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Dear Ms. Spreen:

The following contains my comments regarding the rebuttal to my IDOT Hearing Presentation of June 24, 2011 that addressed the Horn et al. report.

The opening remarks of the Horn et al. rebuttal acknowledge the complex nature of the urban wind environment. This of course is a point I have repeatably made since the opening of the question of the safety of the proposed Children's Memorial Heliport. They suggest that analysts can disagree about the suitability of specific assumptions, and that such disagreements are normally dealt with in an academic environment that involves peer reviewed manuscripts, symposia, etc. Normally I would agree. However as stated in their objectives, the Horn et al. report "sought to identify any wind conditions that create unusual or potentially unsafe flight conditions for helicopters operating from the heliport". Furthermore, their conclusions purported to define "safe flying limits" on which IDOT intends to rest their judgement on the certification of the heliport.

There is in fact no certitude to the simplified methods used by Horn et al. They did not even perform the minimum level of error analysis that is required by best practices, and defined by professional engineering societies, to determine the uncertainty in their "safe flying limits". If this were an academic exercise, the result would be debated in an academic setting. However, this is not an academic exercise. Their report is being used by IDOT and Children's Memorial Hospital to declare the heliport as safe. Lives depend on this conclusion. Thus this goes well beyond academic exercise. Given the haste to which IDOT and Children's Memorial Hospital wish to accept the Horn et al. conclusion, it is my ethical duty to point out the flaws in their analysis and the uncertainty in their conclusion without the normally long academic scrutiny that such a conclusion deserves.

In their rebuttal, Horn et al. suggest that the modeling approaches for the complex urban wind field are beyond the state-of-the-art. However in fact, the "Large Eddy Simulations" approach I suggested has been used for more than a decade for atmospheric simulations^{1,2,3}, in the simulations of ship airwakes⁴, and in a potentially more complex problem of the trans-

¹Moeng, C.H., "A Large-Eddy Simulation Model for the Study of Planetary Boundary-Layer turbulence". J. Atmos. Sci. 41 (13), 1984.

²Moeng, C.H. and Sullivan, P.P., "Large Eddy Simulation". Encyclopedia of Atmospheric Sciences, 2002.

³Stoll, R. and Porte-Agel, F., "Large-Eddy Simulation of the Stable Atmospheric Boundary Layer using Dynamic Models with Different Averaging Schemes". Boundary-Layer Meteorology, 126, 2008.

⁴Polsky, AIAA-2002-1022, 2002

port of (hazardous) scalar contaminants^{5 67}. The cited publications represent a small fraction of those that apply the more physically based LES method to the simulation of an urban wind environment. These myriad of researchers have successfully utilized and validated this approach, which is justification that rather being beyond the state-of-the-art, is in fact the accepted norm.

Horn et al. also suggest in their rebuttal that my critique involved the “application of unrepresentative and inappropriate metrics for assessing errors in the analysis.” In fact I simply applied the principles of Verification and Validation that are the precepts of accessing computational simulations of complex systems. These precepts are documented in policy statements and guidelines given by the American Institute for Aeronautics and Astronautics (AIAA), and the American Society of Mechanical Engineers (ASME)⁸⁹ These guidelines address the metrics for assessment of iterative convergence, spatial grid convergence, temporal convergence, comparisons of the computational fluid dynamics results to experimental data, uncertainty analysis, and assessment of computer programs to check for coding errors. Horn et al. failed at all levels to address these metrics. They furthermore made no statement on the accuracy or uncertainty of their conclusions. This makes it impossible to judge the soundness of their conclusions. Since their conclusions are being used as testament to the flight operation safety of proposed heliport, the complete lack of a professional uncertainty assessment is egregious.

In their rebuttal, Horn et al. re-stated their objective from the original that appeared in their report (which I previously cited) to the following, “to perform research on the wind effects to support a preliminary estimate of operational risks associated with flying to the helipad along the flight paths”. The new key phrases are “preliminary estimate” and “along the flight paths”. This has the tone of a retraction of their original stated objectives, and conclusions. *If this was intended to be a preliminary estimate, then it certainly does not qualify to be the resting statement on the safety of the heliport.*

Following this re-stated objective, the Horn et al. rebuttal acknowledge that the specific criticisms regarding the predicted flow “are valid in the sense that the flow does not perfectly capture measured wind fields.” They again mollify this as being “beyond the state-of-the-art of helicopter/wind analysis tools currently available.” This is justifiably incorrect as evidenced simply by the citation in the Horn et al. report to Polsky, 2002, who utilized LES in helicopter/wind analysis of ship airwakes.

Carrying this point further, Horn et al. state in their rebuttal that given their recognition of the “inevitable limits of the computational models used”, they pursued flight testing

⁵Boris, et al. “Fast and Accurate Prediction of Windborne Contaminant Plumes for Civil Defense in Cities”, 5th International Symposium on Computational Wind Engineering, Chapel Hill, NC, 2010

⁶Patnaik et al. “Validation of An LES Urban Aerodynamics Model With Model and Application Specific Wind Tunnel Data”, The Seventh Asia-Pacific Conference on Wind Engineering, November 8-12, 2009, Taipei, Taiwan

⁷Grinstein, et al. “LES based Urban Dispersal Predictions for Consequence Management”, Los Alamos National Laboratory Report, LA-UR-08-5009, 2008.

