Modélisation et simulation de la crise d'ébullition dans les REP à l'échelle CFD

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The Doctoral thesis of Luc Favre discusses the findings of an impressive effort to further the understanding and improve the modeling of boiling heat transfer, with the specific objective to support the prediction of DNB type thermal crisis in PWR reactors with CFD simulations. The thesis covers quite a range of aspects, starting from an initial evaluation of the baseline capabilities in the NEPTUNE_CFD code and all the way to a qualitative demonstration of DNB performance in a representative PWR fuel assembly, while providing its most valued contribution in the thorough re-evaluation of the closure models involved in the boiling heat transfer formulation.

A number of important contributions are made in this work. To my knowledge the thesis provides one of the best discussions available of the DEBORA experimental data for use in multiphase CFD assessment. The further comparison of the NEPTUNE_CFD formulation is also of great value to the community, providing a more consistent baseline, where the existing literature has often incorrectly interpreted or implemented sparse DEBORA tests. Chapter 5 is undoubtably the key contribution of this work. The thesis provides the most complete discussion of the experimental and modeling efforts toward a consistent representation of the ebullition cycle. The systematic effort to leverage the existing databases, including very recent ones from Kossolapov, and complementing it with a detailed review of the assumptions in the existing closures is certainly of great value to the modeling community. The effort to further extend the applicability of the models, leveraging a mix of analytical and semi-empirical approaches is massive and impressive for a single person effort. The thesis also presents to my knowledge the first effort to evaluate the possibility of directly leveraging the Zhang triplet to identify DNB, again demonstrating the value of this work in assessing the state-of-the-art proposals. While somewhat disconnected from the rest of the thesis, the analysis of the DEBORA-PROMOTEUR is also a valuable new contribution that could be extended in the future to support assessment of DNB predictions for fuel spacer design.

I have enjoyed reading the thesis and provided a lot of small comments directly in the manuscript, which serve mostly as discussion bullets. Here I provide a few of this discussion points which I hope to see discussed during the thesis defense:

<u>Chapter 3</u>
The chapter discussed first the use of R12 as simulations for high pressure water. While we have confidence on the scalability for bubble dynamics and general bulk flow behavior, it is unclear how R12 and other similar refrigerants can be use to scale CHF, as their interaction at the level of ebullition cycle is potentially different. Given the extensive assessment of all the components of the ebullition cycle performed in the thesis is there any finding we could leverage to better understand the scalability of DNB tests in refrigerant.

The chapter points at a potential error in the experimental quantification of the heat flux. Such uncertainty is shown to have a measurable impact on the void fraction prediction and would strongly impact the applicability of the DEBORA data for quantitative assessment. Has there been discussion with the DEBORA team about this uncertainty, and how to clarify it and possibly quantify it for example by simulating the full experimental apparatus.

Chapter 4

While not a fundamental issue, the results presented for NCFD show strange discontinuities, I believe it would be important to clarify what causes them.

The results shown in Figure 4.11 are clashing with expectations. Li's correlation was developed specifically to produced lower number of sites in respect to the Hibiki-Ishii model. Therefore, the temperatures predicted by the Hibiki Ishii simulations are expected to be considerably lower than those from Li. It would be useful to understand why that is not the case.

Chapter 5

This is the core chapter in the thesis and looking forward to see it discussed. The main thing to clarify is related to the decision to not include the modeling of the microlayer, and most importantly the later assessment of the model's performance. While reading it would appear strangely inconsistent that a model that does not include the microlayer influence on bubble growth could perform correctly at low pressure. Both experiments and DNS have shown that for water at low pressure the microlayer evaporation plays a dominant role in the bubble growth. As the microlayer is not expected to play any role at reactor conditions the choice of developing a purely high-pressure approach is reasonable. However, when testing the approach we should see the model largely underpredicting the bubble growth. Matching the bubble growth at low pressure without the microlayer contribution would indicate that the model is largely overpredicting the other contributions and should be either re-calibrated or modified. I look forward to understanding what I am missing during the defense. In particular in light of the conclusion that *"the new growth law can be of greater interest for low pressure boiling".* This seems inconsistent with the decision of not considering the microlayer which would then make models only applicable above 3 bars.

Chapter 7

While it might be a misreading on my side, it would appear that the work did not consider that the Richenderfer and Kossolapov experiments being a small local heater experience a developing boundary layer and cannot be simply treated with fully developed heat transfer correlations. If this is the case this section of the thesis should be revised and either discuss the error on the base of this difference or even better correct the results by accounting for the developing thermal boundary layer. If I misread the section I look forward to this discussion during the defense.

The review of the NSD representation should better discuss the difference between the Hibiki-Ishii model and the Li's correlation. This is very important as the two approaches have different meanings and should be chosen consistently with the HFP approach. The chapter has a very good discussion of sites interaction, which can be leveraged to explain why Li and Hibiki-Ishii are fundamentally different.

Finally, an important aspect to discuss is the adoption of the Departure diameters (radius) to quantify evaporation terms. While the RPI approach took this direction as the experiments at that time did not provide sufficient information to include a better description of the ebullition cycle, estimating the evaporation from the size of the departing bubble is inconsistent as it will double count the heat going to the fluid and then evaporating in the bubble. While simplifications are certainly necessary the departure diameter impact the heat removal with a third power effect so it becomes a dominant decision. This should be discussed in the thesis, as it will force the model to reduce the number of nucleations or the model would greatly overpredict evaporative heat transfer. Going back to the classic RPI this is why we have kept using the incorrect Lemmert-Chawla approach at high pressure, to compensate for the use of the departure diameter cubed in the evaporation term.

Chapter 8

The main comment on chapter 8 is what the chapter does not include. The early chapters had shown a range of temperature predictions in DEBORA and compared them to correlations, and later the NEPTUNE baseline model had been applied to the available experiments and compared for all conditions. Since all the setup and data are already available it would be expected that the new formulation results was then shown for the same conditions, instead a single condition is shown in Fig 8.11.

Further on Fig 8.11 the approach adopted to evaluate the HFP seems inconsistent and should be discussed and/or modified. If the implementation is based on a point averaged implementation, then why would you use the wall adjacent temperature instead of the bulk temperature? The results show that indeed you have a 10 degrees difference from the NCFD results, which for single phase should be accurate. Is this not simply deriving from incorrectly using the Tl at the wall adjacent cell.

In conclusion, I have greatly enjoyed evaluating this thesis work and I believe the boiling heat transfer modeling community will certainly value this contribution. I give a favorable opinion for the defense of the thesis of Luc Favre.

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