PHYSICAL REVIEW E VOLUME 56, NUMBER 1 JULY 1997

Reply to "Comment on 'Light scattering from the L_3 (sponge) phase: Evidence against logarithmic corrections to ideal scaling' "

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(Received 18 February 1997)

We rebut the claims made by Porte *et al.* in their Comment by discussing how our improved control of sample composition influenced the fit to data of proposed free energy scalings for the sponge phase, and consequently how the putative logarithmic correction term to ideal scaling is indeed discounted by our data. [S1063-651X(97)15006-1]

PACS number(s): 64.70.Ja, 82.65.Dp, 78.35.+c

The authors of the Comment [1] arrive at two conclusions regarding our previous paper [2]: (i) The data presented by us were essentially similar to their own previously published. (ii) Our data do not give convincing evidence against logarithmic corrections to ideal scaling.

Concerning (i) we pointed out in [2] that the two data sets probably were consistent. However, Porte *et al.* (in Ref. 3 of the Comment) had failed to properly specify, and apparently to even control, the compositions of their samples. This introduced an auxiliary experimental uncertainty ($\approx 30\%$) in their data. In our study we eliminated this uncertainty, reducing the relative error by an order of magnitude. The crucial question in this context is to what extent our improved experimental accuracy also significantly improves the ability to discriminate between the different functional forms for the concentration dependence of the scattering intensity. Porte *et al.* claim that this is not the case. With their less accurate data Porte *et al.* fit seven out of seven data points using Eq. (2) of the Comment. With our data we can with this equation containing two free parameters fit only three and four points, respectively, for our two series of nine data points. Thus, improving the experimental accuracy does make a difference. We also note that Porte *et al.* state in their Comment that they had additional data points that were not well fitted by their Eq. (2), which they did not report in their original publications.

In contrast, we proposed a three-parameter function based on a previously published theoretical model that fits the whole data set. Within conventional criteria we purport this operationally to be a more satisfactory fit.

(ii) Do the data disprove the existence of the logarithmic correction term? Porte *et al.* argue that there exists an unspecified correction term to their Eq. (2) that increases in importance with increasing concentration. Clearly, if one allows for such ad hoc assumptions no proposition can be disproved. However, our study demonstrates that if one wants to maintain the free energy term of Eq. (2) the unknown correction term actually influences the free energy over the whole stability range of the sponge phase. Apart from the problem of the quantitative fitting of the data discussed above there is also a discrepancy of a qualitative nature. Plotted on a linear scale all the indications are that the data (obtained up to the limit of maximum swelling of the sponge phase) extrapolate to a positive value at $\phi = 0$ and not to a negative one as the presence of a logarithmic term necessarily implies. In mathematical terms $d/d\phi(\Delta R(0)\phi)^{-1}$ is experimentally found to be an increasing function of ϕ and shows no sign of the $1/\phi$ dependence that Porte *et al.*'s Eq. (2) predicts.

Porte *et al.* spend a large part of their Comment discussing particular aspects of the thermodynamic theory [3–6] that we have used to derive the osmotic compressibility and hence the light-scattering properties. In doing so they do not distinguish between quantitative and qualitative aspects of that theory. The majority of the objections concern quantitative aspects of approximations that have been clearly stated in the original papers. The accuracy of these approximations will not affect the functional form of the free energy density given as Eq. (1) in the Comment but only the quantitative interpretation of the parameters extracted from our fit of the data. This is an interesting issue for future work but it has no consequence for the main question we address which concerns the proper functional form for the free energy density. Concerning the qualitative issue of the existence or not of a logarithmic correction term in the free energy, Porte *et al.* admit that there, in fact, does not exist an internally consistent derivation of this term, but they think that such a term could be derived.

In the original article [2] we focused on the light-scattering properties of a particular sponge phase and provided experimental data for discriminating between two qualitatively different expressions for the free energy density of such systems. The outcome of this test is clear cut. The data can be fitted on the basis of Eq. (1) in the Comment over the whole concentration range available. The data can only be made consistent with Eq. (2) if one introduces an auxiliary unspecified correction function with a non-negligible amplitude over virtually the whole stability range of the phase. The study is a contribution to an existing debate [7,8] on the factors determining the swelling behavior of sponge phases and balanced microemulsions. A study of a single system with a single method never settles such a broad question, but the complete picture must be built on studies of individual systems with relevant methods.

[1] G. Porte, J. Appell, and J. Marignan, preceding Comment, Phys. Rev. E **55**, 1276 (1997).

[2] J. Daicic, U. Olsson, H. Wennerström, G. Jerke, and P. Schurtenberger, Phys. Rev. E **52**, 3266 (1995).

[3] J. Daicic, U. Olsson, H. Wennerström, G. Jerke, and P. Schurtenberger, J. Phys. (France) II **5**, 199 (1995).

[4] H. Wennerström and U. Olsson, Langmuir **9**, 365 (1993).

[5] J. Daicic, U. Olsson, and H. Wennerström, Langmuir **11**, 2451 (1995).

[6] H. Wennerström, J. Daicic, U. Olsson, G. Jerke, and P. Schurtenberger, J. Mol. Liquids (to be published).

[7] D. Roux, F. Nallet, C. Coulon, and M. E. Cates, J. Phys. (France) II **6**, 91 (1996).

[8] J. Daicic, U. Olsson, H. Wennerström, G. Jerke, and P. Schurtenberger, J. Phys. (France) II **6**, 95 (1996).