⁸AIAA, Guide for the Verification and Validation of Computational Fluid Dynamics Simulations”, AIAA-G-077-1998, 1998.

⁹ASME Editorial Board, “Journal of Heat Transfer Editorial Policy Statement on Numerical Accuracy”, ASME J. of Heat Transfer, 116, Nov. 1994.

to obtain data to validate their approach and conclusions “to the extent possible”. They suggest that it is “not unprecedented to conduct simulation studies without quantitative validation of the simulations model”. As support of this approach, they cite the UK off-shore oil platform study that led to the 4.6kt vertical wind criteria. Certainly this one example does not justify this practice, and more importantly, *it is not the supported position of the largest and most respected Aerospace Engineering professional society (AIAA)*.

The Horn et al. rebuttal has some specific response to a number of points that were made in my hearing presentation. The first focuses on the comparison made between their Streeterville wind simulation along one of the flight paths that was examined in the RWDI wind tunnel experiments. They offer a number of “possible interpretations” that could account for the large differences in the predicted and measured velocities. They suggest that there might be a displacement in the wind tunnel points, or similar qualitative behavior that is “displaced in time”. They suggest that the comparison points were selected to convey wild discrepancies between the simulation and the wind tunnel experiment.

The fact is that Horn et al. had a year to develop their report. This was ample time to perform a quantitative validation study of their Streeterville wind simulation against the RWDI wind tunnel data. This validation was never performed. The SOAR experts had three weeks to perform its validation analysis of the Horn et al results. Given more time, I am confident that an even more expansive analysis would reveal further simulation discrepancies. Horn et al. do not dispute that such discrepancies exist, or that they are large. However in seeking “explanations” for these discrepancies, they *completely disregard any possibility that their unverified, highly simplified flow simulation may be in serious error*.

With regard to Verification and Validation, the Horn et al. rebuttal states that “although the details are not provided, in our report, the flight dynamics model was exercised extensively to verify that the model was reasonable.” When Horn was directly questioned on the Verification of the flight simulation model, he stated that “there was no specific uncertainty and sensitivity analysis of the flight dynamics model beyond what was presented in the report.” Thus Horn et al. now contradict their original position.

The Horn et al. rebuttal lists three “verification” tests that include ensuring consistent trends with conventional helicopters. However based on their description, this is not verification but instead it is validation. “Validation” is an assessment of the accuracy of a computer simulation in representing the real-world physics to which it is intended. “Verification” is a process to address whether or not a computer code has been written correctly and without errors.

They follow this by stating that the “validation” study, which they previously referred to as a “verification” study was consistent with what is often seen in articles and reports in the field of flight simulation. Of this I have no doubt. However, validation studies lead to an uncertainty or error measure that is reflected in *error bars*. What is the Horn et al. assessment of the error? Since there was no statement of the error, or error bars anywhere in the report, we are to presume that the error is zero!

The Horn et al. rebuttal has made numerous references to their approach being standard practice in the helicopter flight simulation community. However in the Information for Authors in the Journal of the Helicopter Society it states that among the criteria for

publishing, a “validity of measured or computed data including sources of uncertainty” must be included. The Horn et al. report and rebuttal, do not meet that standard.

The Horn et al. rebuttal indicates that such “real world effects” as dynamic stall transonic flow, aero-elastic blade models, and advanced wake models are “impractical” for their study and “not a standard practice in the field of rotorcraft flight simulation.” They state that they chose to develop the *simplest* “Level 1” helicopter model which they felt was sufficient for other than “aggressive maneuvering.”

The fundamental question is if the *simplest* model used by Horn et al. is sufficient to capture all of the flight effects in the highly turbulent and variable conditions in the Streeterville community. How can it be proved? What is the level of confidence?

The Horn et al. rebuttal goes on to discuss each of the rotor aerodynamics omissions brought out in my presentation. They neglect transonic and dynamic stall effects as only important in high-speed or high load factor flight that they claim is not important in approach and hover maneuvers. How about in an aborted landing? How about during an engine failure? The modeling approach of the Horn et al. *never considers worst-case scenarios* that are essential in making conservative estimates.

They acknowledge the limitations of the Pitt-Peters model. They neglect the effect of aero-elastic rotor blades because it would require a detailed structural model. They neglect blade vortex interaction models because they feel it is beyond the current state of the art. However this is not the opinion of others in the field, for example Brown and Houston, at Imperial College, London¹⁰. Brown and Houston elevate the discussion by noting that a *good correlation on simplified systems does not necessarily translate automatically to a valid simulation of more complex systems (e.g. flight tests)*. Furthermore it has been often demonstrated with the modeling of complex systems that validity does not even translate between systems with similar complexity *if the physics is missing*.

