

Interactive comment on “Physically-based model for gully simulation: application to the Brazilian Semiarid Region” by Pedro Henrique Lima Alencar et al.

Anonymous Referee #1

Received and published: 25 January 2020

Pedro Henrique Lima Alencar et al. 2019, entitled “Physically-based model for gully simulation: application to the Brazilian Semiarid Region,” studies the applicability of two simplified/empirical or physically based erosion models for a study area in the semiarid region of Brazil. The author’s choice of the study area is commendable as almost no attention would be paid whatsoever in such small gullies those are erosion susceptible. The author’s performed properly planned investigations, i.e., topographic survey and soil data collection. Two models are compared i.e., FL and SM and the authors propose a combined modelling approach which they name it as FL-SM model. The authors are familiar with the codes and have used them for their study efficiently. The model evaluation performed shows the reliability of their approach. I commend

[Printer-friendly version](#)

[Discussion paper](#)



the number of efforts put by the authors. However, I do have some major significant suggestions which would be helpful for the authors to improve the presentation of their data and readability of the manuscript. I would suggest major revisions, and after substantial revisions, this manuscript would be a useful contribution. Specific comments:

1. Abstract: Though the abstract tries to suffice most of the stuff from the manuscript, the way it is written makes it very hard to read and understand. A complete rewriting of the abstract is required. For example, the first two sentences could be written as “Gullies are most prone to erosion processes, leading to land degradation and desertification, especially in arid and semiarid regions. The second sentence could be written as “despite the slowly possessed threat of gully erosion, there are not many developments being made in this regard.”
2. Introduction: Similar to the abstract, the introduction part should also be revisited. The authors refer to the global and local scale importance of gully erosion by providing many examples but do not address how and why gully erosion is an issue in their selected study area. Further, the main contents of the manuscripts, the objectives and methodology part are not summarised in the introduction.
3. Coupled Model (FL-SM): The authors describe the governing equations of the FL model and SM model separately. It is not very clear how these equations are coupled and what platform the authors have used to run their simulation. The flow-chart showed in supplementary Fig. S3 should be moved to the main text and the modifications performed by the authors should be shown more clearly so as to ease the understanding of readers. Without that, it looks like the authors did not perform substantial modifications of the code. The evaluation of the proposed coupled model (FL-SM), along with the FL and SM models shown in Fig 6, doesn't tell the quantitative performance. I suggest the authors add the R2 value for each model and verify whether a statistically significant result is gained (p-value test) or not. The same can be done for Fig 8 and Fig 11. It is also not clear why the authors have shown the rainfall comparison plots within the modelling section. Further, the explanation of Fig 5, and its relevancy to the corresponding section (modelling) is not understandable.
4. Model evaluations: The authors have put a lot of effort into validating their new model. But it

[Printer-friendly version](#)

[Discussion paper](#)



is very difficult to understand the model evaluator shown in Figure 10. Further explanations would suffice. The evaluation for the gully growing modelling provides satisfactory results. It would be better if the authors explain what methods did Poesen et al. (2011) employ. 5. Discussion part: A long discussion is provided explaining the limitations of the model and data availability, i.e., topographic, soil and rainfall data. The titles of the sub-sections could be rethought as 4.1. model limitations and 4.2 data limitations. 6. English language usage: Though the authors have put a lot of effort into the technical aspects of this manuscript, the overall writing could be improved much. I suggest the authors have a native speaker to check and rewrite the manuscript. Technical corrections: 1. Most of the figures are referred to at places after the figure is presented. The flow of the overall reading is not smooth as many figures are misplaced. 2. The authors are requested to first site the figure in the manuscript and then put it somewhere near. Also, figures are pasted in different sections, which is not relevant. 3. Overall, I would suggest the authors perform professional proofreading and grammar corrections. 4. The supplementary files just contain some figures without any description regarding how to use them whatsoever. 5. In my opinion, this manuscript does not have major technical flaws despite the weak overall structure and complex writing style.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-460>, 2019.

Printer-friendly version

Discussion paper

