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Status, Emerging Ideas and Future Directions of Turbulence Modeling Research in Aeronautics

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Abstract

In July 2017, a three-day Turbulence Modeling Symposium sponsored by the University of Michigan and NASA was held in Ann Arbor, Michigan. This meeting brought together nearly 90 experts from academia, government and industry, with good international participation, to discuss the state of the art in turbulence modeling, emerging ideas, and to wrestle with questions surrounding its future. Emphasis was placed on turbulence modeling in a predictive context in complex problems, rather than on turbulence theory or descriptive modeling. This report summarizes many of the questions, discussions, and conclusions from the symposium, and suggests immediate next steps.

1 Introduction and overview of the symposium

Turbulence modeling is one of the great challenges in computational fluid dynamics (CFD). Two strong trends appear poised to make modeling increasingly prominent. The first is the ambition to apply CFD over the entire flight envelope, for a wide variety of systems. The motivations for aircraft are cost, schedule, performance, and design risk. The motivations for spacecraft are similar, with the additional factor that some experiments are not possible on the ground. The financial implications are in the billions of dollars, and the safety of astronauts is at stake. In all cases, there is a demand for reliable predictions in more complex flow regimes than those CFD is now trusted for, notably involving flow separation and large-scale unsteadiness.

The second trend is the increase in computing power and quality of CFD solvers and grid generation (especially with automatic adaptation). In the near future, this should make possible the numerical convergence to smooth solutions of the Reynolds-Averaged Navier-Stokes (RANS) equations, even for problems as complex as an airplane in full landing configuration. Note however, that the 2017 High-Lift Prediction Workshop [1] demonstrated that such a convergence has not been achieved yet (with the additional issue of multiple solutions), which adds ambiguity in separating modeling errors from numerical errors. The community engaged in CFD solver work is much larger than the community engaged in RANS research, extending over worldwide academia, governments, and industry, including well-funded vendors of CFD software. This raises the question of overall balance in CFD research.

In today's CFD landscape, RANS is widely used, mostly in a steady mode, although the known weaknesses of this approach for predicting many classes of flows can be very problematic. Some argue that there has been a stagnation in RANS model improvements for some time, possibly because of a lack of new ideas, or possibly because of an *ultimate barrier*, which implies that further and decisive improvement in the more difficult problem areas would not be possible. It is generally believed that continued increase in computer power will eventually make RANS obsolete, but that time is still *many* decades away, and industry needs improved capability at a manageable cost now. Other avenues such as wall-modeled large eddy simulation (LES) and hybrid RANS/LES continue to gain traction, but even these methods may be too expensive for *routine* practical use for at least two decades into the future. These approaches also have their share of failures and controversy.

[1] https://hiliftpw.larc.nasa.gov/, accessed 10/22/2017.

Research in RANS has also been declining over recent decades, and fewer students are being trained. The practice of turbulence model development is also very peculiar, and not easy to transmit. There can be a vicious cycle between stagnation and sponsor fatigue. NASA is now using the Vision 2030 document as a general guideline, which places a timeline on RANS research - specifically involving Reynolds Stress Transport (RST) models - with an implied decision point around the year 2020, and a major workshop specifically addressing this question at NASA Langley in March 2018.

In this atmosphere, it seemed like an appropriate time to bring turbulence modeling experts together to talk about where we are as a community and where we are headed; or perhaps more importantly, where we should be headed. A symposium sponsored by the University of Michigan and NASA was held in Ann Arbor, Michigan in July 2017.