It was pointed out in my hearing presentation as well in my report, that the Horn et al. flight dynamics simulation assumed that the rotor aerodynamics did not affect the local wind environment. My interest in this aspect of their flight dynamics model was prompted by a citation that appeared in the Horn et al. report to Alpman et al.¹¹, that concluded that that when a helicopter operates close to a solid structure, the induced flow from the helicopter rotor wake can significantly affect the on-coming airwake, and because of this non-linear interaction, a simplified one-way coupling between an airwake simulation and a helicopter/pilot flight simulation *may be seriously in error*.

The aspect is considered important because of the proximity of the raised elevator structure near the helipad. In their rebuttal, Horn et al. suggest that the elevator structure is “relatively small” and “oriented obliquely” to the helipad. They however have no *quantitative* basis to exclude its influence on the rotor aerodynamics near the helipad. In light of the severe warning posed by Alpman et al., this issue needs more than a qualitative judgement. The rebuttal argument that including the more physical two-way coupling is too expensive

¹⁰CFDp-Based Simulation and Experiments in Helicopter Aerodynamics. Integrating CFD and Experiments in Aerodynamics, Glasgow 8-9 September, 2003

¹¹Alpman, E., Long, L., Bridges, L. and Horn, J., “Fully-Coupled Simulations of the Rotorcraft/Ship Dynamic Interace.” American Helicopter Soc. 63rd Ann. Forum, 2007

might be justified in a purely academic study, but not one in which public safety rests in the balance.

The Horn et al. rebuttal acknowledged that velocity fluctuations in turbulent boundary layers, and turbulent wakes immersed in turbulent boundary layers are not random. However, the Control Equivalent Turbulence Model (CETI) that they used assumes that the turbulent velocity fluctuations in these situations is a random process. Thus they acknowledge that CETI is not physical. It is not a compelling argument to justify the use of the CETI by stating that it is the adopted approach of the helicopter community, particularly if it was not validated for the Streeterville wind conditions.

Was the CETI validated in an urban wind environment with thick energetic boundary layers and multiple merging wakes? The purpose of a physics-based Streeterville wind simulation was to remove the need to rely on such ad-hoc models. A failing of the Horn et al. study is that they chose to attempt to validate their flight simulations with wind directions that were not simulated.

The Horn et al. rebuttal finds fault in the method of defining simulation error. The strict definition in evaluating a simulation model is to take the difference between the predicted quantity and the experimentally measured quantity. My approach was completely transparent. I chose to take the difference between the two quantities in order to preserve the sign indicating under or over prediction. The root-mean-square error is also a standard approach. *The central point is that regardless of the approach, Horn et al. made no attempt to quantify errors or uncertainty of the flight simulation predictions.*

The citation to Lusardi, 2004 in the Horn et al. rebuttal describing a methodology is interesting but not conclusive. The Streeterville wind simulation time series was made up of loops of the repeated time series with a random component added. The total length in physical time of these loops did not represent the full spectrum of turbulence time-scales that are an integral part of a well resolved turbulence simulation. Such a spectrum must fundamentally contain an identifiable inertial sub-range and a low frequency range¹² that are not observable in the spectra included in the rebuttal. Although not perfect, the turbulence spectrum in the RWDI experiments was far more representative of the atmospheric boundary layer than the spectrum exhibited in the Horn et al rebuttal.

I had planned to perform a more thorough analysis of the turbulence statistics of the Horn et al simulations. However, the time constraint of having the report for only three weeks prior to the public hearing prevented a thorough critique. There are further metrics that can be applied to the time series including the analysis of higher statistical moments, velocity fluctuation energy ratios, and further spectral analysis that will go further in determining the suitability of the turbulence simulations generated in the study.

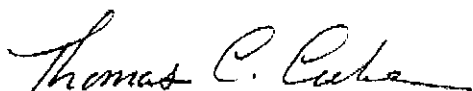
In summary the Horn et al. report applied a wind simulation approach that was not verified to be free of errors, or validated against experiments, and in which they acknowledged its "inevitable limits". As they explained in their rebuttal, this prompted the effort to almost exclusively focus on the use of flight tests. The limited number of flight tests coupled with un-simulated wind conditions caused them to utilize a turbulence substitute in their flight

¹²Tennekes and Lumley, "A First Course in Turbulence", MIT Press, 1972.

simulations. A limited set of flight tests were performed that encompassed a small fraction of the possible wind conditions. This number was too small to statistically remove the effect of random errors. Although they had the means, a quantitative comparison between the flight tests and the flight simulations was not performed. As a result there was no uncertainty analysis presented. Finally they now deem the work to be "preliminary".

Returning to the first argument made in their rebuttal, the Horn et al. study should not be viewed as an academic exercise that would normally be given sufficient time for critical review and debate. This report has been made public for only approximately 5 weeks, and was available for critical review to the SOAR experts before the public hearing for three weeks. Yet even before the public hearing, it was advertised to the Streeterville community by Children's Memorial Hospital to declare the heliport as safe.

As my report and my rebuttal indicate, there are serious deficiencies in the Horn et al. report. This is evident simply in the re-statement of their objectives in the time period following the SOAR critique. The Horn et al. rebuttal suggests that the level of effort that we recommended to address the question of safety of the heliport is either impossible, will take too much time, or is too expensive. I strongly disagree. Lives depend on reaching the correct conclusion.



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