

Hypersonic Turbulent Quantities in Support of Morkovin's Hypothesis

Corresponding Author: Professor Nick Parziale

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Comments on paper "Hypersonic Turbulent Quantities in Support of Morkovin's Hypothesis" by Segall et al.

This manuscript performs wind tunnel experiments to validate the classical Morkovin's hypothesis using the KTV method. The mean velocity and velocity fluctuation intensities are measured, showing excellent agreement with DNS data, especially the vertical velocity fluctuation, which has never been achieved before. This manuscript is very well written and organized. I can recommend its publication if the following issues can be properly addressed.

(1) The authors adopted the Walz equation to estimate the mean temperature and density. Considering that the wall temperature is smaller than the recovery temperature, there could be significant wall heat transfer. Under such circumstances, the generalized Reynolds analogy given by Zhang et al (2014, J. Fluid Mech.) should be a better candidate. Please compare the differences.

(2) The beam angle θ_{RF} is 14 degrees herein. Although the authors compared several different angles (14, 25 and 40), the convergence seems not to have been achieved. I understand that it can be difficult to lower this angle further, but please at least discuss at what angle the convergence is expected to be achieved.

(3) Recent numerical studies, such as Yu et al (2019, Phys. Rev. Fluids, 4(12):123402), have shown that compressibility effects can be significant near the wall, which cannot be measured by KTV for the time being. Future work should be dedicated to promoting the measuring technique to validate such findings. Please consider incorporating related discussions.

Reviewer #2

(Remarks to the Author)

Morkovin's hypothesis remains a cornerstone of turbulence modeling for compressible flows, providing a crucial bridge to extending incompressible turbulence models to higher Mach numbers. This manuscript offers a valuable albeit preliminary assessment concerning the applicability of Morkovin's hypothesis in the context of a zero pressure gradient boundary layer at Mach 6 and $T_w/T_r = 0.54$. The reported measurements provide useful comparisons to computed profiles based on direct numerical simulations (DNS), establishing a valuable benchmark for future experimental studies. The manuscript's most significant contribution is the demonstration of more accurate measurements of wall-normal velocity fluctuations (v') in a hypersonic boundary layer relative to prior measurements based on particle image velocimetry (PIV), which is known to suffer from the effects of particle lag. Measuring v' in a thin shear layer is a challenging task in general, and therefore, the above accomplishment deserves primary emphasis in the manuscript. While the comparison of the RMS velocity profiles with DNS and the low-speed measurements does provide evidence supporting Morkovin's hypothesis, this finding is somewhat limited in scope. The test conditions are within previously explored parameters, and the measurements correspond to only a single station in a relatively simple flow. I recommend that the authors recalibrate their focus to highlight the methodological achievement in measuring wall-normal velocity fluctuations in hypersonic shear layers, rather than emphasizing broader conclusions about Morkovin's hypothesis.

A more comprehensive study would benefit from measurements at multiple streamwise stations to better characterize the

boundary-layer behavior. Nevertheless, I support eventual publication provided the authors revise their presentation to address the concerns outlined in this review.