The three-day symposium included 32 participant talks, divided into the following broad areas: new ideas for turbulence modeling, Reynolds stress transport modeling, uncertainty quantification, experiments, data-driven methods, flow solution technologies of the future, LES, direct numerical simulations (DNS), and applications. There were also 6 keynote/plenary invited talks. The full list of talks is provided in the Appendix. The symposium was designed to encourage discussion and to be *speculative*. There was well over 8 hours of discussion time in the symposium that often led to very open/candid exchanges of views. Total attendance was 88, with 53 from academia, 20 from national labs and 15 from industry. There was representation from outside of the United States, including 8 participants from Europe, and representation from experimentalists (6 talks). There was also a blend of providers and users of turbulence modeling. Overall, three goals were accomplished:

- In general, a big positive from the symposium was to get a number of turbulence modeling experts and users in one room together for three days of focused interactions, which reflected much goodwill. Although there was an attempt to define some common goals/focus, this proved to be a challenging task, in part because stake-holders had very diverse needs from CFD. Further refinement will be necessary in future versions of the symposium. However, there is no doubt that the symposium itself may have broken down some barriers and fostered some new alliances. Many subsequent exchanges of information and ideas have taken place, and many participants expressed a desire to continue this type of interaction.

- One key idea of the symposium was to try to get all of the participants to include in their thinking about RANS and to critique the concept of an *ultimate barrier* (at the workshop, the expression "glass ceiling" was used). Here, *ultimate barrier* refers to an unseen and not exactly defined, yet unbreachable barrier. If such a barrier indeed exists, then progress beyond it will be impossible (or at least highly unlikely), despite the ability to "see" the desired goal and even to define it. More details about this concept, and the discussion surrounding it, will be given in Section 2.2.

- An important goal of the symposium was to discuss some of the more recent topics to emerge in the field of turbulence modeling, primarily uncertainty quantification (UQ) and data-driven modeling. The purpose was two-fold: (1) to educate the participants about the basics behind these topics, and (2) to try to place these topics in the context of mainstream turbulence modeling, as it has been practiced since the middle of the

> last century. There was a general consensus among the participants that UQ must be better integrated into our CFD processes, aimed at both the modeling errors and the numerical errors. A significant amount of discussion also took place regarding the potential of data-driven modeling, how it can be used, and what it can and cannot do.

The purpose of the present paper is not to summarize the contents of the various talks. Slides from all talks are available from the symposium website.[2] Rather, the main purpose here is to distill key points, controversies, trends, and ideas, with an overarching goal of attempting to suggest a possible roadmap or recommended path forward.

The talks and focused discussions at the symposium led to many observations, which will be summarized below. There was not always consensus, a fact which we try to capture. The key points were collated by the authors. These points were subsequently shared with the participants and feedback was solicited; an attempt was then made to incorporate the feedback. We make an honest attempt to be fair and even-handed in our assessments, but we welcome continued debate, as much remains unsettled. At the end, we make some recommendations for next steps based in large part on the notes from the symposium. We feel that it is important for the turbulence modeling community to have shared goals in how to engage the turbulence problem.

2 Observations from the symposium

The key observations from the symposium have been categorized into several areas.

2.1 Do we still need RANS-based turbulence models?

Here, we address the needs of the CFD community in Aeronautics. This community consists not only of turbulence model developers and coders, but also regular CFD users throughout industry, academia, and government as well. One message came through very clearly from the participants of the symposium: industry still needs RANS, all the time. Most industrial users are probably decades away from any routine use of scale-resolving simulations, not to mention the cost and time and user skill it would take to run these computations. Therefore, there is still a strong desire for RANS model improvements, not just in the near-term. This message was echoed after the workshop by members of the AIAA Turbulence Modeling Benchmarking Working Group (TMBWG) during a discussion about the symposium.

LES and RANS have a place in different parts of the CFD spectrum and have different uses. Even if one is able to afford a wall-resolved LES over an airplane in 2040 (or wall-modeled LES in 2025), it is going to take some of the world's most powerful computers (involving weeks of run time, and high cost of compute time, electrical power, etc.) and high performance computing (HPC) experts to perform such a computation. This is not to say that LES and DNS are not useful. They have already made their mark in some practical contexts (example: turbomachinery, especially in the context of a single blade passage) and are invaluable for greater insight and discovery. The appeal of LES in being

[2] http://turbgate.engin.umich.edu/symposium/agenda.html, accessed 10/22/2017.

less empirical than RANS is clear. LES will (or may) also play an essential role in RANS-model improvement. However, even if one is able to afford a "hero" LES computation at flight Reynolds numbers in the decades to come, the requirements of CFD in an industrial setting are multifaceted:

- Requirement to execute thousands of parametric explorations and design runs.
- Computations over the full flight envelope. As an example, in a hypersonic vehicle, the ratio of the time-scales of the flight vehicle to that of the 'large turbulent eddies' in the boundary layer may be 10 orders of magnitude or more.
- Additional areas in which CFD (much less RANS) has not even penetrated - this includes conceptual design, trajectory prediction, robust design, etc.

