

Review of: "Aerodynamic Design and Performance Analysis of Mars Ascent Vehicles"

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Potential competing interests: No potential competing interests to declare.

Any successful experience in the application of computational fluid dynamics technologies is important, especially in the atypical case of its application to Mars atmosphere conditions and to hypersonic flow modes, so the reviewed paper can be considered relevant in both theoretical and applied aspects.

Comments on the paper text and content are given below:

1. Page 7: Move the "Reference" from the end of the Introduction section to the list of References at the end of the paper.
2. Page 7: **"Research Objectives"** look like a set of declarations without explanation of any reason to choose the two distinct geometries of mid-lift-to-drag ratio entry vehicles, namely: "Ellipsled and Hammerhead". The corresponding reference link is present, perhaps as an attempt to explain this choice by the results of research of Hollis & Hollingsworth, 2013, but why the reader should read this paper for understanding the choice of the authors of the reviewed paper? The aspect of the reasonable choice of the objects of study is very important, and it should be clarified.
3. Pages 7-10: In addition, the motivation for choosing the sets of geometric parameters of the two studied geometries (Tabl. 1, Tabl. 2) should be explained, at least briefly, but not just declared.
4. Page 10: The motivation for choosing the sets of calculation conditions (Tabl. 3) should be explained, at least briefly, but not just declared. Even if they correspond to "Mach 6 wind tunnel test" – the authors need to explain why exactly these conditions were chosen (all of them correspond to only one angle of attack 40 deg. and $M \approx 6$). Why was the angle of attack fixed? And what is the reason for the problem with the paper document **"Error! Reference source not found."** – it should be corrected both here and in all similar places below.
5. Page 11: I disagree with this statement: "CFD, a sophisticated analytical tool, enables the quantitative prediction of fluid flow events through digital computation, grounded in the fundamental principles of fluid mechanics.", because CFD isn't an analytical, but rather computational tool. In addition, this sentence is very general and can be removed from the paper without losing the logic of understanding its results.
6. Page 11: The sentence "Aerodynamic force plays a pivotal role in the flight dynamics of a vehicle, significantly affecting its performance and stability (Wu, 1981)" is also very general and can be removed from the paper without losing the logic of understanding its results.
7. I don't understand the first sentence of the "Methodology" chapter: "In the study, we used Tecplot, computational fluid

dynamics (CFD),...”. I think it also should be removed or finalized...

8. The subchapter topic “**Governing equation**” should be given in the plural form, namely: **Governing equations**”.
9. Pages 13-14: The description of the applied mathematical model is very typical and traditional, so this is a good place to remove the equations 3.1-3.7 and give the link to any CFD textbook with this set of equations.
10. Page 14: I suppose that the symbols u_i (u_k) in the formulae (3-6, 3-9), as well as in the expression for the pulsating kinetic energy k should be presented as: u'_i – pulsating velocity components. In addition, it is important to add the dash above the products $u'_i u'_i$ ($u'_k u'_k$), because this is a correlation.
11. Page 14: I don't understand the explanation about “the independence of the k - ϵ model in the free flow”. The reformulated form of the k - ϵ model in the SST model is used in the SST model, not in the “free flow”, but in the outer region of the boundary layer. And here I also don't think that the traditional SST model description (eqn. 3.11-3.20) is needed – this is a paper, not a textbook. I recommend to remove this description and give the link to some popular textbook associated with turbulence modeling.
12. Page 17-19: I recommend to move the RANS abbreviation and its explanation from the sentence “In this paper, a transition model based on the Reynolds Averaged Navier-Stokes (abbreviated as RANS) equation was used to simulate dynamic transition.” to the previous subchapters, because all the equations (3-6 - 3-20) are based on the RANS approach. And in this subchapter, I recommend again to remove the equations (3-21 - 3-33) and give the link to some popular textbook associated with turbulence modeling (this is a well-known modeling approach and it was applied by the authors of the reviewed paper without any improvements or modifications).
13. Page 20: The boundary conditions are not actually described at all. But it is indicated that “CFD software using an operator-friendly interface”, however, it is not stated which solver was used for pre-processing and computing. Mentioning Tecplot is not enough, because it is only a postprocessor... The grid also deserves a more detailed description together with an explanation of its structure, shock-waves adaptation (if this has been done), and ability to provide the grid-independent numerical solution.

The list of references (18 sources), the style of presenting the results, and the structure of the paper seem quite appropriate.

The paper generally corresponds to the profile of the Qeios journal. Thus, based on the above characteristics and comments, I consider it possible to publish this paper in the Qeios journal after correcting the above-mentioned shortcomings, since only clear formulations of the applied conditions for typical governing equations can be a guarantee of the correctness of the results and conclusions. Finally, this article requires **major improvements**, while having a chance to be published after the shortcomings are eliminated.