

Earth Surf. Dynam. Discuss., author comment AC2
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Comment on esurf-2021-16

Shawn M. Chartrand and David Jon Furbish

Author comment on "Examining the transport of sediment mixtures with coupled transfer functions inspired by a birth-death model" by Shawn M. Chartrand and David Jon Furbish, Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-16-AC2>, 2021

Response to Comments Document for Chartrand and Furbish – esurf_2021_016

Dear authors,

The two reviewers provided a number of comments, which are constructive and detailed. Reviewer #1 (An) asks for clarifications on most parts of the manuscript. These should be fairly straight-forward to address. The comments by Reviewer #2 (Ancey), seem substantial to me. He developed the original birth-death Markov model, which serves as a basis for the present work, and identifies a number of problems with the theoretical set up.

I agree that the theoretical treatment would benefit from some clearer and more detailed explanations. In addition, I think the motivation / reasoning for choosing the particular line of analysis can be better communicated, and the relevant background should be explained. The Fourier transform analysis is introduced with a brief technical sentence (page 7, line 9), without providing any background information on the objectives of this step and on the mathematical details. While most researchers will have heard about Fourier transforms, not many geomorphologists will be familiar with the relevant details, let alone having used it.

Finally, it seems to me that there is some relevant literature that has been overlooked and I encourage the authors to amend that. Two particularly relevant publications are by Ghilardi et al., WRR 2014¹ and by Radice et al., WRR 2009², as well as chapter 5 in the thesis of Florian Heimann, ETH 2015³. There may be others; it has been a while since I worked on this topic.

Together with the revised manuscript, please provide a detailed rebuttal with answers to the comments and explanations of the changes you have implemented.

Looking forward to reading the revised paper!

Best wishes, Jens Turowski

Dear Editor,

Thank you for the opportunity to provide a revised manuscript for further consideration for publication with *Earth Surface Dynamics*. We appreciate the efforts of the referees in reviewing our work, and for providing thoughtful and helpful reactions to the presentation of our ideas. The revisions and responses below are based on careful consideration of their comments, questions and recommendations. To aid the review process, we separate our responses and explanations based on the three reviews provided by you, Reviewer #1 and Reviewer #2. Before presenting our responses, however, we offer the following brief synopsis of the major changes we provide with the revised manuscript.

We note that the reviews of our work are mostly focused on the need for clarification. In response we have fully revised key sections of the manuscript and added a significant amount of material. We appreciate that we did not fully explain the basis of our formulation of the transfer functions to describe the time series of the particle size fractions, in particular how the formulation is – and is not – related to the birth-death Markov model introduced by Ancey et al. (2008), and the interpretation of the coupled frequency response functions. To this end we have added a section (Section 2.2) that provides key elements of the birth-death Markov model as context for our formulation of the transfer functions. This is supported by a new appendix (Appendix B) that shows the basic derivation (and assumptions) of the birth-death formulation and its modification to include collective entrainment involving multiple particle sizes. This then provides motivation for our revised Section 2.3 (and Figure 3), which fully describes the transfer functions and how (and why) they differ from the birth-death model. This section is, in turn, supported by a new appendix (Appendix C) that shows how the transfer functions are cast in the frequency domain leading to our interpretations of the spectra of the transport time series. Also note that we have changed the title of the manuscript in order to clarify that the work was motivated by the birth-death model while hopefully avoiding giving the impression that the work involves an adaptation of the birth-death model to a mixture of particle sizes. We look forward to receiving subsequent reviews of our revised manuscript. Thanks in advance for your time and handling of our work.

