Response to "Comment on 'The rheological behavior of concentrated colloidal dispersions' " [J. Chem. Phys. 101, 1757 (1994)]

John F. Brady
Division of Chemistry and Chemical Engineering, California Institute of Technology, Pasadena, California 91125

(Received 15 March 1994; accepted 7 April 1994)

Cichocki and Felderhof are correct that a factor of 2 was dropped (not from a failure to recognize something, however, but from a simple mistake) and my variable β should be modified accordingly. A copying error was also made in the numerator of Eq. (57), although the plotted curves are correct; the correct numerator should read: $1+2\beta+2\beta^2+\frac{8}{9}\beta^3+\frac{4}{27}\beta^4+\frac{4}{27}\beta^5$. Equation (56) is correct from which (57) and (58) can be obtained afresh by an interested reader. The overlooked factor of 2 does shift the curves to the right as Cichocki and Felderhof suggest. The agreement with experiment might now be considered less than "excellent," although the raw experimental data were not known and therefore it is possible a constant factor is needed to scale time to properly compare. In any case, the adjective "excellent" was meant to imply the collapsed scaling when $a^2/D_0^s(\phi)$ is used, not the quantitative agreement. I was aware of these oversights, as any careful reader would discover, and have made note of them in my forthcoming paper[1] on the long-time self-diffusivity in concentrated colloidal dispersions.

About these issues I agree with Cichocki and Felderhof. As to whether the scaled dynamic viscosity is a universal function of $a^2/D_0^s(\phi)$, my theory is a *scaling* theory as maximum packing is approached and decidedly *includes* hydrodynamic interactions. (Just because my final approximate equations after scaling resemble those in the absence of hydrodynamic interactions, does not in any way imply that hydrodynamics are unimportant or neglected in my development.) Conjectures made and analyses carried out without hydrodynamics, as Cichocki and Felderhof have done, may lead to a different conclusion. Any attempt to infer from a dilute or semidilute analysis the behavior near maximum packing is not something I would undertake lightly. The experimental data, viewed in total, do seem to conform to this universal scaling, and remarkably well for a wide range of concentrations, not just near maximum packing. Deviations from this universal behavior for small volume fractions are to be expected, however, as should be clear from my paper where I note the additional, subdominant, contributions to the stress, and this may explain why the analysis of Cichocki and Felderhof suggests otherwise.

I am pleased to see that Cichocki and Felderhof only question the functional form for the reduced dynamic viscosity. The much more important aspects of my paper, such as the scaling prediction that the viscosity diverges at close packing with an exponent of -2, the influence of interparticle forces on rheology, etc., are uncommented upon, and therefore I infer accepted. I also wish to add that I am, and was, aware of their work on this subject, but as it deals with dilute systems, its relevance to concentrated dispersions is questionable.

[1] J. F. Brady, J. Fluid Mech. (in press).

 0021-9606/94/101(2)/1758/1/$6.00