

Dear Ms. Cori Jia,

Thank you for allowing us to submit a revised draft of the manuscript “*CFD Prediction of External Thermal Loads on Film-Cooled Gas Turbine Vanes: A Validation of RANS Transition Models and Scale-Resolving Simulations on the VKI LS-94 Test Case*” for publication in the *MDPI Fluids* journal. We appreciate the time and effort you and the reviewers dedicated to providing feedback on our manuscript and are grateful for the insightful comments and valuable improvements to our paper. We have incorporated most of the suggestions made by the reviewers. Those changes are highlighted within the manuscript. Please see below, in blue, for a point-by-point response to the reviewers’ comments and concerns.

Reviewers' Comments to the Authors:

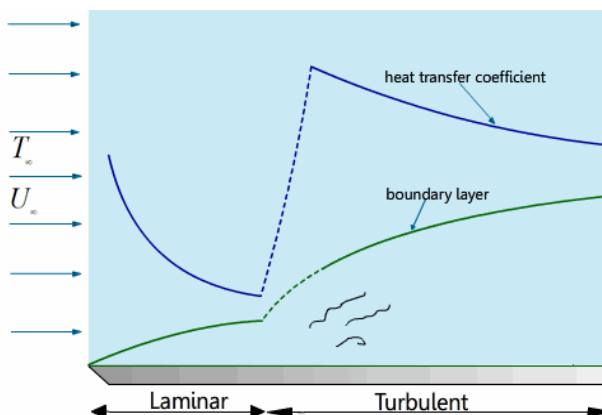
Reviewer 1

1. The gamma and gamma-Re_theta models predict a reasonable transition location for the un-cooled blade on the suction side but seem to miss transition on the pressure side. This is likely because the cooling holes act as a ‘trip’ in the experiments but not for the transition models. This seems not unexpected, as none of these models appears to be calibrated under ‘tripped’ conditions.

Author response: We thank the reviewer for pointing this out. We added the latter consideration to the conclusions.

2. The differences in HTC downstream of transition for the un-cooled case between the models are consistent. The fully-turbulent simulation will result in an earlier/stronger growth of the boundary layer and therefore a thicker boundary layer downstream compared to the transitional simulations. Thinner boundary layers have a higher wall shear-stress and therefore also a higher heat transfer. In other words, the later transition, the higher the HTC. It is not clear however, why the results even from the SST are above the exp. data.

Author response: We appreciate the reviewer’s considerations, however, we don’t agree with the last statements.



It is not true that “the later transition, the higher the HTC” because the later transition means a wider laminar region and so a lower HTC, as visible in the figure of the characteristic of the thermal boundary layer. The HTC increases only after the transition to a turbulent boundary layer. Then, indeed a growth in BL thickness leads to a reduction in the HTC.

Concerning the last consideration, the results from the SST are expected to be above the experimental data since this solution corresponds to a fully turbulent along the entire

vane. As a result, as said, the HTC is higher and reasonably overpredicted compared to laminar regions.

3. *The RANS study is carried out consistently and demonstrates the capabilities (or lack thereof) of these models.*

Author response: We thank the reviewer for the comment.

4. *The motivation and conclusions for the hybrid RANS-LES study (SBES) is less clear. Since transition is one of the main effects in these flows, one would expect that SBES would also need a transition model to capture the upstream laminar part, as the model will operate in RANS mode there. However, even when ignoring this aspect, one would still need to answer the question as to the mode in which the hybrid model is expected to operate. Is the expectation that the model operates in RANS mode upstream of injection and in LES mode downstream? If this is the goal, the authors would need to provide a LES-type mesh resolution in the region downstream of the cooling holes.*

Author response: No, we were not expecting that film cooling would have triggered the LES mode. Using this kind of hybrid RANS/LES model, the solution will always be RANS close to the wall, and we agree that in these regions there is no transition model, so the HTC is expected to be higher than the laminar one and similar to the fully turbulent solution. However, we expected that SBES would have improved the prediction compared to the SST model, due to a better turbulence solution in the freestream. However, a comparison is not straightforward, because turbulence are directly imposed as a boundary condition for the RANS, whereas SBES requires the artificial creation of velocity fluctuations through a volumetric turbulence generator which could not worksimilarly to what expected in RANS. Upstream of the film cooling we are surely more sensible to the boundary conditions compared to the downstream of the holes, where the film cooling itself determines the thermal loads.