Additionally, given the resources a typical CFD practitioner will have access to, unless the computing cost magically decreases by many orders of magnitude (an unrealistic scenario), moving away from RANS in the foreseeable future is not going to be possible. Even if we eventually do, we are going to rely on near-wall models!

2.2 Expectations and ultimate barrier in RANS modeling

The participants spent some time discussing the concept of an ultimate barrier (using at the time the expression "glass ceiling") in RANS. The primary question is whether such a barrier exists and, if so, at what level. For example, RANS is already considered adequate for many classes of flows, such as fully turbulent attached boundary layers, although even for such flows the expectations of industry have risen (for a clean transonic wing, there would be tremendous value in predicting the parasite drag within 2%, and similar levels would be most desirable for the specific fuel consumption of a jet engine). But RANS is considered less adequate, or even unacceptable, for other classes such as flows with massive separation, assuming such a flow is treated as simply time-averaged. To bound the discussion, consider the flow past a circular cylinder. Simple time-averaging produces a well-defined mean flow field and Reynolds stresses. We contend that very few people in the community expect a RANS model to exist that accurately reproduces this (steady) flow field, the strongest reason for skepticism being the intermittency of the turbulence in some regions due to vortex shedding. In other words, we believe the cylinder viewed as a time-averaged problem is beyond the ultimate barrier. But where is the line? Only at massive separation? Somewhere among the thin shear flows? In vortical flows? Note that there are two valid questions: (1) does this barrier exist, and where is it? and (2) if today's best models have not reached the barrier, then what should be done to approach it, how complex a model is needed, what data is needed, and so on?

The main reason for asking this question is that it might be central to the question of whether it is worthwhile continuing to fund and/or make attempts to improve RANS. If RANS has already hit against an ultimate barrier, then no amount of effort would be fruitful. If the barrier has not been reached but exists, the research community should not promise feats that are beyond it.

There are arguments on both sides of this issue. On the one hand, although turbulence models for RANS have been developed and improved over the course of decades by many great minds, the number of people and years has not been so great as to suggest something akin to Fermi's paradox. In other words, considerably more time and effort may be required before consistent failure implies a high probability of the existence of an ultimate barrier. On the other hand, in some areas logic suggests the presence of an ultimate barrier. For example, although there is certainly a long-time-average turbulent flow behavior that exists behind a circular cylinder, which can be measured and simulated, specific locations can be identified that are alternately inside and then completely outside of the shed turbulent shear layer/vortical structures. It is difficult to envision how physics-based RANS can account for this behavior with a single time-averaged flow field. Note that the ultimate-barrier argument is mainly about the physics (or loss of information) in Reynolds-averaging, rather than about technical limitations (such as the inability to calibrate a model). Not having fully exercised the power of the theoretical foundation of RANS (a foundation which is not clear to all observers), or comprehensively used data, it is not very clear whether an ultimate barrier exists or has been reached. However, it was argued that a RANS model that is universally accurate for all flows of aeronautical interest might be too much to ask for.

Related to the question of an ultimate barrier are the questions: "How good is good enough?" and "Is the achievable accuracy commensurate with industry expectations?" At some level, a given CFD capability and level of accuracy can be considered acceptable for design work or for making decisions. In fact, industry already makes do with RANS (with all its current shortcomings) by knowing which classes of results are trustworthy and which are not, and managing decisions accordingly. Wind-tunnel data create similar issues. Related to these questions is the need for better uncertainty quantification (UQ), to be discussed further in the next section. UQ essentially helps to quantify the management of risk by attempting to associate error bars with CFD solutions. We know how to do this for numerical errors (although the error bounds tend to be somewhat loose), but it is not easy to estimate uncertainties from turbulence modeling. It is also possible for CFD to get the right answer for the wrong reasons. Typically, this might be for an integrated quantity such as lift or drag. Examples were given from the recent High Lift Workshop 3 where C_L near maximum lift could be reasonably predicted by some participants even with wing separation occurring in the wrong location compared to experiment.

