Thus, Eq. (A.1) in the Appendix is not valid for the level swell analysis since $\langle j_f \rangle \neq 0$, as the liquid must be lifted at first and then falls back, while the level first swells and then recedes.

The steps between Eqs. (A.1) and (A.4) are intuitive and unsupportable in view of first principle evidence. The vapor generation rate is proportional to α [see local form of Eq. (1) above]; it depends on pressure (saturation properties) and on rate of change of pressure. The authors' assumptions are in conflict with these facts.

Returning to Eq. (15) of the paper, it must now be concluded that this equation is wrong for two reasons:

1. It ignores the fact that the vapor is escaping through the level interface with the relative velocity $V_g - dH/dt$, i.e., with the superficial velocity,

$$\langle j_g^- \rangle - \langle \alpha^- \rangle \frac{dH}{dt} = \frac{\langle \alpha^- \rangle}{1 - \langle \alpha^- \rangle} [(C_0^- - 1)\langle j_m \rangle + \langle\langle V_{gi}^- \rangle\rangle] \ , \tag{8}$$

   where j_m is the volumetric mixture flux at the level.

2. It assumes that the liquid superficial velocity is zero, which it cannot be.

It has been shown[3,4] that level swell analyses must always involve two fundamental principles. First, they must obviously involve conservation equations applied to control volumes with moving boundaries. Second, they must involve the mass jump conditions (conservation at the interface). Model formulations that contradict first principles are suspect at best.

It is interesting to note that the authors did not employ Eq. (21). Instead, they "tuned" U_∞ to fit the data. The risk is high that compensating errors keep the difference between prediction and experiment below 20% of total variation (Fig. 13). One cannot expect the model to work in general, for example at higher pressures or for bottom draining. Most importantly, however, ΔP measurements were used but are very poor for level measurements because they indicate only collapsed liquid levels.

Simplified models should not be in conflict with conservation laws; they should be shown to approximate rigorous models with necessary accuracy. Good agreement with experiments is necessary but not sufficient.

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Response to "Comments on 'Level Swell Analysis of Marviken Test T-11'"

We thank Wulff[1] for pointing out an error in our paper.[2] Equation (6) should refer to the unnumbered equation preceding Eq. (6) on p. 232:

$$c = x \frac{di_g}{dT} + (1 - x) \frac{di_f}{dT} \ .$$

The following sentence, which begins with "The mixture heat capacity..." and ends with "(note $C_{vf} \simeq C_{p,f}$)," should be ignored. Numerical calculations were based on the unnumbered equation rather than the approximation.

The other points raised by Wulff challenge the validity of the underlying analytical approach taken in the paper. The authors duly noted and are familiar with analysis that emphasizes analytical rigor.

The paper attempts to apply a semiempirical approach to level swell data obtained from the Marviken test and thereby provides a useful engineering tool for such an analysis. Due to the complex nature of the fluid behavior, a one-dimensional lumped parameter model is used, and a quasi-steady behavior is assumed for the liquid pool and the vapor phase above it. As stated in the paper, we were not attempting to present an exact solution due to its limited usefulness and complexity in implementation. Specifically, Eqs. (15) and (16) imply a first principle mass balance on the vapor phase essentially stating that the vapor mass accumulation is equal to the difference between the vapor generation rate in the pool and the rate at which vapor leaves through the vent. Under most practical conditions, the change in vapor density

$$\frac{1}{\rho_g} \frac{d\rho_g}{dt}$$

is smaller by more than an order of magnitude compared to the change in level

$$\frac{1}{H} \frac{dH}{dt} \ .$$

The assumption on the quasi-steady pool behavior was used to derive the general form of the relations between j_g and $\bar{\alpha}$. A reader familiar with the available data on this relation is probably aware of the large scatter in the data. Therefore, the approach of using an empirical parameter C_0 is frequently used in engineering applications and analytical studies to compensate for the actual complex pool dynamic. The value of $C_0 = 1.7$ used in this analysis is within its normal uncertainty bounds and on an average represents a good correlation for the available data.

The other parameter adjusted in the analysis was the bubble rise velocity coefficient in Eq. (21). It is unfortunate that an impression was given that this had to be "tuned" to obtain good agreement. The effect of the adjustment taken in the paper was a rather minor one, which in hindsight perhaps should have been left unaltered from the literature value of 1.53. This certainly would not have significantly changed the overall data comparisons or interpretations.

The authors do not believe the analysis presented in the paper to be in conflict with more rigorous models in applications where the approximations are appropriate. The application of these first-order methods is the main purpose of the paper. We would agree with Wulff that the analysis should not work, as presented, for bottom draining or for pressures sufficiently high such that the approximation $\rho_f - \rho_g \simeq \rho_f$ is not valid.

We cannot agree with Wulff's criticism of the ΔP measurements. It would appear that the pressure measurements in this test provide *excellent* indications of both the collapse and the *swell level dynamics*. The location of the swell level can be clearly inferred from the data, as pointed out in Sec. IV of the paper and depicted in Fig. 7.

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