

**Bonn and Meunier Reply:** In our Letter [1], we studied the stability of soap films of a nonionic surfactant under different applied capillary pressures on the film, notably the common black film (CBF)–Newton black film (NBF) transition. The theory developed in the Letter allows one to relate the lifetime of the metastable states directly to the colloidal interactions, and accounts for the stability (or instability) of soap films, a problem of considerable practical importance. In the accompanying experiment, we created the simplest model situation possible, in order to be able to compare with the theory in a meaningful way. Stubenrauch *et al.* [2] comment on a few aspects of our study, asking whether the comparison is meaningful. We believe it is, and reply to their detailed comments below.

(1) We agree that a hysteresis had been observed before in [3], and apologize for the omission. However, a quantitative understanding of the transition probability between a CBF and a NBF, as developed in our Letter, did not exist before. The lifetime measurements reported in [3] were in fact done for different systems, as a function of the electrolyte concentration. Our experiments, on the contrary, are done for a single system varying the surface pressure; it is only in our case that a meaningful and direct comparison with our simple nucleation theory can be made.

(2) The linearized version of the Poisson-Boltzmann theory is certainly doubtful in many cases, as is discussed at length in the soap film literature [4]. Also for our case, the agreement between the experimentally measured disjoining pressure and the theoretical expression for it is not excellent, and this is indeed likely to be due to the fact that the film thickness is on the order of the Debye length. Equations (2) and (3) should therefore be considered merely as an explicit example for which the energy barrier for nucleation can be evaluated explicitly; our theoretical scheme for calculating the nucleation probability, however, does also work for other forms of the disjoining pressure. This is the key point of our Letter: the comparison we make between the theory and our experiment is *independent of the precise form of the interactions* as long as one is close to the spinodal of the first-order transition. This is because the theory relies on a development of the interaction potential around the spinodal point. Consequently, our theory does *not* rely on the assumption that the electrostatic part of the interactions can be linearized and is universal in this sense.

(3) We agree that for nonionic surfactants the surface charge in general decreases with increasing surfactant concentration [3,4]. However, the results of Figs. 1 and 2 were obtained by varying the salt rather than the surfactant concentration. Comparing with the recent measurements of [5], several explanations for the observed differences are possible. Besides slight but possibly important differences in concentrations, surfactant purity, and *pH*, there may be a more fundamental reason, related to the hysteresis. Namely, the observation or not the metastable CBF state

depends sensitively on the time necessary to reach (mechanical, in this case) equilibrium in the metastable state, compared to the lifetime of the CBF. In the thin-film balance used in both experiments, the former also depends on the porosity of the frit that holds the film. As we were interested in observing the metastable states, in our experiments the porosity was taken large, so that equilibration was fast.

(4) As mentioned explicitly in the Letter, we start counting the delay time after the system has stabilized mechanically (the measured thickness drifts less than 5% per minute). That the measured thickness in this case indeed corresponds to metastable equilibrium states follows from the observation that the values for the thickness are the same upon increasing the pressure coming from a CBF and decreasing the pressure coming from a NBF. We thus start from a mechanically equilibrated (metastable) state, showing that the hydrodynamics of film thinning or thickening does not intervene in the nucleation time. We agree that the time to reach these metastable equilibrium states is long, and can be on the order of a minute or so, depending on the experimental conditions. However, this does not change our conclusions. If, in the experiment, the NBF had nucleated during the film thinning, the event is discarded. This is possible since the distribution of delay times is an exponential (Poisson) distribution (Fig. 3 of our Letter); consequently, the shifting of the origin of time zero has no impact on the nucleation time taken as the characteristic time of the exponential. Therefore our measured nucleation time is a well-defined quantity.

In summary, there is no need to reconsider either our theoretical treatment or the conclusions drawn from the comparison of our experiments with the theory.

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