Regarding numerical errors in CFD, although theory tells us how to determine accuracy based on grid convergence studies or adjoint-based techniques, there are considerations that undermine efforts in this area. When performing CFD on three-dimensional complex configurations, geometric fidelity is typically compromised. Very rarely are wind tunnel walls or other (possibly unknown or unmeasured) characteristics of wind tunnel tests included in CFD comparison studies, and often geometric features of the model itself are simplified. Another problem faced by many state-of-the-art CFD studies is lack of sufficient iterative convergence for complex 3-D flows. Such lack of convergence can corrupt grid convergence studies by introducing additional unquantifiable errors. Furthermore, there is some evidence that typical grids used today for many complex 3-D problems may be orders of magnitude too coarse, residing well outside of the "asymptotic range" of grid convergence. Here, it is believed that targeted adaptive gridding may be beneficial, but such a capability is still not routine in most of today's CFD processes.

On the experimental side, there was some discussion of accuracy and how this might impact the use of experiments to help "train" turbulence models. For example, the current accuracy of experimental skin friction measurements (when they are provided at all) is believed to be only to about 2-5%. But what is particularly shocking is the revelation that some recent measurements are as much as 5% below the accepted correlations for the zero-pressure-gradient turbulent boundary layer. That correlation has been considered a "gold standard." This issue is new, and may be resolved reasonably rapidly. However, the most common complaint about experiments is the limited number of quantities that can be measured. Also, measurements in the viscous and buffer layers are often out of reach. Thus, an expectation of predicting the drag on a commercial aircraft to within "2 counts" which is better than 1% may be unrealistic.

2.3 Uncertainty quantification

Modeling uncertainties abound in a RANS model and manifest themselves at several levels:

- At the highest level, the mere introduction of the time or ensemble averaging operator introduces uncertainties. For instance, given a macro-state of turbulence characterized by the mean field and Reynolds stresses, an infinite number of realizations of fluctuating velocity fields (or micro-states) may be consistent with the macro-state. As the flow evolves to a new state, different micro-states may give rise to different macro-states. Thus, in some particular flow conditions, the expected chaos of instantaneous turbulence (the micro-state) can lead to hysteresis for the macro-state. This inadequacy is fundamental to RANS itself (due to the loss of information in the Reynolds averaging process).

- At the next lower level, one introduces different model structures (one-point, two-point, PDF-based, etc.) and specific choices within them (such as isotropic dissipation rate) that lead to model inadequacy.

- Further below, crucial but arbitrary empirical decisions in the functional forms (near-wall corrections, rotational correction, source terms, etc.) in a given model structure have to be considered.

- At the lowest level, given a model structure and functional forms, the values assumed by a finite set of coefficients (parameters) always remain uncertain to some extent and obviously a matter of compromise between numerous demands.

Even if, for instance, some industrial applications invoke RANS-based CFD to observe trends or "deltas" in quantities of interest (QoIs) with respect to parametric changes (such as geometry), UQ is still important because uncertainties in the prediction of QoIs may be comparable to the deltas. This will especially be problematic in examining off-design conditions such as stall. As a result, it was easy to reach a consensus that RANS models should (ideally!) be equipped with robust uncertainty estimates.

However, obtaining rigorous bounds for modeling uncertainty is fundamentally challenging, as one has to essentially quantify "unknown-unknowns." Compared to techniques to quantify *errors* due to numerical discretization, model-form uncertainty quantification is in its infancy, and not just in turbulence modeling. Experimental and DNS data can clearly help, but data has to be used and results should be interpreted appropriately. It was pointed out that just because a computation provides results that agree with experimental observations, the prediction is not necessarily validated. This is because the specific quantity of interest may be insensitive to errors. Similarly, inconsistency of computations with measurements does not necessarily mean an invalid prediction.