5. *Figure 21 shows a relatively active LES contribution inside and near the injection hole, but it is not clear what happens further downstream. It looks like the resolved eddies are damped and the model reverts back to RANS? What is the expected benefit from such a setup compared to a fully turbulent RANS simulation?*

Author response: Yes, the resolved eddies are indeed damped due to the flow acceleration in the mainstream. The model is reversing back to RANS not in the mainstream but only in the very proximity of the wall where the shielding function forces the solution to be LES, as described in Section 3.5 "Scale-resolving modelling". Concerning the expected benefits, we expect benefits related to the prediction of every mixing-related phenomenon, e.g., the film cooling injection in the mainstream and its impact on the aerothermal loads. Film cooling, and generally speaking jets in crossflow, is well known to suffer the underprediction of turbulent mixing due to the isotropic turbulence closure used in standard RANS models.

6. *From the current reviewer's perspective, this test case is not suited for a hybrid RANS-LES model like DES/SBES etc. as the main effect of transition cannot be captured. In addition, the mesh resolution provided is most likely/certainly not sufficient to maintain the model in LES mode past the interaction zone with the injection flows. For the SS-injection case, where the SST model gives results very close to the exp. data, it is also not clear what additional benefit the hybrid model is expected to provide. The current reviewer does not see the added value of the hybrid RANS-LES simulation to the publication. It is more of a distraction/inconsistency than a helpful extension of RANS capabilities. It should therefore be removed from the article.*

Author response: We both agree and disagree, because this test case is dominated by near-wall effects, which RANS models, but, at the same time, LES resolution in the mainstream should improve the prediction. Indeed, the benefits associated with hybrid RANS/LES models

would be more evident in other applications such as combustor cooling, for example, where the aerothermal loads are dominated by the accuracy in the prediction of air-fuel mixing and the turbulent combustion process.

Concerning the inadequate mesh refinement, we kindly disagree because Pope's mesh criterion clearly shows that the mesh is indeed adequate to resolve at least 80% of the turbulence spectrum. Despite that, the proximity of the coolant to the wall may result in a solution affected by the shielding function and the RANS modelling close to the wall.

We acknowledge that the benefits of the SBES are limited. However, we believe that even though this result is negative, it is still significant in the context of simulations that are increasingly using LES and hybrid RANS/LES models.

7. *For the current case and with the increasing availability of cheap computing power (GPU), it would be much more interesting to carry out a LES (maybe with a wall model) to see which resolution would be required to improve results compared to RANS – including the transition zone.*

Author response: We agree with the reviewer that GPUs could enhance the analysis of this test case, but unfortunately, we do not have access to such hardware. A WMLES treatment would be undoubtedly more effective for this test case, but the laminar flow developing in the vicinity of the body's leading edge is not modelled when using this approach. Therefore, it must be completely captured by the numerical method, imposing quite restrictive grid requirements due to the very small thickness of the boundary layer in this region. A study (Slotnick, Jeffrey P., et al. CFD vision 2030 study: a path to revolutionary computational aerosciences. 2014.) estimated that the cost of accurately solving the very thin laminar boundary layer near the leading edge of an airfoil may be 10 to 100 times higher than the cost of solving the modelled turbulent region. Since the thickness of the developing laminar boundary layer in this region is very small, the computational costs become prohibitive. We added this consideration in Section 3.5 "Scale-resolving modelling".

Reviewer 2

1. *The paper deals with a topic which is extremely relevant for the turbomachinery community. The research, which is quite extensive and articulated, is carried out with scientific correctness. The results are interesting and the conclusions are supported by the findings. It is definitely a very good paper and this reviewer recommends it for publication in the Fluids journal.*

Author response: We thank the reviewer for the comment.

2. *Page 2, "Introduction section". It is stated that: "Natural transition: the most common type, due to the inherently unstable nature of the boundary layer at increasing Reynolds number". This is not true, especially in turbomachinery flows. Natural transition is probably the most common transition mechanism in external aerodynamics, that is characterized by high Reynolds numbers and very low free-stream turbulence. In turbomachinery bladings, boundary layers are exposed to relevant free-stream turbulence levels, and transition in attached flow is likely to be triggered by Klebanoff streaks rather than natural instabilities. Thus, it generally occurs in the by-pass mode provided that the boundary layer is attached and not separated. Please rephrase the mentioned paragraph according to these observations.*

Author response: We have fixed the error. The new sentence reads as follows: "Bypass Transition: it occurs when the laminar BL is disrupted by significant external disturbances, such as high levels of free-stream turbulence, accelerating abruptly the transition. For example, in turbomachinery blading attached boundary layers are exposed to relevant free-

stream turbulence levels, and the transition is likely to be triggered by Klebanoff streaks". Moreover, we deleted the "Natural Transition" bullet point to keep a close focus on gas turbines application.