Response to Editor Comments:

1. Need for clearer and more detailed explanations, including more attention to motivation

Response: Thanks for this important set of suggestions. We have addressed these comments through edits made to several sections of the manuscript, by developing additional text, and in general through revisions made based on comments provided by Reviewer 2. Here is a summary of the specific changes made:

- We have edited the last three paragraphs of the introductory Section 1 to more clearly lay out our work, with a re-focused emphasis on development of the coupled transfer functions presented in Section 2 of our work. This addresses part of Reviewer's 2 critique in the sense that our original submission incorrectly presented our work as being framed as a mixed grain size birth-death model. For example, we have revised the first three sentences of the third to last paragraph of the introductory section as: "Our specific interest here is collective entrainment as a sediment transport process, and we build from previous work in two ways. First, we propose coupled transfer functions for a mixture of particle sizes by making several assumptions, most importantly that fluid-entrainment of particle size mixtures is not a fixed Poisson rate (with associated fluctuations) (Ancey et al., 2008; Ancey, 2010), and instead can be treated as a random quantity which fluctuates in time, including the possibility of

temporal persistence. We provide an example of this using two particle size classes. The transfer function of each size class is an ordinary differential equation in time which depends on elements of the birth-death formulation: particle immigration and emigration, fluid and collective entrainment, and deposition (Ancey et al., 2008). The proposed transfer functions couple across particle size classes from larger to smaller through collective entrainment, and involve frequency response functions that act as low-pass filters. We explore the proposed functions in the frequency domain to examine how collective entrainment contributes to the various frequency components of the flux signals. More generally, the frequency content conveys information about the underlying sediment transport processes, although making a direct link is challenging (e.g. Radice et al., 2009; Dhont and Ancey, 2018). Second, we use similar time series techniques and analyze an experimental data set of 1 Hz sediment flux for six different particle size classes. As detailed below, the time series represent contrasting conditions of steady-state followed by a transient, which we repeat twice. We explore possible links between the coupled transfer functions and experimental data set to conceptually outline how collective entrainment may contribute to the transport of gravel sediment mixtures.”

- We have expanded the introductory paragraph of Section 2: Problem Set-up and Theory. The additions we have made provide the reader with a clearer understanding of our overall approach, a bit more background on use of Fourier analysis, and some basic rationale to support our work. Here is the additional text we provide: “Our work explores the time dependent character of particle-size specific sediment flux measured in the laboratory during development and adjustment of pool-riffle like bed topography (see Chartrand et al., 2018). Because sediment flux is measured in a variety of ways in the laboratory (e.g. Frey et al., 2003; Zimmermann et al., 2008; Singh et al., 2009) and the field (e.g. Hubbell, 1964; Helley and Smith, 1971; Rickenmann and McArde, 2007; Diplas et al., 2008; Rickenmann, 2018), it is necessary to establish context for the information carried by the transport time series that we report here (cf. Heyman et al., 2013). Below, we first relate particle number activity to the area-based flux which we measure using a light table device. The particle number activity is the total count of moving particles measured over a specified area of the bed during some interval of time. We then develop theory using Fourier analysis in order to conceptually explore the effect of collective entrainment to size specific particle activities. This effect is analyzed with particle size specific plots of the estimated power spectral density (PSD), which is a direct outcome of the Fourier analysis. The analysis occurs over the frequency range of 10^{-3} – $10^{-0.3}$ Hz. Last, we examine our experimental data and use insights from the Fourier analysis and published literature to identify plausible explanations for the transport behavior of a mixture of particle sizes. Our general approach is useful for a couple of reasons. Even under steady flow, the transport of sediment particles is known to fluctuate in time. Fluctuations carry with them information about the specific transport processes responsible for the signal being measured. Decoding this information is in part provided by results of the Fourier analysis, which converts a time varying flux signal into its frequency components and associated power, or signal magnitude. This representation provides an intuitive way to analyze and interpret bedload signals.”
- We have separated Section 2 into three subsections to help provide an improved presentation of the transfer functions. We have also tried to more carefully explain (without taking too much additional space) how our work builds from that of Ancey and colleagues, but also how it differs and why our approach deviates from the assumptions of the birth-death formulation. This expanded presentation sits at the front of the new subsection 2.2: Context for the Mixed Grain Size Transfer Function Formulation. We also note that Section 2 is now supported by two new appendices (B and C). Inclusion of the appendices permits us to keep the discussion in the main body of the manuscript focused, placing the details for interested readers within the appendices.
- We have incorporated the references suggested at appropriate spots in the manuscript.

Response to Reviewer 1 Comments:

1. Explanation of the results from birth-death model

(1) Lines 18-19 of Page 11: Compared with Run b, Run c (in Figure 4) has much larger values of μ_{22} and μ_{33} (the values of other μ are also different, but not as significant as μ_{22} and μ_{33}). I do not quite understand how the conclusion here is reached, especially how it is linked with the values of μ .

