Responses to Comments from two Referees and R. Schumer on "Morphodynamics of river bed variation with variable bedload step length"

Responses by Anna Pelosi and Gary Parker

We thank the two referees and R. Schumer for their insightful comments. We have modified the paper according to their advice, as outlined below.

<u>Referee 1 (V. Voller)</u>

1. It seems to me that eq. 18 is based on the assumption of equilibrium yet it is ultimately used in non-equilibrium calculations. I suspect this is fine but it might require some explanation.

The underlying assumption here is that the entrainment rate E is a local function of flow, but due to the convolution form of (5), the transport rate is perforce nonlocal (only approaching locality for the case of thin-tailed $f_s(r)$ and $\bar{r} \rightarrow 0$). The text has been rewritten to reflect this. The specific new sentence is as follows. "*In the present implementation of (4), then, we take E to be a local function of flow conditions, so that q is nonlocal according to (5).*"

2. The results clearly illustrate the curvature modification of the landscape surface arising from the proposed non-local treatment. In the long-time equilibrium limit, however, I note the surface shape prediction match the local non-linear behavior. Is this always the case? Or is this just a feature of the by-pass example problem solved in the paper?

Please remember that in our numerical solutions, we let domain $0 < \hat{x} = x / L_d \leq 1$, with the further constraint $0 < \varepsilon = \bar{r} / L_d \leq 1$. We are thus not considering the arbitrarily long domains necessary to capture asymptotic behavior. We have made this clear in the text as follows. "*In interpreting the results regarding the thin-tailed and heavy-tailed case, it should be recalled that the problem is solved numerically only over the domain $0 < \hat{x} = x / L_d \leq 1$, with the further constraint $0 < \varepsilon = \bar{r} / L_d \leq 1$. This constraint prevents attainment of an asymptotic nonlocal state*." Asymptotic behavior is now discussed in a new Section 4.

3. In regard to the above observation that under equilibrium conditions the non-local and local non-linear models coincide, it is worth noting that in a recent paper: F Falcini, E Foufoula-Georgiou, V Ganti, C Paola, VR Voller A combined nonlinear and nonlocal model for topographic evolution in channelized depositional systems Journal of Geophysical Research: Earth Surface 118 (3), 1617-1627. we show a similar result when using an alternative, fractional calculus based, non-local treatment. That is, in the study of an equilibrium deposition system, our predication of the land surface profile exactly matches that predicted by a local non-linear transport model. However, when we apply our fractional calculus non-local model to the equilibrium bypass system, essentially identical to the one studied in the current paper, we predict a concaved down profile for the land surface profile. Thus we see (predict) a signal of non-locality in the equilibrium landscape. In contrast in the non-local model proposed in the current paper, as we approach equilibrium in the system, it appears as if the nature of the non- locality adjusts in such a way to remove its signal in the landscape. So as I see it there is a

difference in the predictive outcomes between the non-local treatment based on particle step size distributions proposed here and those based on power-law thick tailed distributions and fractional calculus (presented in the JGR paper noted above).
Yes, in the long time equilibrium limit our non-local model coincides with the local non-linear one and the equilibrium profiles become linear in contrast with your finding that a non-local treatment gives rise to a curved equilibrium profile. The results that we have found is essentially imposed by the equation (29) at the final equilibrium (i.e.).

We have added a paragraph to Section 5 which compares our model with the model of Falcini et al. (2013). The paragraph begins with: "*Recently Falcini et al. (2013) have presented a nonlocal formulation…*" Thank you for pointing it out to us.

__Anonymous Referee #2__

1. Equation (3) introduces non-locality into the model. Yet it is barely justified in the paper, although it has many physical implications. Assuming that deposition results from this upstream integral is equivalent to assuming that the trajectory of the transported particles is entirely determined by the conditions of their ejection from the bed. This is reasonable if their trajectory is purely ballistic. It is likely to be true in air, probably less so in water, where the flow conditions influence the particle during its flight. It is certainly wrong in viscous flows, and when the particle travels over very long distances. This should probably be discussed briefly in the paper.

The Equation (3) underlies the hypothesis that the trajectory of the transported particles is determined not only by the condition of their ejection but also by a function fs(r), which describes the lengths of the patterns. I think that by means of fs(r) more complicated behavior (than the purely ballistic one) could be modeled.

2. The first point brings us to this: other "non-local" models have been developed, which in a sense represent the end-member opposite to the present model. Namely, the "erosion-deposition" model assumes that the flow sets particles into motion according to the local shear stress, and deposits them in proportion of the density of moving particles [2, 4]. In other words, it assumes that a particle forgets about the initial conditions quickly after it is set into motion. In the erosion-deposition model, non-locality is embedded into a physical variable which keeps the memory of past events: the concentration of travelling particles. The exchange between the bed and the reservoir of moving particles introduces a typical length lsat, often called the "saturation length" [1, 3]. In a way very similar to the model presented here, bedload is in phase with the flow conditions if the ratio of saturation length to domain length lsat=Ld vanishes.
When this is not the case, however, the Exner equation and the bedload transport equation must be solved jointly. I believe the erosion-deposition model is a better candidate for comparison with the authors's model than the naive model in which bedload adjusts instantly to the local flow conditions.