Bayesian techniques to quantify parametric uncertainty have become well-established in the context of turbulence modeling over the past few years. Inevitably, the output (posterior probabilities) depends to a large degree on the problem set up and the prior assumptions. Assigning priors on a nonphysical quantity (such as a model coefficient) is arbitrary. For instance, just because an expert specifies a range for $C_{\epsilon 1}$ within the k-ϵ model does not mean it is necessarily valid, mainly because the rest of the model is highly uncertain. Uncertainties in the functional form of the model offer a broader view but are again limited by the fact the model structure itself is unavoidably a considerable oversimplification, thus complicating identifiability.

Additionally, obtaining sufficient data that is informative for determining parameters/model inadequacy and relevant to prediction is a challenging task. Calibration data and validation data will also have to satisfy different requirements.

When simulating real world problems, one has to be mindful of the fact that model predictions will be sensitive to a number of factors. Uncertainties due to the inherent loss of information in the Reynolds averaged representation, model structure, and modeling assumptions were discussed above. Additionally, there is numerical error, uncertainties in boundary conditions, and natural variabilities. In a practical scenario, all of these aspects may confound each other.

Recently, the rate of increase in computing power has weakened markedly and computing costs are becoming a concern. Thus, solver and grid improvements are also very relevant. Discretization error must be quantified and minimized for all benchmark simulations, including complex configurations, in order to fairly assess modeling uncertainties.

2.4 Data-driven modeling

With the prevalence of "big-data" and "machine learning-based predictions" in a number of popular applications, it is natural to ask whether one can bypass the traditional ways of intuition/hypothesis-driven model creation and instead use data and a known algorithm to generate turbulence models free from human intuition. The availability of large amounts of high-resolution data from both experiments and DNS/LES certainly appears to encourage this viewpoint. However, the idea of data-driven turbulence modeling has been met with pessimism from the traditional turbulence modeling community until very recently when one could characterize the reaction as cautious optimism. While this perspective may be justified, it was pointed out that turbulence modeling has always involved data (for calibration), inference (intuition/trial and error tests) and rudimentary machine learning (curve-fitting using simple functions). It could thus be natural for turbulence modeling to take advantage of large and diverse data-sets and adopt *formal* methods of inference and learning. Leveraging data-science towards the improvement of turbulence modeling does not constitute a new philosophy. Rather, data-driven modeling brings in a new set of tools that allows for a more formal and comprehensive use of data.

It was discussed that data cannot be an alternative for turbulence modeling, but when combined with, and informed by, a detailed knowledge of the physical problem and problem-specific constraints, a data-driven approach is likely to yield successful solutions. Thus, one should not throw away the existing knowledge-base in turbulence modeling but rather build on top of it. Some foundations of conventional turbulence modeling, notably dimensional analysis and Galilean invariance, must be preserved.

Modern data-driven techniques are capable of finding "beautiful" but hidden exact equations, relations, or truths. This has been demonstrated in simpler settings: for instance, Burgers equation and even the Navier-Stokes equations have been discovered purely from data (in a specific context). However, it can be argued that a closed, universally accurate turbulence model is not waiting to be discovered (because of the information loss in RANS - ultimate barrier concept). Against this backdrop, we may be able to find *optimal models* in a user-defined sense, with prospects of significantly higher accuracy than intuition-based trial and error models, which represent the present state of the art.

Different elements of the data-driven modeling process include the following:

(1) Assembling relevant and informative data-sets,

(2) Performing physical and model inference to account for model discrepancy (this is a critical step to ensure consistency between the data and the model),

(3) Performing machine learning/calibration of model discrepancy in terms of mean flow and turbulence variables,

(4) Embedding machine learned/calibrated function in the predictive solver, and

(5) Executing the predictive solver to obtain outputs (including uncertainty bounds, if sought).

