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Authors' reply ☆

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1. Introduction

The authors of Ref. [1] would like to acknowledge the consideration Smol'yakov and Tkachenko gave to their comments. As a preliminary, it should be noted that the aim of this publication is to propose a novel, alternative model established on a thorough set of experiments and interpretations, hoping that some members of the research community may find it useful. With this intention, the authors attempted to define a set of fundamental parameters that determine the properties of a fluctuating surface pressure field in a low Mach number turbulent boundary layer. The importance and legitimacy of all previous work is by no means questioned in this paper, which should be seen as a proposed contribution to the advancement of knowledge in its area.

2. Erratum

The authors of Ref. [1] wish to apologise to the readers for their mistake in the legend of their Fig. 14, which, as pointed out by Smol'yakov and Tkachenko, and also in the main text of Ref. [1], represents coherence in the transverse direction. However, the models are not misrepresented in the legends of Figs. 13 and 14. It is possible that the dashed and dotted lines could be confused. To overcome the difficulty to distinguish between these two lines, the main text of the original paper contained a detailed description of the figures, to help the readers in their interpretation.

As pointed out by Smol'yakov and Tkachenko, a significant mistake made in the paper, is the misrepresentation of the convection velocity model proposed by Efimtsov [2], and they propose a correct version of the original paper's Fig. 10 in their Fig. 1. The discrepancy between this model and the measurements is now more significant, which may be due to the difference in the experimental conditions reported in Refs. [1,2], mainly in terms of flow Mach number. However, the point remains that Efimtsov's model for the surface pressure convection velocity does not take into account the influence of spatial separation. The main consequence of the mistake in Fig. 10 of

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the original paper is that Efimtsov's coherence model is underestimated in Figs. 13 and 14 of the same paper, and the correct representation is included in Figs. 1 and 2, respectively, in this reply. A comparison shows that the mistake does not affect the conclusions that were drawn in Ref. [1].

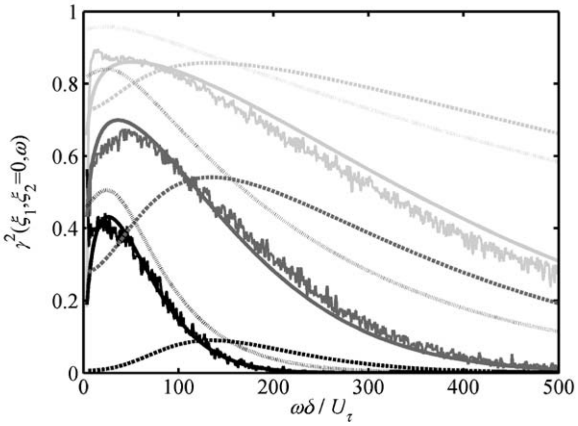

Fig. 1. Coherence measured between two transducers with various longitudinal separations ξ_1 ($\xi_2 = 0$, $U_\infty = 40\,\text{m/s}$), comparison between models and measurements: proposed model: , $\xi_1 = 0.10\,\delta$; , $\xi_1 = 0.42\,\delta$; , $\xi_1 = 1.63\,\delta$. Efimtsov's model: , $\xi_1 = 0.10\,\delta$; , $\xi_1 = 0.42\,\delta$; , $\xi_1 = 1.63\,\delta$. Smol'yakov & Tkachenko's model: , $\xi_1 = 0.10\,\delta$; , $\xi_1 = 0.42\,\delta$; , $\xi_1 = 1.63\,\delta$.

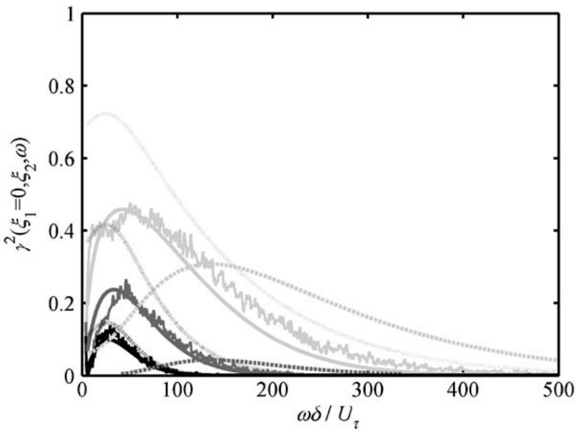

Fig. 2. Coherence measured between two transducers with various transversal separations ξ_2 ($\xi_1 = 0$, $U_\infty = 40\,\text{m/s}$), comparison between models and measurements: proposed model: , $\xi_2 = 0.10\,\delta$; , $\xi_2 = 0.28\,\delta$; , $\xi_2 = 0.59\,\delta$. Efimtsov's model: , $\xi_2 = 0.10\,\delta$; , $\xi_2 = 0.28\,\delta$; , $\xi_2 = 0.59\,\delta$. Smol'yakov & Tkachenko's model: , $\xi_2 = 0.10\,\delta$; , $\xi_2 = 0.28\,\delta$; , $\xi_2 = 0.59\,\delta$.