Itemized concerns

1. The manuscript's structure deviates from conventional scientific format by presenting results before methods, with uncertainty estimates appearing at the end. This organization disrupts the logical flow expected in a scientific paper. Furthermore, adding a concise summary statement with primary conclusions in the end would enhance the reading experience and help highlight the key takeaways.
2. Regarding the validity of Morkovin's hypothesis in hypersonic flows, the primary scientific question concerns the delineation of its upper limits in the M - T_w/T_r parameter space. Based on existing literature, Morkovin's hypothesis would be expected to hold under the experimental conditions. Consequently, confirming this expectation represents a modest scientific contribution. The authors should consider whether this finding alone warrants rapid publication in *Nature Communications*, or if the manuscript would benefit from incorporating additional analyses that provide deeper physical insights (particularly regarding measurements near the flare region), along with more comprehensive validation of the experimental measurements.
3. I. 119-120: The statement "The tagging location is 0.84 m (32.9 in) or $Rex = 11.5e6$ downstream of the HCF leading edge" raises concerns about experimental methodology. To establish flow recovery from the extended sandpaper strip and to confirm equilibrium state, measurements at multiple axial stations are essential. Furthermore, data from various streamwise positions would significantly strengthen confidence in the accuracy and generalizability of the results presented in Figure 6.
4. I. 125-126: The claim that "Those results match those of DNS, bringing confidence to the understanding of the state of the boundary layer" requires clarification. Additional details must be provided to clearly identify which particular results are being compared between the experimental data and DNS, and what specific metrics were used for this comparison.
5. Fig. 6: The pronounced waviness observed in both u'_{RMS} and v'_{RMS} profiles based on KTV measurements requires explanation. The authors should address whether these oscillations represent actual physical phenomena or potential measurement artifacts.
6. I. 301-302: The assertion that "streamwise fluctuation contamination can be held to acceptable levels with $\theta_{RF} < 15^\circ$ " is inadequately supported by Figure 10, which only includes a single data point below 15 degrees. To substantiate this claim and properly support the error estimation methodology, additional measurements at multiple lower θ_{RF} values should be included in the analysis. Furthermore, what does "streamwise fluctuation contamination" refer to? Nominally, it would be construed as the contamination of streamwise fluctuations; however, Fig. 10 is focused on v'_{RMS} profiles.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have addressed all the issues I have raised in the previous review. I have no further questions.

Reviewer #2

(Remarks to the Author)

I would like to thank the authors for their sincere effort in addressing the concerns raised in my previous report. The inclusion of an additional case with a lower value of T_w/T_r is a welcome addition that has significantly strengthened the manuscript. Overall, I am satisfied with the revisions and would be happy to endorse publication once the following three points are addressed in the final revision. I am happy to leave this task to the authors and do not need to review the revised manuscript.

1. The v'_{RMS} profile for the case of lower surface-temperature ratio shows a continued rise close to the wall, well within the peak location observed in the DNS profile. This discrepancy should be highlighted and a plausible explanation provided.
2. The comparison of FLDI spectra at a single point does not constitute a definitive validation of an equilibrium turbulent boundary layer. This limitation should be clearly communicated to the readers.
3. Additionally, in lines 323-325, the authors state: "DNS predicts that the wall-temperature ratio will shift the peak magnitude and location slightly at wall-normal locations below $y/\delta \approx 0.1$, a feature apparently captured for the $T_w/T_r = 0.17$ case." However, it is unclear which profile this refers to. Please clarify whether this applies to the streamwise normal stress or specify the relevant profile.

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Reviewer #1 (Remarks to the Author):

Comments on paper "Hypersonic Turbulent Quantities in Support of Morkovin's Hypothesis" by Segall et al.

Comment: This manuscript performs wind tunnel experiments to validate the classical Morkovin's hypothesis using the KTV method. The mean velocity and velocity fluctuation intensities are measured, showing excellent agreement with DNS data, especially the vertical velocity fluctuation, which has never been achieved before. This manuscript is very well written and organized. I can recommend its publication if the following issues can be properly addressed.

Response: Thank you for the review. We very much appreciate the effort, and the comments below have strengthened the manuscript.

Comment: (1) The authors adopted the Walz equation to estimate the mean temperature and density. Considering that the wall temperature is smaller than the recovery temperature, there could be significant wall heat transfer. Under such circumstances, the generalized Reynolds analogy given by Zhang et al (2014, J. Fluid Mech.) should be a better candidate. Please compare the differences.

Response: This is a good suggestion. We have now evaluated the mean density profile with the generalized Reynolds analogy from Zhang et al. 2014. This mean density profile feeds into the van Driest transformed mean velocity profile and the Morkovin scaled fluctuations. This could have been important in reducing this data because of heat transfer. The result is that the effect is quite small (less than two percent). We added the reference and noted the small change in highlighted text in the manuscript. We should note that we added $T_w/T_r=0.2$ case, which will have increased heat transfer.

Comment: (2) The beam angle θ_{RF} is 14 degrees herein. Although the authors compared several different angles (14, 25 and 40), the convergence seems not to have been achieved. I understand that it can be difficult to lower this angle further, but please at least discuss at what angle the convergence is expected to be achieved.

