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Earth Surface Dynamics Discussions

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

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Associate Editor We revised the manuscript to account for all the comments pointed out by the reviewer. In the following, we provide the answer to the specific points. Thanks to the precise and constructive comments, we hope the general quality of the manuscript, as well as the clarity of text and illustrations, has been improved.

The authors noticed that a unit typo had produced wrong values of slopes for Table 1, which are now corrected. Some text was modified to accommodate the changes which do not influence the discussion and final conclusions.

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Discussion paper

âĂČ Answer to O. Sequeiros The reviewer's suggestions are kindly acknowledged since they have an encouraging purpose of improving the manuscript. Reply to the queries made by the reviewer and the answer given by the authors are listed below. The English of the manuscript has been checked by a professional service.

General comments: 1) The word geomorphic in the title refers to change in morphology, including process of sediment entrainment, transport and deposition. It's probably too generic to be used in the title of the paper that mainly focus on the erosion process. The title was changed into: "Potential erosion capacity of gravity currents created by changing initial conditions". We have changed also in the abstract, line 7, "erosion potential" instead of "geomorphic". 2) We have specified in the introduction how the set-up is by adding the sentence: "The bottom of the channel was designed in order to have a variable slope angle of the lock and a following flat surface." A new reference has been added in the introduction: "Mulder, T. and Alexander, J.: Abrupt change in slope causes variation in the deposit thickness of concentrated particle-driven density currents, Marine Geology, 175, 221–235, 2001." In the discussion, modifications of the text are done as well in order to avoid misleading interpretations of the set-up configuration. 3) The function H(t)=ud(t)h(t) has been defined by considering both the gravity current contour and the depth averaged streamwise velocity because this represent a flow rate per unit width. The head is generally elevated with respect to the following body and it is also characterized by a core of intense streamwise velocity. Therefore, the function here defined takes into account both features. Finally, Nogueira et al. (2014) used a similar procedure which considered the depth averaged density instead of the velocity, hence defining the currents regions recurring to a measure of mass flux. The following sentence has been added in Section 3.1: "We can moreover notice that, by dimensional analysis, the function H corresponds to a flow rate per unit width." 4) The filtering process is actually filtering above the 8Hz frequency, this means that the filter passes (without modifications) signals with a frequency lower than the 8Hz. The following sentence has been added for clarity in Section 2.2: "The 8 Hz cut-off has been chosen because the signal, for frequencies higher than 8 Hz, showed white noise.” 5) The comparison is done looking at tests with lock-slope (S_i tests) and correspondent tests with same lock-volume but on horizontal bed (L_i tests). Figure 1 has been modified in order to clarify the parallel between the two sets-up: lock-slope and volume reduction on horizontal bed. The fact that results for tests S_i show a reduction in the streamwise velocity is therefore not caused by the reduced volume of release but a consequence of the developing descending flow in the upstream reach of the channel. To precise this point, the sentence in Section 3.2 has been modified to “By comparing tests S_i with the correspondent L_i tests, which have the same lock-volume but are performed without upstream slope, it is noticed that mean streamwise velocity is slightly higher for tests on horizontal bed.” 6) The reviewer is right and the sentence has been changed in the text. 7) The assumption of hydraulically smooth flow was indeed verified. The shear Reynolds number (or skin roughness, ks, normalized by the viscous layer) has be verified to be lesser than 5: ks u*/ ν <= 5 (Zordan et al.,2016). This is now in the manuscript. 8) In Zordan et al.,2016 the logarithmic profile method applied for one of the velocity profile is shown. The collapsed near-bed profiles of all tests following a line with equation u/u*=1/k(ln z/z0) is also shown in Zordan et al.,2016. This reference has therefore been added to the paper and a more complete explanation is made now. 9) We reformulated the misleading sentence in “Tests performed with a slope break at the section of the lock show. . .” 10) The computation of interface shear stress as proposed by Chikita et al (1991) requires estimation of the depth-averaged density which was not measured in the present study. The application of the regression curves for the estimation of the drag coefficients are derived for turbidity currents observed in a reservoir by Chikita et al (1991) whose application at our case study is arguable: these are physical factors that depends on the specificities of the case. 11) We took out the word “steady”. In the configuration with the greatest volume of dense fluid released, quasi-steady conditions can be reached in the body region but, as pointed out by the reviewer, with shorter locks the flow is not steady but inherently transient. 12) and 13) The main reason for potential bottom erosion reduction in essentially the reduction in the volume of release. To make clearer this point

I've added the sentence in Section 4.3: "This is mainly the result of the released volume reduction caused by the presence of the lock-slope, therefore originating shorter current bodies". The reviewer is right in his comment but we realize that this is the product of misleading text. In fact, our objective was to verify how the process occurring in the current initiated still at the lock, which may be caused by different densities and with different inclinations, would influence the propagating current in a downstream horizontal reach. The introduction and the conclusions were edited to make this clear. The physical interpretation is kept but now adequately spatially contextualized. 14) We have rewritten the sentence in Section 5 as "Bottom erosion capacity is reduced by the presence of the extra gravitational forces, most probably due to lower streamwise velocities which are consequence of gravity currents dilution." 15) We took into account your comment and therefore the conclusions were introduced with a new paragraph (Section 5): "In most practical situations gravity currents are flowing on different topographies and most of the time travels along inclined but discontinuous slopes (slope breaks). Moreover they are generally originated by the release of a certain amount of a fluid of various densities. The present study these both changing initial conditions which trigger gravity currents that are commonly observed in nature.".

Minor/format comments: 1) The new sentence is: "The shape of the current is modified due to the enhanced entrainment of ambient water and the body is the region of the current where this most happens." 2) The new sentence is: "The implications of an inclined lock on the potential entrainment capacity of the flow is here discussed." 3) Typing error: "in some cases". 4) I've corrected the typing error in the bibliography: Niño. 5) Caption Table 1 has been corrected. 6) The definition of R-square is added. 7) "faster material" was changed to "faster fluid". 8) Lh and Lb are also time parameter and therefore the statement T1=Lh and T2 =Lb are dimensionally correct. Anyway we wanted to highlight the duality space-time, which is important to translate the herein temporal measurements into spatial measurements, as others works may use. Therefore, a sentence was added in line 5 chapter 3.1:" L_h identifies the temporal extension of the head. The conversion from time to length scale may be done by using Taylor frozen hypothesis and considering a reference velocity of the current velocity as advection velocity." 9) Units have been added to the plots.

References: Chikita, K., N. Yonemitsu, and M. Yoshida (1991), Dynamic sedimentation processes in a glacier-fed lake, Peyto Lake, Alberta, Canada, Jpn. J. Limnol., 52(1), 27–43, doi:10.3739/rikusui.52.27. Mulder, T. and Alexander, J. (2001). Abrupt change in slope causes variation in the deposit thickness of concentrated particle-driven density currents, Marine Geology, 175, 221–235. Nogueira, H. I., Adduce, C., Alves, E., and Franca, M. J. (2014). Dynamics of the head of gravity currents. Environmental Fluid Mechanics, 14(2):519–540. Zordan J., Schleiss A.J. and Franca M.J. (2016). Bed shear stress estimation for gravity currents performed in laboratory. Proc. of River Flow 2016, St. Louis, USA, 855-861.

The revised manuscript is hereafter:

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2017-63/esurf-2017-63-AC1-supplement.pdf

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