Response: We have removed Figure 4 in our revisions in order to provide a more focused and clear test of the proposed transfer functions, the results of which are now shown within a re-imagined and revised Figure 3. A primary outcome of revising the transfer function formulation is to show that regardless of the specific values used for the collective entrainment rate constants (coefficients), the effects of the low-pass response functions (see Eqs. 10 and 11 in the manuscript) – to shift spectral power to lower frequencies – is a persistent feature of the description. Importantly, the revised formulation shows that collective entrainment is key to shifting spectral power to lower relative frequencies, and that fluid-driven entrainment may not be as important in this respect as initially thought.

(2) Lines 19-21 of Page 11: Does larger AR(2) weight parameters mean an enhanced collective entrainment? How?

Response: We have removed use of an AR(2) process, and instead focus our example using a white noise and an AR(1) process as inputs for fluid-driven entrainment, in order to focus on the role of collective entrainment in the transfer function formulation. The results are clear. An AR(1) for the fluid-driven entrainment input will amplify the power spectral density across all bandwidths relative to a white noise, but an AR(1) does not shift power when compared to a white noise. Shifting is a function of the magnitude of collective entrainment.

2. Explanation of the experimental results

Figure 7 shows the temporal variation of sediment flux and bed elevation at fixed locations, which is very informative. Have you recorded the longitudinal profile/slope and the bed surface texture during the experiment? It would be interesting to see how the bed profile and texture respond to the change of water and sediment supply from the inlet.

Response: The requested results have been published previously by the first author in two separate publications. The reader is directed to these publications at several places in the manuscript, and we have made this link more clear with a minor edit within the Discussion section. Revised sentence within the first paragraph of the Discussion: "The contrasting transport behaviour for small versus large grains describes both steady-state and transient bed topography conditions (Fig. 6c) (for related and additional experimental results see Chartrand et al., 2018; 2019.)"

3. Comparison between experiment and theory

According to the experiment, fall of NPSD with frequency occurs at lower frequency of about $10^{-3} \sim 10^{-1.8}$, and the NPSD at higher frequency shows the character of white noise (Figures 12 and 13). Whereas according to the theory, the NPSD falls at relatively high frequency but keeps about constant at relatively low frequency. Could you explain or discuss such difference?

Response: Thanks for the question. We are not quite sure if we understand the question, but it appears the reviewer is asking us to explain why, for example, the NPSD in Figure 4a differs from the results of the experiment. As just mentioned, we have removed Figure 4 in our revisions, and our test of the transfer function formulation now focuses on Figure 3. A key outcome of our revised work is that we more clearly show the role of collective entrainment as a key factor in shifting power to lower relative frequencies, which generally determines the structure of the power spectra. However, we do want to be clear that our intent was not to directly compare between theory and experimental results because it is difficult to constrain values for the various rates and rate constants in Eqs. 7 – 11 (see manuscript). At this point all we can do is conceptually explore the problem in order to understand if it is justified to focus future work in a manner that reflects the proposed transfer functions. We make this clear at several locations in the manuscript. For example, see the last sentence of the second paragraph in the concluding remarks section: “Despite similarity between our experimental results and the transfer functions, the link made here is conceptual, and substantial work remains.”

4. Expression of grain size fractions

In the paper, the authors describe the sediment groups with two different expressions:

(1) the retaining particle sieve size; and (2) the actual grain size. In many places, the authors do not explain clearly which expression they are using, which makes me confused. For example, in Section 4 the authors define the finer grain size fractions as 4-11.3 mm. I do not know whether this is the retaining particle sieve size, or the actual grain size. If it is the former case, then the finer fractions should include the finest four groups in Table 3 (with the regard that grain size in Table 3 denoting the retaining particle sieve size, which is also not explained in the paper); if it is the latter case, then the finer fractions should include the finest 3 groups in Table 3. Also, the authors define the coarser grain size fractions as 16-32 mm. Again, I do not know whether this is the retaining particle sieve size, or the actual grain size. If it is the former case, but there should be no sediment coarser than 32 mm according to Figure 6 and Tables 2-3; if it the latter case, then which category should sediment in 11.3-16 mm belong to, finer fractions or coarser fractions? To avoid such misunderstandings, I suggest the authors to use the same expression for the grain size throughout the paper. For example, the authors could use the actual sediment grain size via replacing “4 mm” by “4-5.6 mm”, “5.6 mm” by “5.6-8 mm”, etc. in the tables and figures.