3. Limitations of the proposed model

In his fundamental publication, Corcos [3] proposes the following model for the surface pressure field cross spectral density S_{pp} as a function of the radian frequency ω, and ξ and η, the streamwise and spanwise separation distances, respectively, as the product of three functions:

$$S_{pp}(\xi, \eta, \omega) = \phi(\omega)\gamma_\xi(\omega\xi/U_c)\gamma_\eta(\omega\eta/U_c)\,\mathrm{e}^{-\omega\xi/U_c},$$

where ϕ is the point surface pressure power spectral density, γ_ξ and γ_η are the coherence functions in the streamwise and spanwise directions respectively, and U_c is the convection velocity, which "is found to be a function of ω, and to be almost independent of ξ and η". This model assumes that the off-axis coherence is the product of the coherence functions in the streamwise and spanwise directions, and that this coherence is a function of $\omega\xi/U_c$ and $\omega\eta/U_c$ only.

It was found in Ref. [1] that the phase velocity varies with both ω and ξ, and that the off-axis coherence is not the product of the streamwise and spanwise coherence functions. Although the findings in Ref. [1] represent fundamental variations from the Corcos' model, they are consistent with other published work referenced in [1]. This non-trivial dependence of the convection velocity on parameters other than ω affects the accuracy of a model established on such normalized variables. The experimental results presented in Ref. [1] consistently show that at the higher end of the investigated frequency range, coherence is a function of the dimensionless number $\sqrt{\nu f^3 \xi_1^2 / U_\tau^2}$, where ν is the kinematic viscosity and U_τ is the friction velocity. Although this representation is again much different to that of Corcos, the experimental results were consistent enough that the authors were confident to propose it for their model.

In an attempt to fit this coherence model within the structure of the Corcos model, Smol'yakov and Tkachenko study the evolution of the equivalent coherence length scale with frequency and streamwise spacing, and show that it grows to infinity with increasing ξ and decreasing ω. The concept of a length scale that is dependent on ξ is somewhat difficult to justify. The model in the original paper [1] does include the important feature that wall pressure remains coherent over a distance that is dependant on the boundary layer thickness and frequency amongst other parameters, and that coherence decreases with ω at low frequencies, in quantitative agreement with experimental observations. Furthermore the surface pressure auto-spectrum in the model of Ref. [1] is proportional to $\omega^{0.2}$ at low frequencies, which implies a very sharp decrease in signal power when ω becomes small.

Another shortcoming of the proposed model is the fact that, as pointed out, it does not have an analytical Fourier transform that would enable it to be easily implemented in the frequency-wavenumber domain. The authors of the original paper agree with this observation, and Fourier transformation can only be carried numerically. However, as written in Ref. [1], this model was derived for structural response computations carried out in the space domain, and the intention to subsequently adapt it to non-uniform surface pressure field [4]. As can be expected, the authors are more comfortable with a physical interpretation of experimental data and subsequent model derivation in the space domain rather than its Fourier equivalent. This affects the ease of application of the model, and not its validity.

4. Conclusion

Based on an extensive set of experimental data, the authors of Ref. [1] investigated the wall-pressure field beneath a turbulent boundary layer at low and medium frequencies, which led to the formulation of a novel model. This model includes variations from previously published models and new forms of variable normalization, which made it worthy of publication. These variations were only proposed when it was felt that they were strongly supported by empirical results obtained from a careful experiment. By no means is the proposed model an attempt to question previously published work: it merely is a proposed alternative that, the authors hope, will raise interest from the readers who work on similar flow configurations.

In spite of the errors in their Figs. 10 and 14, the authors remain confident that the model they propose accurately describes a thorough set of reliable experimental data. They look forward to seeing this model checked against other experimental work carried out in a similar range of flow applications.

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