3. Page 8, "Computational methodology section": The authors reports a value of 7.5 mm for the turbulent length scale. Is it the real turbulent length scale (e.g. $l = \sqrt{k}/(\omega)$) or is it the integral length scale? Please specify.

Author response: This observation is correct. It is the experimental integral length scale, which is used as a turbulent length scale to determine k and omega. We added this statement in Section 3.2 "Computational methodology".

Reviewer 3

1. Literature review is not sufficient, please add more new citations.

Author response: We agree and have extended the literature review on the application of transition models to gas turbines.

2. The definitions of some parameters in Table 1 are similar to many commonly used parameters. To avoid confusion, it is recommended to use other symbols or add subscripts. For example, pitch (g) is similar to the commonly used symbol for gravitational acceleration, vane height (H) is usually for the head of pumps, fans and compressors.

Author response: The parameters were chosen in this manner to be consistent with the reference PhD thesis of Fontaneto (2014), so we prefer not to change them to avoid misunderstandings or inconsistencies.

3. The mathematical accuracy of different parameters in the article varies, please unify them.

Author response: As suggested by the reviewer, we adjusted the parameters.

4. In Figure 4, I feel unreasonable about the given boundary conditions. For example, the GGI indicated in the figure should be periodicity, and the periodicity in the figure should be symmetry? Is what I said correct?

Author response: We apologize if Figure 4 is not clear. We added the following sentence related to the periodicities boundary conditions: "translational periodicities were set at both spanwise (Z) and radial (Y) directions".

5. Please add control equations for different turbulence models.

Author response: We added the transport equations for turbulent kinetic energy, laminar kinetic energy, and the specific dissipation rate for the k-kl-omega model, the intermittency and momentum-thickness Reynolds number for the gamma-Re_theta model, and the improved turbulence intermittency for the gamma model. For each model, we address the reader to refer to the ANSYS Fluent 2023R2 theory guide for details since the complex formulation.

6. Both 'mesh' and 'grid' are used in the text, please standardize them.

Author response: We have fixed the issue, we just used them as synonyms.

7. I didn't see your explanation of yplus (y+), it's important and needs to be supplemented.

Author response: We thank the reviewer for pointing this out. In Section 3.4 "Mesh sensitivity" we added a comment on the y+. We decided not to include a full paragraph with images on this topic since a $y^+ < 1$ is taken for granted when dealing with transition models applied to turbine vanes for heat transfer analysis.

8. *The difference between experimental values and numerical simulation values has far exceeded the difference between different mathematical models. Does this phenomenon indicate that this study is meaningless? (see Fig. 8)*

Author response: Figure 8 is related to the mesh sensitivity and, therefore, is focused on demonstrating the impact of incremental refinements, e.g. number of layers inside the prismatic layer and surface sizing, more than discussing the prediction capability of the RANS model. The differences between the experimental values and numerical simulation values are to be expected as we are considering a steady-state RANS turbulence modelling approach to study the vane's thermal behaviour, which is highly dependent on turbulence. If the RANS heat transfer coefficient had resulted in an accurate prediction, scale-resolving strategies would have been useless, but that would have been very surprising in our experience. Indeed, the mesh sensitivity analysis was just a starting point for the assessment of the different transition models.

9. *Pay attention to the issue of italicizing variable fonts.*

Author response: We adjusted the variables missing italic font. Concerning the heat transfer coefficient (HTC), it was italicised only in the description of the results, so when indeed using it as a variable.

10. *This study has unique characteristics in analyzing the leakage flow, but further elaboration is needed in terms of flow mechanism.*

Author response: Presuming the reviewer meaning "film cooling" with the expression "leakage flow", in presenting the results, Sections 4.1.2 and 4.1.3 report a detailed description of the different film cooling regimes at the PS and SS sides, mainly the penetration and the mixing regimes.

On the other hand, if with "leakage flow" the reviewer refers to the ingestion and re-injection of the mainstream hot gas inside the unfed film cooling holes we believe that further analysis exceeds the scope of this work. This phenomenon, also experienced in the experimental tests, is only described to clarify the reason behind the particular trend of the heat transfer coefficient near those holes.

11. *The conclusion section is not concise enough to serve as a summary. Please make significant revisions. It would be best if several important conclusions could be described separately.*

Author response: As requested, we tried to resume as much as possible the conclusions and rewrote them in the form of bullet points to better visualize the most important outcomes of the activity.