REPLY TO THE COMMENTS OF REFEREE #1

by Saletti Matteo et al.

We would like to thank the Referee for his/her helpful comments. Here is how we intend to address the issues that have been raised:

A roughness is a measure of amplitude. It seems very awkward to call roughness a variable that can be negative. What is called the roughness in this manuscript is basically a local downstream slope.

> We realize that roughness is not a good lexical choice and we indeed had discussed other alternatives. In the revised manuscript we have decided to call this variable Ex (Exposure).

This is not because you develop a "Reduced Complexity Model" (RCM) that it is not necessary to check some basic relations, such as the relation between sediment transport and slope. But, taking into account the previous comment, this is basically what you do when you investigate the mean roughness with respect to E and i_R. This relation should be used to set-up the model and not presented as a result.

> We plan to modify what is now in the first part of the results, related to the steady-state simulations, in the direction suggested by the referee. We plan to add a new section (e.g. 'Model Set-Up') where we will use some of the outcomes of the steady-state simulations to show the basic relations suggested by the referee.

There is no deposition length in the model and you never discuss the characteristic length scale of the exponential decays for particle hop distance. You need to test the dependency of this characteristic length scale to the system length and to describe how it varies with respect to the model parameter values.

> The referee is correct in saying that we do not specify a-priori any deposition length: the exponential distribution of particle hop distances arises as a consequence of the model's rules. In Figure 5d we show how the mean HD varies as a function of the entrainment parameter E and the input rate i_R. We do not discuss it in relation to the system length X because the maximum value of particle HD is always much smaller than X. In the revised manuscript we plan to give more relevance to this part and better discuss the implications of our assumptions.

You propose a two-dimensional RCM with only 3×10^3 cells. This is two orders of magnitude smaller than the actual number of particles in continuous numerical models that solve *turbulent flow and particle collisions. I do understand that size does not matter but you should explain and justify why such a small section of the bed is enough in your model.*

We realize that the “scale-issue” has not been addressed directly in the manuscript. We plan to do that in the revised version, by running a simulation with much greater grid dimensions, in order to show that size does not have an effect on the results, but only on the time needed to reach the equilibrium point.

Please clarify your initial condition. This is particularly important because I have the feeling that you can have stationary states for which there is no erosion or deposition. Furthermore, the steady-state is not very convincing from the fluctuations observed in Fig. 3a. You should also be more careful about your downstream boundary conditions and explain how the 10 sections that are removed affect the results.

In the simulations having a constant set of parameters (i.e. steady-state case) the initial conditions (ICs) of the system do not matter: the system is going to reach always the same “equilibrium point” in terms of average fluxes in and out of the reach. ICs only determine how long it will take to reach this attractor point. Fig. 3a, which is a zoom in a restricted time window, aims to show this is a rather dynamic equilibrium point, around which the system fluctuates. We realize this is an important point that will be discussed in more detail in the revised manuscript. The downstream boundary condition is not influencing the system already a few cross-sections upstream. We removed 10 cross-sections from the control volume just to be sure to avoid any kind of influence as is normally done in simulation.

Is there an increase of the sediment flux downstream leading to saturation and then to jamming? If it happens to be the case, how does this mechanism relate to the saturation flux and the saturation length in classical transport model?

Dynamic jamming is actually due to a sort of saturation process in the transport layer (i.e. it happens when the transport layer is full with moving particles). However, this process in the model is localized in single cross-sections, and does not have a direct relation to saturation length theories. Jamming is indeed caused by a local increase in sediment flux, which may be due to global changes in flow conditions (i.e. increase of the entrainment probability) or stochastic fluctuations in transport rates.

In relation with the previous point, is there a characteristic wavelength for the formation of step-pools? It could be informative to characterize the height distribution of these step-pools.

We did not address directly step-pool geometry and statistics (such as wavelength, height, spacing, etc), focusing only on step formation and stability as a function of jamming and sediment supply. We did not observe any specific step spacing, wavelength or step height that stand out beyond what would be expected from purely geometric considerations. However, we will look at some of these aspects in the revision process.

Before exploring the role of variable flow strength, it is necessary to understand the dynamics of step-pool formation. I have the feeling that the huge amount of deposition creates a barrier, which is subsequently responsible for net deposition upstream. Is it the case in the model and in nature?

The referee is correct: the process of jamming enhances deposition upstream as it happens in nature where steps are formed, among other factors, by deposition and clustering of bed material around large clasts (i.e. keystones). Of course in nature the process is much more complex (depending on hydraulic conditions, geometry, grain-size, etc): here we try to address the step-formation process with a RCM approach by assuming that the granular effect of jamming is what drives step formation. There are other ways of forming steps, e.g. by erosion of the bed around keystone clasts, but we are of the opinion that flume experiments and especially steps in natural streams form predominantly by the joint blocking of coarse grains in motion close to threshold conditions.

I suggest to plot a space-time diagram for the formation (and the disappearance) of step-pools.

A diagram for step formation and stability (like the jammed-state diagram of Church & Zimmermann [2007]) would be a nice addition to the paper. We plan to work on that in the revised version.