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Reply by the Authors to G. Mengali

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IN his Technical Comment,[1] Mengali has expressed his opinion on two papers recently published by us and another coauthor.[2,3] Before we consider Mengali's comments, let us briefly summarize the main points presented in our papers.[2,3] In the first paper,[2] we rewrote the linearized small-perturbation equations of aircraft dynamics in second-order form, used a fast–slow decomposition to derive literal approximations to various aircraft dynamic modes, and showed that it was important to account for the fast mode static residual in arriving at the literal approximation for the corresponding slow mode. In the second paper,[3] we developed on the ideas presented in the first paper[2] and derived consistent literal approximations to the longitudinal modes, that is, when the quadratic factors for the short period and phugoid modes from our derivation were multiplied, they satisfied every term in the fourth-order characteristic polynomial for the longitudinal dynamics. Thankfully, Mengali, although unwilling to acknowledge our contributions, does not dispute any of the points just highlighted. Furthermore, it was clearly stated in both the papers[2,3] that the results derived therein were not specific to any select set of aircraft data and that questions regarding the relative numerical significance of the different terms in the literal approximations were irrelevant in this context. Unfortunately, nearly all of Mengali's comments appear to arise from a lack of understanding of this point, coupled with his belief that "it is unlikely that at this stage something significant can be added about such an old problem."[1]

Let us first take up Mengali's comments[1] on the second paper,[3] where we used the ratio of the two timescales T_f/T_s as an order parameter ϵ and then derived consistent literal approximations to zeroth and first order in ϵ. To help establish consistency, all of the terms in the literal approximations and the characteristic polynomial were tagged with a subscript indicating their order in terms of the parameter ϵ. Strangely, Mengali misinterprets "order in ϵ" to mean "order of magnitude," and all his confusion and comments regarding this paper[3] can be seen to stem from this misunderstanding. Once it is clear that "order in ϵ" has nothing to do with the relative numerical value of the term, all of Mengali's remarks[1] about the second paper[3] and the numerical data in his tables become irrelevant.

Let us now return to the several frivolous comments by Mengali on our first paper.[2] For example, note the following points.

1) It is clearly stated that the dimensional stability derivatives are defined in the standard manner as in Refs. 1–3 of Ref. 2, which are standard textbooks in the field, but Mengali prefers another standard.

2) It is clear from the Appendix in Ref. 2 that the moment equations are written in principal axes and, hence, no primed derivatives (as per Mengali's notation) are required to account for the I_{xz} term.

3) Primed derivatives used in Eqs. (6) and (7) of Ref. 2 have been clearly defined following Eq. (7) of that paper, and there can be no cause for confusion.

4) It is also very clearly stated following Eq. (3) in Ref. 2 that rate derivatives for the force equations and derivatives with respect to $\dot{\alpha}$ are not considered for convenience, and not out of any regard for their numerical magnitude.

Mengali's accusation[1] that we ignore phugoid approximations other than that by Lanchester is ridiculous because we refer to the recent paper by Pradeep (Ref. 7 of Ref. 2) on phugoid approximations over the last century and also compare our phugoid approximations with those derived by him. Mengali then goes on to selectively quote us to say that "... they claim that 'previous literal approximations... assumed $\Delta\alpha$ and $\Delta\dot{\alpha}$ to be zero during the phugoid mode.'" However, the correct statement from our paper[2] is as follows: "... previous literal approximations *that did not consider the static residual value of* $\Delta\alpha$, *but* assumed $\Delta\alpha$ and $\Delta\dot{\alpha}$ to be zero during the phugoid mode, essentially ended up with the following equation for the phugoid dynamics...." By deliberately omitting the words in italics, Mengali tries to twist our statement to suit his convenience. Following this, Mengali suggests using a three-degree-of-freedom (3-DOF) approximation for what every one agrees is a 2-DOF phugoid motion and comes up with Eq. (3) in his comment,[1] which can be seen to be incorrect by comparision with Eq. (11) of our paper.[2]

Mengali's comments on our lateral mode approximations are even more bizarre. There is, of course, no sign error in Eq. (19) of our paper[2]; it is just that Mengali, as always, prefers a different sign convention. Again, Mengali is wrong when he suggests that we have claimed the following expression for the spiral eigenvalue to be a new result:

$$\lambda_S = \frac{\mathrm{LHS}\left[S^1_{\mathrm{lat}}\right]}{\omega^2_{\mathrm{DR}}\lambda_R}$$

However, when the new approximation for ω^2_{DR} is substituted in the preceding expression, the resulting formula for λ_S is a new result and is clearly not the same as Mengali's "well known" spiral approximation [Eq. (12) of Ref. 1]. Mengali then argues, without citing any supporting evidence, that the derivative L_β should influence only the dutch-roll damping, but not the frequency. However, a glance at the literature (e.g., Fig. 5.14, p. 201, of Nelson[4]) will establish that his argument is incorrect, and hence, all of his succeeding discussion on the dutch-roll mode is flawed. For example, while trying to factorize the term D in Eq. (21) of his comment,[1] he neglects the Y_β term in his Eq. (19), but fails to notice that our factorization successfully captures all of the terms including the one he ignores.

In conclusion, Mengali's rambling comments[1] have no basis and are devoid of any merit, technical or otherwise.

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Received 3 December 2002; accepted for publication 20 December 2002.

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