Response: Thanks for this important comment. We have made a large number of changes in the manuscript to address a consistent presentation of grain (particle) size information. We decided to go with the retaining sieve size presentation to be consistent with Fig. 6. Specifically:

- We have provide the following sentence at the beginning of the Results section: “Please note that presentation and discussion of grain size specific results from here forward are reported according to the six retaining sieve size classes of the experimental grain size distribution (Fig. 5).”
- We have modified Figs. 8-11, and 14-15 to include the phrase “Retaining Sieve Size ____ mm” above each sub-panel. The original text stated: “Particle Diameter ____ mm”.
- We have modified the captions for Figs. 8-15, A1, A2, B1 and B2 to indicate the results are displayed according to the retaining sieve size.
- We have made many edits within the results section to refer specifically to the retaining sieve size classes. In the discussion section however we generalize the language based on careful presentation in the results section.

5. Line 4 of Page 15: What do you mean by a “ramping up and down period”? What is it for?

Response: We have modified this part of the text to read: "Water supply rates were steadily increased over a period of 4-5 min until the desired flow was achieved (i.e. 42, 60 or 80 l s⁻¹). The same procedure was followed when the water supply was stopped. This was done in order to minimize bed erosion associated with a flood wave, or localized deposition due to a rapid loss of flow."

6. Section 4.3: I think it would be better to state clearly at the beginning that results in this section are particle count based. Since in the appendix, there are also results which are particle mass based. The same is for Section 4.4.

Response: The presentation of results in Sections 4.1 and 4.2 point out at several locations that the results are based on particle counts, which should prepare the reader for Section 4.3 and 4.4. However, we have added the following sentence to the captions for Figs. 11 (and 12): "Recall that the underlying sediment flux is based on 1 Hz particle counts (Fig. 8)."

7. The order of the appendices as listed at the end of the paper is not the same as they are referenced in the main text. Appendix D is referenced first in the main text, and then Appendices A-C.

Response: Thanks, we have corrected this mistake.

8. Appendix C: What is the difference between the particle-based flux discussed here and the count-based flux presented in Section 4 of the main text? My understanding is that they are basically the same, except that in Section 4 you present fractional flux, whereas in Appendix C you present composite flux. If this is true, then please use the same name (either particle-based or count-based) for both the main text and the appendix.

Response: We have edited Appendix D (note that the former Appendix C shifted to Appendix D because we added an additional appendix) to indicate "particle count-based", consistent with Section 4 of the main part of the manuscript.

9. Notation

(1) Line 12 of Page 4: What does f denote? Please define the parameter the first time it appears in the manuscript.

Response: f stands for frequency, and it was defined in the original submission on line 32 of page 2. We have added it to the list of notation so it is more easily known by the reader.

(2) In Section 2.1, N denotes moving particles within Area A , n denotes the number of particles that exits the right boundary from starting time. However, in Section 2.2, n_j denotes the number of active particles within area of A for the j -th size. Please keep consistent.

Response: Thanks. We have changed $N(t)$ to represent the number of particles which exits the right boundary.

(3) Does the superscript $*$ in Section 2.1 denote the characteristic parameters for the light table? Please clarify.

Response: Yes, you are correct. We have added this information to the list of Notation for clarity.

(4) Line 18 of Page 6: Shouldn't it be A^* rather than A ?

Response: No, the sentence is referring to the particle activity upstream from the light table over the bed area A. We have read the sentence a few times and believe this point is clear. No change made.

(5) Line 3 of Figure 3's caption: λ_3 □

Response: Note that Figure 3 has now been re-imagined and revised. In our revisions, however, we address the spirit of this comment.