Response: Lowering the angle does decrease the max bias error. Please see Figure R1 below. We have included more data points in the figure in the manuscript and expanded the discussion of bias error.

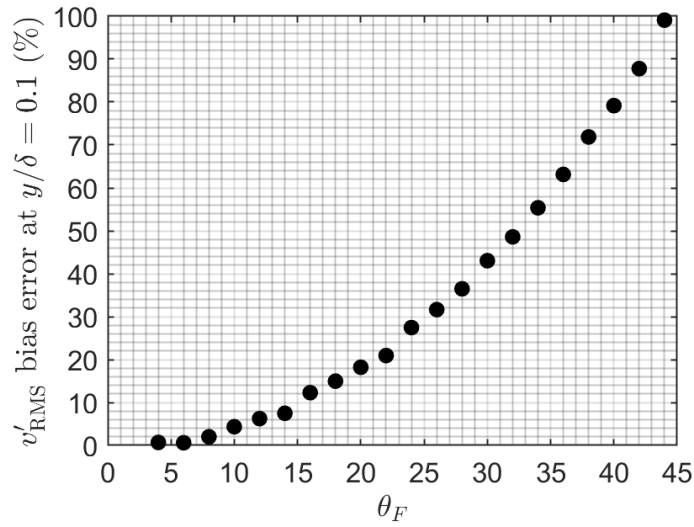


Figure R1. Theta_F vs max bias error showing the convergence of bias error with decreasing theta_F.

Comment: (3) Recent numerical studies, such as Yu et al (2019, Phys. Rev. Fluids, 4(12):123402), have shown that compressibility effects can be significant near the wall, which cannot be measured by KTV for the time being. Future work should be dedicated to promoting the measuring technique to validate such findings. Please consider incorporating related discussions.

Response: This is a very good point. A citation to this work is added in the context of a new future work section in highlighted text at the end of the manuscript.

Reviewer #2 (Remarks to the Author):

Comment: Morkovin's hypothesis remains a cornerstone of turbulence modeling for compressible flows, providing a crucial bridge to extending incompressible turbulence models to higher Mach numbers. This manuscript offers a valuable albeit preliminary assessment concerning the applicability of Morkovin's hypothesis in the context of a zero pressure gradient boundary layer at Mach 6 and $T_w/T_r = 0.54$. The reported measurements provide useful comparisons to computed profiles based on direct numerical simulations (DNS), establishing a valuable benchmark for future experimental studies. The manuscript's most significant contribution is the demonstration of more accurate measurements of wall-normal velocity fluctuations (v') in a hypersonic boundary layer relative to prior measurements based on particle image velocimetry (PIV), which is known to suffer from the effects of particle lag. Measuring v' in a thin shear layer is a challenging task in general, and therefore, the above accomplishment deserves primary emphasis in the manuscript. While the comparison of the RMS velocity profiles with DNS and the low-speed measurements does provide evidence supporting Morkovin's hypothesis, this finding is somewhat limited in scope. The test conditions are within previously explored parameters, and the measurements correspond to only a single station in a relatively simple flow. I recommend that the authors recalibrate their focus to highlight the methodological achievement in measuring wall-normal velocity fluctuations in hypersonic shear layers, rather than emphasizing broader conclusions about Morkovin's hypothesis.

Response: Thank you for this thorough review. We believe we have addressed the comments and that this exercise has greatly strengthened the manuscript. We have brought details on the v' measurement approach into the main body of the text. However, we politely refute the assertion that “test conditions are within previously explored parameters” as these are the first successful v' experimental measurements in a hypersonic turbulent boundary layer presented with error bars, albeit in a relatively simple flow at a single station. We concede that this parameter space has been explored by DNS.

Comment: A more comprehensive study would benefit from measurements at multiple streamwise stations to better characterize the boundary-layer behavior. Nevertheless, I support eventual publication provided the authors revise their presentation to address the concerns outlined in this review.

Response: We have included new data at cold-wall conditions at higher Reynolds number and heavily revised the manuscript's organization; we believe that this has improved the manuscript, so thank you very much for the time in reviewing.

