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Discussions

Interactive comment on "The role of velocity, pressure, and bed stress fluctuations in bed load transport over bed forms: numerical simulation downstream of a backward-facing step" *by* M. W. Schmeeckle

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I am thankful to the time and effort of Reviewer 1. The reviewer's comments are shown in red text and my responses are in black text.

This paper describes the effects of turbulence on the bed downstream of a backward facing step. Using state of the art modelling techniques comprising of LES and DEM interesting results can be obtained. In this paper the well-known backward facing step is simulated. Provided that the simulations have resulted in a wealth of information, the

presentation of the results is very limited and superficial and disappointing in relation to the promises of the title.

I am sorry that the reviewer felt that the results were "superficial and disappointing". The experimental paper by Nelson et al (1995), which this paper numerically replicates, is one of the most cited papers on sediment transport mechanics over the last two decades. Their paper showed that transport is correlated with near-bed sweeps and outward interactions, and they also showed that modeling transport using only the distribution of downstream, near-bed velocity (an idea put forth in Grass and Ayoub(1982) for fine sand) would be insufficient. My paper shows that sweeps and outward interactions are spatially related and are correlated with pressure fluctuations. I also show that vertical velocity plays a key role in grain motion. Motion of fluid into and out of the bed provides the key ingredient that is missing from analyses using only near-bed downstream fluid velocity. I shall try to make these points more clear in the introduction and conclusions.

In my opinion the line of reasoning "extreme fluctuations are responsible for particle entrainment" , needs a more careful analysis and description. This pertains especially to the mechanism of particle entrainment and motion as only parameterized drag, interpolated pressure gradient and buoyancy was accounted for.

Much of the text in the paper, most of the figures, and the two animations were designed to provide a careful analysis of the line of reasoning "extreme fluctuations are responsible for particle entrainment.

As no aim for the research has been provided, nor any hypothesis stated, the paper appears to me as a fast presentation of simulation results. If the aim is to understand particle entrainment by large-eddy structures (of extreme amplitude), there is far more to say about the mechanisms of entrainment. At least a sketch should be added explaining the forces acting on a particle. The associated particle equation of motion should be provided with a discussion on the neglected forces.

I attempted to write a concise explanation of simulation results that I believe are necessary to understand the development of finite amplitude bedforms. As noted above, I will expand on the aim of the paper in the introduction. The Nelson et al (1995) paper raised a number of questions about the mechanics and pattern of transport downstream of separated flow. I believe this paper answers some of those questions that remain standing. The reviewer earlier stated the forces acting on particles quite succinctly. I do not believe a sketch showing these forces adds important information for the reader. The two animations and their matching figures visually show in detail the relationship between turbulent structures and particle motion. The particle and flow equations are given in detail in a previous paper, and the physical basis of these equations are restated in the present paper. I have read many papers that have taken this same approach. If the ESurfD editors agree with the reviewer that all of that work should be cut and pasted into this paper, I will consider doing so.

Also a discussion is needed on the resolved vs. the subgrid scales for pressure and velocity fluctuations. It is not clear from this paper how these scales relate to particle and pore sizes and how the unresolved sub-grid fluctuations (pressure and velocity) are represented other than using a sub grid viscosity.

Agreed. I can elaborate on these points.

The author could address in more detail how the findings of this numerical experiment relate to the simple relations that use critical shear stresses (Shields) etc.

I do state, using the data in Figure 2d and 2e, that a monotonic relationship between mean stress and mean sediment flux cannot work. Nelson et al(1995) and other experimental research has shown that a change in turbulence structure, from that of wall turbulence, has a substantial impact on the relationship between mean stress and transport rate. Expanding upon that here would take me away from the focus of the article.

In order to be interesting for the readership it would be good to provide more clarity,

also in a quantitative way about the gain in understanding and accuracy that comes from this research. Otherwise it is 'just an example' without context and from which no clear lessons are learned. Despite the information that cannot easily be obtained from ex- periments the conclusion in this paper are not containing any new insights nor do they provide suggestions for an improvement of current sediment transport modelling tech- niques.

As I stated before, I will expand the introduction to clarify why the conclusions provide critical new understanding of bedload transport over bedforms that may prove critical to a "first principles" explanation of bedform growth. Importantly, the penetration of fluid into the bed (e.g. splat events) is critical to understanding the spatial and temporal distribution of transport, and in showing the relation between quadrant events, transport, and pressure. As far as I know, these conclusions are not in previous published research.

Page 218 line 16: The periodic boundary conditions for the inflow section are inadequate as the length of the periodic section is of the order of the water depth, where 5 to 10 times the water depth is considered sufficient. No validation is provided on the velocity field.

The current boundary condition is sufficient to provide law-of-the-wall turbulence statistics at the step. I could provide a supplementary figure.

In general there is insufficient information on the way the simulation was carried out. Reference to the JGR2014 paper is not enough service to the reader.

Please see my arguments above.

Validation of the model is extremely limited only figure 2e contains some data. However, hardly any discussion is spent on the comparison and the cause of the differences.

There are many ways to tweak the model to more closely match the experiments of

Nelson et al (1995). At such low transport stages any minor change to parameters result in large changes in the transport rate. I chose not to adjust parameters. The paper is focused on understanding the physics of sediment transport and not to produce a model to calculate transport rates accurately. The downstream peak in transport deserves some mention in a revised manuscript. Nelson et al(1995) left open the question as to how much of that peak could be the result of statistical variance of their sampling methodology. We (Potter and Schmeeckle, AGU Meeting Dec 2014,EP43C-3575) have largely redone the Nelson et al(1995) experiment. We find a relatively smooth increase in transport, similar to my simulation results in Fig 2e. Ideally, our new experiments would be included in this paper, but I feel that the simulation results as presented provide sufficiently new insight that they warrant publication now. I hope that the ESurfD editors will agree.

If particles are removed from the bed is affects the bathymetry locally. The authors should explain more carefully how this phenomenon was dealt with. Were particles really eroded? Did bed forms appear?

Yes, particles were eroded. The simulation was not long enough for bedforms to appear. I could provided a “zoomed in” figure and animation of the transport from a small section of the simulated domain.

Figure 2e, should be addressed in the results section. Fig 5, caption is not complete, triple (d)

Thanks.

The referenced literature is rather limited. Knowing the enormous numbers of papers on particle-turbulence interaction, granular and two-phase flows.

I included references to papers whose results were necessary to contextualize the reported results. I did not wish to write a review paper.

In my opinion the paper is publishable only in case it provides a careful and complete

description of the simulation method, including its limitations. Furthermore, a clear aim should be formulated that will be of help in structuring the paper and leads to a clear line of reasoning that results in sound conclusions.

Again, I hope that an expanded introduction will help to show why the conclusions are new and important.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 715, 2014.