Referee's report on:

Physical theory for near-bed turbulent particle-suspension capacity
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This contribution reconsiders the flow and sediment dynamics within a fully developed turbulent boundary layer and proposes a new dimensionless measure to determine the balance between the submerged gravitational weight of the suspension and the turbulent uplift. Some comparisons are given with experimental data.

In general this contribution raises some interesting issues but it requires careful revision to address several issues in their analysis.

1. Much of the analysis is based upon hydraulically smooth data sets and their interpretation, a regime corresponding to $u_* d/\nu \ll 1$. In this setting it is inevitable that the kinematic viscosity, ν, plays a vital role in the mechanics and associated dimensional reasoning. However, how relevant are hydraulically smooth conditions to natural settings?

2. The key new empiricism is to write a quadratic function for the dimensionless acceleration (eqn 7) and hence a cubic equation for $\overline{w'^2}$. This function is determined by fitting data from DeGraaff and Eaton (2000). Are there theoretical grounds for expecting a quadratic dependence? Or rather is the important insight to elucidate the dimensional dependence of the acceleration? Also given the comments in (i), how relevant is this fitted form for natural systems with non-vanishing hydraulic roughness. Could it be that roughness enters the expressions if the boundary is not smooth?

3. I wonder whether it is helpful to describe the dynamics in terms of an 'acceleration'. Instead, presumably, the mean pressure field is altered by non-vanishing gradients of $\overline{w'^2}$ in order to satisfy the 'vertical' momentum balance and it is through this pressure field that the particle-scale dynamics are affected.

4. Following (iii), I think more must be said to justify the particle motion and to explain why it is appropriate that the particles follow the motion of fluid elements. My approach would be to form the mean pressure field (see (iii)) and deduce the stresses on the particle due to it. It is presumably necessary to average over the particle size and therein it is necessary to assume that that the particle diameter d is much less than the flow lengthscale ν/u_*, a condition that reduces to $u_* d/\nu \ll 1$. Furthermore a more thorough analysis of the particle motion would naturally identify the Stokes number as the important measure of the effects of particle inertia.

5. Much is made of the realisation that Γ is independent of grain size (and/or settling velocity). For incipient motion in hydraulically smooth conditions, I suspect that this is already captured by the usual dependence of the critical Shields parameter $\theta_c \equiv u_*^2/(Rgd)$ upon the particle Reynolds number, $Re = u_* d/\nu$. When $Re \ll 1$, $\theta_c = K/Re$, where K is a constant. Thus incipient motion is determined by $\theta_c Re = u_*^3/(Rg\nu) = K$. This conclusion is identical to what is derived in the paper, but is surely in line with a 'competence'-approach to modelling sediment transport.

6. The need for a reference concentration or a flux boundary condition in gradient diffusion models of sediment transport: this is of course, a long standing issue in sediment transport research and one for which steady flows do not shed much insight. For steady flows, one might prescribe a reference concentration at a small elevation above the bed. Alternatively, one might prescribe an erosive flux, but then since the suspension is in a steady balance between erosion and settling, this also leads to a prescribed concentration at the base. It is only for unsteady dynamics that the two types of boundary conditions behave differently - and I wonder what this new formulation can say about the concentration, or its flux, in this scenario?

7. It is assumed that the concentrations are sufficiently dilute so that the particles do not affect the flow and thus the clear fluid correlations of eqn (7) are applicable. However the theory is used for volumetric concentrations in excess of 0.1 and so I wonder how secure this assumption is? It is also used qualitatively to describe the collapse of the turbulence, which might well be a phenomena associated with high concentration suspensions.

Other minor comments follow:

1. Page 1, line 22: Be more precise with the proximity to the boundary.

2. Page 2, line 18: ‘wall bounded’

3. Page 3, line 9: ‘prime’ rather than ‘apostrophe’.

4. Page 3. u_* ought to be defined in terms of the boundary shear stress.

5. Page 6. Better to say ‘net gravitation pull’.

6. Page 8. The parameter Z depends on grain size and so it is incorrect to say that the expression (14) is independent of grain size.

7. Page 10, line 21. I am not sure how the data from Cantero et al. is used because the the parameters are not identical. It must depend upon w_s.

8. References. There are many typos in the references - see, for example, the entries for Bagnold, Dorrell, Montes Vudela, Spalart and Vanoni.