Itemized concerns

Comment 1. The manuscript's structure deviates from conventional scientific format by presenting results before methods, with uncertainty estimates appearing at the end. This organization disrupts the logical flow expected in a scientific paper. Furthermore, adding a concise summary statement

with primary conclusions in the end would enhance the reading experience and help highlight the key takeaways.

Response: We were originally trying to adhere to Nature Communications style by having a more extensive Methods section, but the reviewer makes the convincing argument that the “Data Reduction and Uncertainty Estimate” section should be moved into the body of the paper. This is marked in yellow. Moreover, a Conclusions and Future Work section is added as is expected in a typical fluids paper. (The senior author had contorted the format of this submission into the form first submitted after having looked at other Nature Communications papers; an error in hindsight.)

Comment 2. Regarding the validity of Morkovin's hypothesis in hypersonic flows, the primary scientific question concerns the delineation of its upper limits in the M - T_w/T_r parameter space. Based on existing literature, Morkovin's hypothesis would be expected to hold under the experimental conditions. Consequently, confirming this expectation represents a modest scientific contribution. The authors should consider whether this finding alone warrants rapid publication in Nature Communications, or if the manuscript would benefit from incorporating additional analyses that provide deeper physical insights (particularly regarding measurements near the flare region), along with more comprehensive validation of the experimental measurements.

Response: To address the concern of adding to the literature, we have now included an additional run condition to add to M - T_w/T_r space. The additional condition is an enthalpy-matched Mach 6 condition we generate with a helium driver. The result is a higher Reynolds number and colder wall, namely $Re_{\tau} = 780$, $T_w/T_r = 0.17$; this, in addition to the original $Re_{\tau} = 450$, $T_w/T_r = 0.54$ condition. See Table 1 below. We believe the colder wall and higher Reynolds number case is an even better test of Morkovin's hypothesis. Moreover, we've added an analysis of the error in the measurement technique. Measurements in the flare region and a thorough investigation of the flow physics will be a topic of exciting future work.

Method	P_R (MPa)	T_R (K)	P_{∞} (kPa)	T_{∞} (K)	U_{∞} (m/s)	M_{∞} (-)	Re_{∞}^U (1/m)	u_{τ} (m/s)	δ (mm)	Re_{τ} (-)	Re_{θ} (-)	T_w/T_r (-)
PIV - Williams et al.	9.60	542	1.37	48.3	1046	7.45	4.42×10^6	53.8	10.2	280	9340	0.79
LDA - DeGraaff and Eaton	-	-	-	-	18.95	0.056	1.30×10^6	0.727	35.87	1700	5200	1.00
DNS - Zhang et al.	0.93	433	0.68	55.0	870	5.86	9.95×10^6	45.1	23.8	453	9455	0.76
DNS - Zhang et al.	3.40	693	0.39	51.8	1155	7.87	8.16×10^6	54.3	35.2	480	9714	0.48
KTV - (N_2 Driver)	2.28	606	1.13	65.7	951	6.35	14.0×10^6	47.9	10.5	450	8300	0.54
KTV - (He Driver)	2.43	1698	1.41	223.8	1740	5.97	2.89×10^6	49.5	15.0	780	2047	0.17

Comment 3. l. 119-120: The statement "The tagging location is 0.84 m (32.9 in) or $Rex = 11.5e6$ downstream of the HCF leading edge" raises concerns about experimental methodology. To establish flow recovery from the extended sandpaper strip and to confirm equilibrium state, measurements at multiple axial stations are essential. Furthermore, data from various streamwise positions would significantly strengthen confidence in the accuracy and generalizability of the results presented in Figure 6.

Response: Different streamwise measurement locations are not currently possible due to the setup. However, we report data from an additional case, namely $Re\tau_w = 780$, $T_w/Tr=0.17$; this, in addition to the original $Re\tau_w = 450$, $T_w/Tr=0.54$ condition. The transformed mean velocity profiles compare favorably in the van Driest transformation. Additionally, we now include FLDI measurements to support the assertion that the boundary layer is fully turbulent, as discussed in the next comment.

