canyons. Science Advances, 3(October), e1700200. Garcia, M., & Parker, G. (1991). Entrainment of bed sediment into suspension. Journal of Hydraulic Engineering, 117, 414–435. Garcia, M., & Parker, G. (1993). Experiments on the entrainment of sediment into suspension by a dense bottom current. Journal of Geophysical Research, 98, 4793–4807. http://doi.org/10.1029/92JC02404 Konsoer, K., Zinger, J., & Parker, G. (2013). Bankfull hydraulic geometry of submarine channels created by turbidity currentsâĂŕ: Relations between bankfull channel characteristics and formative fl ow discharge, 118, 216–228. http://doi.org/10.1029/2012JF002422 Traer, M. M., Hilley, G. E., Fildani, A., & McHargue, T. (2012). The sensitivity of turbidity currents to mass and momentum exchanges between these underflows and their surroundings. Journal of Geophysical Research:

Earth Surface, 117(F1), n/a-n/a. http://doi.org/10.1029/2011JF001990

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