Now, the equilibrium between drag and buoyancy in a uniformly fluidized bed can be represented (Wallis 1969) by

$$\frac{u^2\rho_f}{gd(\rho_s-\rho_f)}g(\epsilon)=\frac{1}{C_d}, \qquad [2]$$

where $g(\epsilon)$ is constant if ϵ is constant and C_d is a drag coefficient that depends on Re by way of an equation such as [A.14]. At very low Re, $C_d \sim 1/\text{Re}$ and [2] becomes at $\epsilon_c \cong 0.44$ (or any other constant value such as ϵ_{mf}):

$$\frac{u^2\rho_f}{gd(\rho_s-\rho_f)} \sim \text{Re}, \qquad [3]$$

which explains figure 8(b). Over a wider range of Re, [A.14] or some similiar correlation may be approximated by $C_d \sim \text{Re}^{-m}$, where $m < 1$, as in the authors' value 0.77, rendering [1] and [2] equivalent. There is also some influence of n's dependence on a weak power of Re. Figure 7 is therefore indeed a correlation of the fluid flux needed to produce $\epsilon \cong 0.44$.

The Re modified by R achieves better success in figure 10 because this compensates approximately for the factor ρ_s rather than ρ_f in the numerator of Q when [2] is compared with [A.7]. However, it would be more reasonable to leave Re alone and modify Q to make it more closely resemble the Froude number on the l.h.s. of [2]. Since the factor in square brackets in [A.7] does not vary much, the required factor is approximately R. Rather than using R itself, which would move both the line and the points in figure 9, it is simpler to leave the line in place and multiply the authors' Q by the density ratio of the particles. This has the property of raising the open points in figure 9 by a factor of $2.47/1.19 = 2.08$ and lowering the solid point by a factor of $2.47/4.14 = 0.6$, which does indeed improve the correlation.

It is doubtful if any significant further conclusion can be reached from these data, either about Batchelor's theory or about the compressibility of a fluidized bed.

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RESPONSE

In his letter, Professor Wallis suggests that our recent experiments on stable fluidization have a simpler interpretation than the one we ascribe to them, namely that the systems studied exhibit an instability whenever the bed expansion reaches $\epsilon \approx 0.44$. A plot of Q vs Re is given in support of this interpretation of the experiments. His interpretation is incorrect on several accounts:

1. The mode of this instability is well-known to be a one-dimensional wave of *dilation*. Simple physical arguments and direct observations (El Kaissy & Homsy 1976) show that the particle motion consists of vertical oscillations about a mean position. Contrary to Wallis' hypothesis, particles will *always* be "sufficiently mobile" to allow this mode of motion. Other modes involving lateral shearing motions of the particles may indeed be stabilized by a yield stress associated with dense packing. We are not unaware of this possibility, which one of us (G.M.H.)

has studied and discussed in detail in the context of gravitational instabilities of the Rayleigh Taylor type (Didwania & Homsy 1981).

2. Wallis' interpretation focuses on the macroscopic scalings we determined. By emphasizing our figure 7, he ignores the *direct* evidence of stable expanded beds we report.

Why then, does his figure 1 apparently collapse our data for u_C? As we clearly point out in the paper, the "gap" between u_c and u_{mf} is under all circumstances very small, and decreases with increasing particle size. The small difference between u_{mf} and u_c results in apparent agreement with standard correlations for u_{mf}, but also to misleading interpretations.

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