Comment 4. l. 125-126: The claim that "Those results match those of DNS, bringing confidence to the understanding of the state of the boundary layer" requires clarification. Additional details must be provided to clearly identify which particular results are being compared between the experimental data and DNS, and what specific metrics were used for this comparison.

Response: We now include the DNS and FLDI spectral comparison in the methods section. The collapse between the $T_w/Tr=0.17$ and $T_w/Tr=0.54$ experimental data sets and the $T_w/Tr=0.76$ DNS data set, when shown in the inner scaling, is quite good both qualitatively (comparing shape) and quantitatively (Pearson correlation coefficient). Further analysis will be an exciting topic for future work.

Comment 5. Fig. 6: The pronounced waviness observed in both u'_{RMS} and v'_{RMS} profiles based on KTV measurements requires explanation. The authors should address whether these oscillations represent actual physical phenomena or potential measurement artifacts.

Response: We address the waviness in the manuscript with highlighted text. It is an artifact of the measurement technique that falls within the error bars.

Comment 6. l. 301-302: The assertion that "streamwise fluctuation contamination can be held to acceptable levels with $\theta_{RF} < 15^\circ$ " is inadequately supported by Figure 10, which only includes a single data point below 15 degrees. To substantiate this claim and properly support the error estimation methodology, additional measurements at multiple lower θ_{RF} values should be included in the analysis. Furthermore, what does "streamwise fluctuation contamination" refer to? Nominally, it would be construed as the contamination of streamwise fluctuations; however, Fig. 10 is focused on v'_{RMS} profiles.

Response: "Streamwise fluctuation contamination" of the wall-normal fluctuation data refers to the contamination of the wall-normal fluctuation measurement by the streamwise mean and fluctuation velocity due to the non-zero dot product of the streamwise velocity vector and the unit normal of the wall, $U \cdot \hat{n} = (\bar{u} + u') \cdot \hat{n} \neq 0$. This section of the manuscript is intensely revised for clarity. In fact, we make the addition of an analytical expression for bias error and compare that to the Monte Carlo (MC) generated pseudo data that was in the original submission. The analytical expression and the MC data match nearly identically, bringing confidence to both. Additionally, we show that for more shallow angles, the error converges to near zero by adding a $\theta_F=5$ degrees data point.

September 19, 2025

Ref: NCOMMS-25-43487A

Nature Communications: Hypersonic Turbulent Quantities in Support of Morkovin's Hypothesis

Thank you for all your efforts in reviewing our submission. In summary, we have

- Addressed the second reviewer's comments in highlighted text.
- Corrected a few grammatical typos in non-highlighted text.
- Added some acknowledgments in non-highlighted text.

Reviewer #2 (Remarks to the Author):

Comment: I would like to thank the authors for their sincere effort in addressing the concerns raised in my previous report. The inclusion of an additional case with a lower value of T_w/T_r is a welcome addition that has significantly strengthened the manuscript. Overall, I am satisfied with the revisions and would be happy to endorse publication once the following three points are addressed in the final revision. I am happy to leave this task to the authors and do not need to review the revised manuscript.

Response: We thank the reviewers/editor for their time. The paper is stronger because of their effort.

Comment 1. The v'_{RMS} profile for the case of lower surface-temperature ratio shows a continued rise close to the wall, well within the peak location observed in the DNS profile. This discrepancy should be highlighted and a plausible explanation provided.

Response: Text is highlighted to this end in the comparison section.

Comment 2. The comparison of FLDI spectra at a single point does not constitute a definitive validation of an equilibrium turbulent boundary layer. This limitation should be clearly communicated to the readers.

Response: This is made clear in the manuscript in highlighted text.

Comment 3. Additionally, in lines 323-325, the authors state: "DNS predicts that the wall-temperature ratio will shift the peak magnitude and location slightly at wall-normal locations below $y/\delta \approx 0.1$, a feature apparently captured for the $TW/TR = 0.17$ case." However, it is unclear which profile this refers to. Please clarify whether this applies to the streamwise normal stress or specify the relevant profile.

Response: This was in the u' data, and highlighted text has been added.