Along the above lines, it was emphasized that the community should not refer to the above process as "machine learning," as it oversimplifies the modeling strategy. Additionally, not all of these actions have to be necessarily pursued in a data-driven process. For instance,

- If the goal is just parameter inference, step (3) will not be required.
- Even if the goal is to discover better functional forms, step (3) may be replaced by a hand-tuned analytical function that matches the target provided by step (2). Seeing the analytical form of the function could be comforting for modelers and can make the model appear less like a "black-box."
- If the model output is very close to the observations (and for the right reasons), step (2) can be skipped and machine learning may be applied directly on the data.

The success of data-driven modeling is highly dependent on the choices made during the process. This includes the data used in the process, priors for inference, features in machine learning, etc. In this sense, the data-driven modeling process is no different from the traditional way of creating a turbulence model. Good scientific principles cannot be ignored.

Additional challenges for both UQ and data-driven modeling are given below:

- *Data*: Obtaining sufficient data that is informative for parameters/model inadequacy and relevant to prediction.
- *Identifiability*: In statistics, a model is considered identifiable if it is theoretically possible to learn the true values of the underlying discrepancy after obtaining an infinite number of observations from it. This is an open question, and again related to the idea of the ultimate barrier.

- *Interpretability*: Even if a data-driven model is able to offer improved predictions, interpretability of the corrections in terms of known modeling constructs will go a long way in improving the utility of the model to the broader community.

The use of formal inference and machine learning in turbulence modeling is still less than five years old, but at least six academic groups around the world are now pursuing elements of the idea. While NASA funded the first such effort, AFRL and DLR are starting new internal efforts on data-driven turbulence modeling. A number of groups have demonstrated promise in a (thus far) very limited class of problems. A broader community effort (and focused development/support at national labs) is required to make more progress.

2.5 Experiments

As mentioned earlier, even for the conceptually (but possibly not experimentally) simplest wall-bounded turbulent flow experiment, the ZPG boundary layer, uncertainty in skin friction measurement may be 2-5%, which is higher than has been believed for many years. This is a big concern. Similarly, the consensus over the constants in the logarithmic law for turbulent wall-bounded flows appears to have been lost, with the bracket for the Karman constant now up to almost 10%.

There was also some concern whether we have placed too much emphasis on measuring canonical flows and forcing models to match them. In spite of this, there are still fundamental issues in matching canonical flows (in particular, near-wall Reynolds-number effects are likely to defeat any conventional RANS model in the prediction of Reynolds stresses and dissipation). What about the impact of roughness, pressure gradient, curvature, 3-D, separation, blowing, etc.? Some of these, including roughness and curvature, have been addressed with corrections designed to be neutral in the canonical flow, but we do not have adequate measurements in the relevant parameter regimes.

There was discussion on what the target quantities of interest should be in an experimental measurement. For eddy-viscosity models, the mean flow and Reynolds stresses are generally sufficient. For Reynolds-stress models, the full budget of the stress tensor would be welcome, but it is extremely rare for all terms to be measured. While this could clearly be desired by modelers, it is not clear whether it can directly enable model improvement. In a predictive model, small imbalances and cancellations between different terms might be more important to the output. Further, models may themselves contain latent/scale-providing variables that are not physical. An obvious example is the eddy viscosity: it cannot be measured. In many cases, the skin friction might be the most desirable quantity but also is the most challenging to measure.

Over the past decade or so, with the insatiable need for CFD validation data, experimentalists have put much-needed emphasis on quantifying boundary conditions (inflow turbulence, back pressure, etc.) more carefully. This has been an evolution. There was discussion on what exactly would give the best return on investment in the future, given the needs of data-driven turbulence modeling. Some symposium participants felt that, given a fixed amount of resources, measuring a few quantities over a large number of configurations might be more valuable than measuring - in great detail - a number of quantities in only a few configurations.