In our reply and our modifications to the paper, we have grouped these two comments together. We believe that they are valuable, and, have modified the text accordingly. We point out that one step length generally consists of many (~ 10) saltations, so that it may not be necessary to consider a relaxation length for inertial effects. Having said this, we note that our model for step length is purely kinematic, but suggest that the model could be improved by a better description of particle dynamics along the lines suggested by the referee. The added text in question begins with the following words: "*It should be pointed out that the formulation of (4) involves a purely kinematic description of particle step length...*"

3. The condition in which the comparison is made might be mathematically simple, but they are rather unrealistic. I cannot imagine a laboratory experiment where condition (23) would be satisfied at the inlet. In my view, the authors would significantly improve the paper if they could provide theoretical results for a realistic experimental configuration, which would separate unambiguously the non-local model proposed by the authors from prior models. The equation (23) is valid at the equilibrium at it allows to better compare the two formulations.

The referee has precisely grasped the reason we have used (23). Without it, there can be no meaningful comparison between the results for the two forms of the Exner equation. Implementing a form in the analysis that could also be easily implemented in the laboratory would defeat the purpose of the study; indeed, the results for the entrainment case would be initial condition-dependent. While we understand the referee's comments, we have decided not to include a case for the entrainment formulation specifically designed to model a laboratory experiment.

4. To me, figures 2-8 show too many curves together. They would be clearer with fewer time steps. In figures 2, 5 and 8, showing the difference between the data and the final state, instead of showing directly the data, might further improve clarity.

The choice of so many curves in single plots seems the better compromise to show the evolution of the phenomena in time. The alternative is to expand the number of figures, each with less information.

R. Schumer

1. I think this manuscript will pack more of a punch if it is put in context of previous non-local studies. Specifically, this manuscript focuses on the transient, pre-asymptotic transport that is observed in lab studies while the focus of others has been asymptotic PDEs.

Reply. We have reworded the manuscript to reflect this comment. We have defined our general sense of nonlocality in terms of a convolution integral that relates a parameter at a point (deposition rate) to all values of another parameter upstream (entrainment rate) by means of a convolution integral. We now quote Du et al. (2012) to justify this. We now a) specifically quote Schumer et al. (2009) in terms of asymptotic nonlocality, and b) add a section to the end of the paper, in which a nonlocal form of the analysis is specifically presented. The added material begins with the sentence, "*Before continuing, it is of value to specifically indicate what we mean by nonlocality*."

2. Is it true that the flux form is asymptotic (continuum) while the entrainment form is a discrete model that can converge in some limit to the to the flux form...or more to the point of this paper…will have solutions that resemble the flux form solutions pretty quickly if the step lengths are short enough with respect to the domain that you can add a bunch of steps up and start to converge to the continuum distribution (???) Perhaps this is obvious to those who know the entrainment and flux equations well, but as

someone from the probability PDE world, this was not initially obvious. A few sentences that describe this would help readers like me.

The continuous form has been in the literature since at least the 1960's, so we beg permission not to derive (4) from a discretized "random walk" formulation.

3. Also things like: on lines 277 and 278 I'm not sure that it is true that the flux form coincides with the non-local form, I think it is the reverse. When the steps are small compared with the domain length, you get to add a number of steps so that pretty quickly the entrainment solution resembles (converges to) the flux solution. In this case, the exponential non-locality goes away quickly a la the central limit theorem.

The sentence has reformulated as follows: "*As expected, the solutions of Eq. (28) and Eq. (29) collapse to the nearly the same results in the case $\varepsilon = 0.01$, i.e. when the mean particle step length is short compared to the length of the domain. Under this condition the local (flux) form, essentially coincides with the entrainment form.*"
We just wanted to say that the flux form (which is local) converges to the entrainment form when the step lengths are small compared with the domain length.

4. Lines 52-56: The motivation for use of heavy-tailed pdfs is that the non-locality is preserved in the limit. The continuum equations that govern asymptotic transport properties (ADEs, fADEs, etc) are of interest in many applications, where anomalous transport persists indefinitely (or over our observational timescales). Only when jumps or waits are heavy tailed do nonlocal asymptotic continuum equations play a role.

The sentences have been reformulated as follows: "*In recent years, considerable emphasis has been placed on asymptotic nonlocality associated with heavy-tailed pdf's for step length (e.g. Schumer et al., 2009; Bradley et al. 2010; Ganti et al. 2010). This is motivated by the desire to preserve nonlocality in the limit of large time, so leading to fractional advective-diffusive equations for pebble tracer dispersion corresponding to the now-classical fADE model (e.g. Schumer et al., 2009). Here we consider nonlocality in a more general sense, as outlined below.*"

In addition, we have specifically added Section 4 to address the issue of asymptotic nonlocality.

5. This is a style issue- I think the results would be easier to read if you discuss the results and parenthetically reference the appropriate figures within your story. Right now, you layout what your figures are with no discussion, which makes me go and look at the figures with no context and I have to figure out what their point is. Then there is another paragraph where you tell me what the results are. As reader, I would prefer that you start discussing the results and send me to a figure to support a point in your story as it comes up so that I know how to interpret the figure. If you prefer to describe the figures before the discussion, then more descriptive captions would help.

Thank you; we have modified the captions to be more descriptive. More specifically, we have greatly extended the captions of Figures 2, 4, 7 and 8, and somewhat extended those of Figures 3, 5 and 6. We hope that this makes the text more readable.