(6) Upper part of Figure 4, what do ϕ_{11} , ϕ_{12} , ϕ_{21} , ... denote? Do you mean ϕ_{a1} , ϕ_{b1} , ϕ_{a2} , ...?

Response: You are correct, however, we have removed Figure 4 from the manuscript.

(7) Line 8 of Page 10: What does AR(2) stand for? Please write the full name of the abbreviation the first time it appears in the paper.

Response: Note that we have removed use of an AR(2) in testing the proposed transfer function formulation, and now use an AR(1). The abbreviation AR(1) is introduced in Section 2.3, on page 11, line 22 of the revised manuscript: "...are either white noises or have autoregressive (AR(1)) structure..."

(8) Equations 24 and 25a: The parameter A is used as area in the previous sections of the paper.

Response: Thanks. Note that Eqs. 24 and 25a have been removed in our revisions. However, we have made sure to address the spirit of this comment in the revised manuscript.

(9) Line 31 of Page 16: The parameter N is used for moving particle numbers in Section 2.1.

Response: We have removed use of "N" at the line and page indicated.

(10) Line 2 of Page 17: Please explain the meaning of $E[X]$ and $V[X]$.

Response: $E[X]$ is the theoretical expected value of a discrete random variable, and $V[X]$ is the variance of a discrete random variable. We have added these to the list of notation.

(11) Line 14 of Page 22: The abbreviation "pmf" should be defined at the first time it appears in the paper. Also, I am wondering if capital letters should be used for an abbreviation.

Response: "pmf" was defined in the original submission on line 22 of page 16. We have edited to "PMF".

(13) Considering that the paper includes so many parameters, I think a Notation Part at the end of the paper would be beneficial for the readers.

Response: Thanks. We have added a list of notation as the new Appendix A to the manuscript.

10. Equations

(1) Equations 21-23: I could be wrong, but it seems that the right hand side of these equations denotes the magnitude of $F_j(\omega)$, rather than the real part.

Response: Thanks for catching this mistake. Note, however, that Eqs. 21-23 have been removed as part of the revisions we completed.

(2) Equations D2-D4: I could be wrong, but it is more common to me to use $d\omega$ in the integration, rather than $\partial\omega$.

Response: Thanks for catching this mistake. Note, however, that these equations have been removed as part of the revisions we completed.

(3) Equations D9-D10, and Line 7 of Page 36: Is A the same for different size ranges? If not, maybe use A_j ?

Response: Thanks, this is a helpful suggestion. Note, however, that these equations have been removed as part of the revisions we completed.

(4) Cannot understand how you get Equation D11 from Equation D10c.

Response: In the previous presentation, take the square root of 1, apply the definition of A_j and note that the magnitudes are the signal amplitudes. Note, however, that these equations have been removed as part of the revisions we completed.

11. Figures

(1) Figure 3: The bottom of Figure 3 shows the sketch of particle entrainment and transport. In the sketch, is there difference between the dark gray circles and light gray circles? (Dark gray circles for collective entrainment, and light gray circles for entrainment due to fluid force? If so, please state in the caption.) Also, I do not see different behaviors of collective entrainment from the sketches of Panels b, c, and d. I cannot link the sketches with the corresponding text above. Please clarify.

Response: Thanks for the comment, and suggestion. We have added a legend to the bottom of the figure which indicates meaning for the different colored particles shown in the figure. As one moves right in the figures below the panels, collective entrainment increases due to more particles impacting the bed. This cascading effect is illustrated by the amplification of NPSD at lower frequencies in panels b --> c. We have modified the figure caption to help the reader more clearly understand how the sketches link to the calculations. Here are the opening lines to the revised content of the caption: "Example implementation of Eqs. (8)–(11) (see Appendix C for more details). The abbreviations are: WN - white noise, AR(1) - first-order autoregressive process, and PSD - power spectral density. In concept, differences observed in moving across the drawings and plots from left to right are due in part to increased collective entrainment of bed particles due to larger particles interacting with smaller ones on the bed surface. This combined with fluid entrainment increasingly amplifies the PSD at lower relative frequencies. More specifically, the results suggest that collective entrainment is more important for shifting spectral power to lower relative frequencies. (a) The PSD of an AR(1) process and WN are calculated using Eq. (11) for λ_1 and λ_2 "

(2) Figure 4: The figure shows the ranges for the input parameters of the Markov model. I am wondering is there references (or physical constraints) for these ranges?