2.6 Philosophy

In the spirit of the conference, a number of philosophical discussions took place:

- If RST models are better in general than simpler models (like SA-RC-QCR), why are so few people using them? RSTs have not yet been able to buy their way into acceptance by proving that they can time-after-time deliver more accurate solutions (regardless of robustness). This does not mean we give up on RST models - RST models are more elegant and theoretically capable of a higher ceiling (with potentially less empiricism than eddy viscosity models), but are harder to calibrate because of a higher degree of model complexity. Perhaps one has to make use of more formal/comprehensive ways of calibration. Very few groups have the resources and patience to carefully calibrate RSTs. A few were represented in this conference. On the issue of improving RSTs, this could be a prime application area for data-driven modeling. There is some disagreement among the participants on whether additional work on RST models is worth the effort or not. Some advocates point out that *existing* RST models may lack the expected accuracy advantage, but as a class, RST models certainly have more potential.

- Global and Zonal models coexist (both for pure RANS and for hybrid RANS-LES methods), and probably will for a long time. In terms of convenience, of course the preference is for global models, but (for example) flow separation and thermal mixing are very different (and they can occur in the same problem!) - we probably cannot expect the same type of model for both.

- We may need to recalibrate RANS models to be more “aggressive,” meaning more prone to separation. Even the SST model is not aggressive enough for the prediction of maximum lift on clean airfoils. But the opposite often seems to be the case in 3-D (for example, CFD often predicts stall too early for 3-D high-lift wings, with too much separated flow predicted). However, grid convergence is not easily demonstrable as of today for such flows, which obscures the findings; in addition, laminar regions in the boundary layers could have an impact. Experiments over 2-D geometries also suffer from side-wall effects, which have become more problematic now that accuracy expectations have risen compared to many decades ago when 2-D experiments were more common.

- A controversy over the well-posedness of Unsteady RANS (URANS) came into the open. The flow past a cylinder is a prime arena for these ideas, and both 2-D and 3-D unsteady solutions are in the literature with mixed results. Some researchers consider that URANS is not well-defined (there is no separation of scales between resolved and modeled turbulence, and there is also no distinction between models aimed at Steady RANS (SRANS) and those aimed at URANS). In other flows with milder separation, a model may go unsteady when run in URANS mode, or it may not. There is really no clear averaging/filtering operator that the URANS would be the approximation of (similar issues exist with Detached Eddy Simulation). Other researchers pointed out that a model has a scale defined implicitly by its prediction of k and ϵ, and advocated ensemble averaging over time averaging, which addresses the averaging-operator issue. An animated discussion of ensemble averaging took place between participants.

3 What can the community do?

The needs are clear, but how do we achieve substantial improvements in RANS modeling? Stating that we want something does no good if we are already hitting an ultimate barrier, or if there is not a sufficient amount of focused, funded work directed toward specific goals. Existing RANS models are certainly useful, albeit very insufficient for many situations. The current RANS toolkit exists, and will not go away any time soon. So, is it best to simply live with the existing toolkit, or to put money and effort into continuing to try to improve the tools?

- Among participants at the symposium, the answer to this varied. For some participants, there is a tendency to lean toward a belief that there is no ultimate barrier (or that the barrier is very high and we have not reached it yet), and we merely need to keep chipping away at the gaps in knowledge. Among those that feel this way, some feel this would be most appropriate at the RST level while others believe that significant progress can be made in RANS even at the one- or two-equation level.
- Another opinion is that modeling accuracy may never improve by much, and so effort should be dedicated to quantifying uncertainties in models.

Model improvements: Regarding RST models, at this time there is agreement that current models are not always more accurate than simpler models. The community has recently made some effort to systematically evaluate RSTs; for example, they are now in NASA codes. Their inability to predict flows systematically better than widely-used one- or two-equation models may be because of an ultimate barrier, or because of a dearth of new ideas, or because of the sheer magnitude of the intellectual task of mastering so many terms in the RST equations. Because we do not know the reason for their inability to significantly surpass the simpler models, it is not straightforward to recommend a specific path forward.