Response: Please note that Figure 4 has been removed from the manuscript during revisions. However, we address this comment in the last two sentences of the Figure 3 caption: "For comparison, please see Ancy (2010) and Heyman et al. (2014) for aggregate experimental values of λ , σ and μ ."

(3) Figure 7: In the color bar, I do not understand the meaning of " $fr \ll 1.0 > fr$ ". Also,

the color bar lacks a scale. In Panel b, I suggest to add some text to explain what is the blue thick line (I guess it is the sediment flux measured by light table). Also in Panel b, the line of sediment supply is beneath the blue thick line, and therefore is not readable. In Panel c, what is the difference between the different lines? Also, how is the elevation averaged and normalized?

Response: The meaning of f_r is given in the text just below the color bar – it represents the ratio of sediment flux to sediment supply. The colorbar does not lack a scale. The very darkest blue represents a value of 1, and the lightest colors represent values $\ll 1$, or $\gg 1$, as indicated. However, in an attempt to be more clear, we have modified a sentence in the figure caption to address the suggestion for panel b: “The thicker line with a color range of near white to dark blue is the sediment flux, and line color represents the ratio of the sediment flux to supply rate.”

The figure caption indicates the difference between the lines in panel c. However, we have modified a sentence in the caption to try to be more clear: “Temporal evolution of average bed elevation at the 12 subsampling locations shown in Fig. 5; the thicker black line is the average across the 12 subsampling locations. The reader is directed to Chartrand et al. (2018; 2019) for more details regarding bed elevation data analysis.”

12. Table

(1) Table 1: I do not see "L.T." in Table 1. What is the footnote for?

Response: Thanks for catching this error. The footnote has been removed.

(2) Table 2: Are the Light table total mass for sediment of all sizes, or only sediment with grain size larger than 4 mm?

Response: Thanks. We have clarified the footnotes to indicate total mass for all particle sizes, for both L.T. and C.B.

(3) Table 3: What do minimum, maximum, mode, median, 90th percentile, mean, and variance particle count mean? Do you mean particle count per second? The same is for the vertical axis of Figures 8 and 10.

Response: These statistics are for the time series of particle flux for each time period indicated. The title to Table 3 indicates the underlying data was sampled at 1Hz. The captions for Figures 8 and 10 indicate the underlying data was sampled at 1Hz. No change made.

13. Typo

(1) Line 3 of Figure 8’s caption: two “reported”.

Response: Thanks. Correction made.

(2) Line 32 of Page 30: affect \square effect.

Response: Thanks. Change made.

Response to Reviewer 2 Comments:

I provide a partial report on the paper submitted by Shawn M. Chartrand and David J. Furbish to *Earth Surface dynamics*. I stopped reading after Section 2 because I failed to understand the theoretical developments. First note that when I introduced the concept of collective entrainment in my 2008 JFM paper, I was referring to a positive feedback loop in the stochastic process: the higher the number of moving particles, the higher the probability of entrainment. In other words, particle entrainment does not depend only on fluid forces, but also on the presence of other particles. This definition is different from the one used by the authors Line 21 on page 1. Among other things, I never stated that collective entrainment implies that two or more particles can be entrained at the same time (and in fact this scenario would conflict with the model's core assumptions). I have a hard time understanding how the model has been built. Here are the main points that were unclear to me:

- On p. 6, the authors say that they took inspiration from my 2010 JGR paper. If so, Eq. (5) represents the ensemble-averaged time evolution for the number of moving particles n_j , and this means that in Eq. (5) n_j is the mean number of particles (thus a deterministic variable); otherwise, the randomly fluctuating part (colored noise) is missing. They then took the Fourier transform of this equation. I do not understand why they introduced a Fourier transform of the entrainment rate λ_j , whereas the other parameters are assumed to be constant. In my model, n was the random variable, and the parameters λ , μ , and σ were constant (for fixed hydraulic conditions). The paragraph L20-32 on p. 7 seems to indicate that the entrainment rate λ was a random variable (in my papers, λ is the constant rate of a Poisson process; and so, entrainment occurs randomly, but its rate is constant). They then interpreted Eq. (8) as a low pass filter on λ . This leads me to think that they have entirely changed the nature of the problem I addressed: in my papers, hydraulic conditions are constant, and so are the entrainment and deposition parameters λ , μ , and σ . The number of moving particles n varies randomly as a result of random events described within the framework of jump Markov Processes (birth-death immigration-emigration processes). As far as I understand the submitted paper, λ is the random variable, μ and σ are constant, and n is a deterministic variable subject to a random forcing.
- Later, on p. 10, the authors assumed that the entrainment rate λ could be described using an autoregressive process. As λ was assumed to an AR(2) process, this means that the whole process was no longer Markovian because λ depended on what occurred not only at time $t - \delta t$, but also $t - 2\delta t$. This is an important assumption that is not justified. In principle, it should be possible to justify this assumption by plotting the partial autocorrelation function of λ (for instance, see the supporting information of my 2020 JGR paper), but as λ is not something that is easy to measure experimentally, it is in practice difficult to ensure that λ time variations can be described by an AR(2) process. Note that this assumption conflicts with the assumption that the number of moving particles is a Markov process. If n variations cannot be described using a Markovian process, then Eq. (5) cannot be used, or at least the reference to Ancy (2010) is not the right one.
- On p. 11, I understand that the authors combined the Fourier transforms of n and the power spectral density function of λ . If I am not mistaken, then there is a problem because their spectral density of n is just the norm of the Fourier transform of n , whereas the power spectral density of λ is the Fourier transform of the autocorrelation of λ . Without further guidance, I cannot understand how all these elements were combined to create Figures 3 and 4.

Summary of our Response and Revisions: We thank the reviewer for direct and helpful comments and criticisms, which provided us a clear path to address the issues raised. In responding to your comments we indeed recognize that the reviewed manuscript did not provide a clear description of our formulation and how it is -- and is not -- related to the birth-death Markov model. As noted in the comments, the presentation also had several mistakes. In particular, we neglected to clearly describe why

the treatment of a mixture of particle sizes cannot be done within the birth-death Markov framework that was formalized for a uniform particle size (or well sorted mixture). Nonetheless, the birth-death Markov work developed by the reviewer inspired our conceptualization involving simple transfer functions.

Our revisions based on the reviewers comments have led to a substantial revision to our development of the transfer functions, and their link with prior relevant work. We note that Section 2 has been revised into three separate subsections to provide readers with (1) an introduction to our proposed transfer functions -- Section 2.1, (2) a review of the birth-death Markov formulation including how and why our approach differs from this work -- Section 2.2, and (3) a summary presentation of the proposed transfer functions -- Section 2.3. We now supplement Section 2 with two new appendices: Appendix B which supplements Section 2.2, expanding on the birth-death Markov formulation to a mixture of particle sizes, and Appendix C which supplements Section 2.3 and provides the necessary details for the summary provided in this subsection.

More specifically, the intent of Appendix B is to show why the birth-death formulation cannot be used for a mixture of particle sizes if aimed at determining the distribution of particle number states -- which is the motivation for stepping back and focusing on the transfer function description, noting that in doing so we do not assume consistency with a Markov jump process. Specifically, even though the fluctuations in the number activity are not formally associated with a birth-death Markov behaviour, the objective is to "...retain the essence of the effect of collective entrainment..." and show that on average more collective entrainment occurs during intervals of large particle activity than during intervals with small particle activity -- and that the frequency spectra of the activities of the different particle sizes are consistent with the idea that these activities are coupled via collective entrainment. Importantly, we do not assume that fluid-related entrainment is a Poisson process. Interestingly, and perhaps surprisingly, our empirical results of the distribution of number states appears to be a Poisson distribution for the coarsest particles (without collective entrainment) and a negative binomial distribution for the smaller particles (with collective entrainment), as if each size behaved independently of the other sizes.

1. doi:10.1002/2013WR014449

2. doi:10.1029/2008WR007192

3. <https://www.research-collection.ethz.ch/handle/20.500.11850/99779>