One of the main ideas to emerge is the following: because the number of RANS turbulence modeling efforts has considerably diminished in recent years, we need better coordinated efforts going forward. This is sometimes hampered by the interests of the aircraft and defense industries as well as those of the CFD vendors, who all may have an incentive to keep the best parts of their models secret. From the point of view of the experts in data-driven modeling, the tools have been developed and the methodology is also somewhat clear; focus should be on more comprehensive (broader) data sets and good choices (such as what invariants to use). This symposium may have helped to bring together the right people to initiate such an effort. Even in terms of pure modeling (for example, developing a better RST model), multiple groups working together in an "open-source" style probably have a better chance of success than one group working alone.

Recent data-driven modeling trials indicate that improvements to RANS are possible, but these improvements have not (yet) been demonstrated to be very generalizable. If data-driven modeling is going to be a common focus of the turbulence modeling community in the future, then benchmark datasets and results are needed to verify/assess data-driven modeling approaches. This should be a short-term goal of this community.

Experiments: A better coordination of efforts also includes increased interactions between model developers and experimentalists. At the symposium, the experimental representatives asked many questions of the group. Two examples include: (1) what are the best 3-D experiments for validation of 3-D separated flow? and (2) what modeling terms have the potential for returning the most value (i.e., what should the experimental community focus on measuring)? There was no clear answer to the first question. Some existing popular validation tests from many decades ago, such as the ONERA M6 wing, have inadequately measured boundary conditions and contain only limited data (e.g., surface pressures). An answer to the second question was given in the section on Experiments. For example, the full budget of the stress tensor could be useful, but it may be that differences between terms are even more important and many of the terms themselves cannot be easily measured. One of the advantages of DNS is that it can provide all budget terms very accurately, but usually only for very simple configurations. In any case, there is no doubt that more coordination and better communication between model developers and experimentalists can only help matters.

Another goal identified by the symposium participants is the desperate need for an up-to-date, vetted, evolving catalog of peer-reviewed experiments, rated for their usefulness and completeness. This catalog would need to be constantly tracked and updated with experiments that use newer technology. ERCOFTAC already has a catalog of experiments available on-line, but it is apparently mostly static.

Numerics: Aside from turbulence modeling itself, the question arose as to whether current community codes are well positioned to accelerate turbulence research toward HPC implementations. There is a strong belief (discussed earlier) that current gridding practices fall far short of what is needed to obtain (for example) a grid-converged 3-D high-lift wing case. From recent studies, even 2-D airfoils can take millions of grid points for ultimate convergence especially at the trailing edge, with blind isotropic grid refinement. This can of course be alleviated to a degree by goal-oriented anisotropic grid adaptation, but such practices still have not entered the mainstream. On a 3-D wing, even on so-called ultra-fine grids running into many hundreds of millions of mesh points (again, not necessarily optimized), numerical error is still 6 counts of drag or more. This does not include the modeling error, but is already larger than the desired *total accuracy*. Efficiency improvements and HPC may be able to help the community move toward grid converged solutions. The community also needs to accelerate the integration of automatic anisotropic grid adaption into standard day-to-day practice.

4 Immediate Next Steps

The discussion in the previous section points to a number of activities over different time-frames. As action items, based on the notes from the symposium, the authors recommend that the following steps be taken in the near future:

- Devise a recommended turbulence modeling research roadmap that ties into Vision 2030, but includes more details directly related to RANS turbulence modeling. For example, how can we better use data-driven modeling and UQ as a community?

- Decide on a common site for a DNS/LES dataset repository that would be useful for RANS (the TMR website[3] has already initiated this to a small degree; decide if this is the most appropriate location). This should be coordinated with the University of Michigan's Turbulence Modeling Gateway[4] and the University of Maryland Wall-Modeled Large Eddy Simulation Resource.[5]

- Determine a method for better cataloging and continuous tracking of existing and ongoing experimental datasets. Identify possible future experiments that have the most potential.

- Establish benchmark problem(s) and practices to evaluate/develop data driven turbulence models.

- Plan follow-up symposia, or link to other related workshop(s) engaging a broader community (Department of Defense, industrial stakeholders, etc).

- The AIAA Vision 2030 integration committee (IC) is being formed - it should look at needs/opportunities. RANS is still critical - the community needs to maintain balance in its research portfolio, and not go completely over to LES-related research.

[3]https://turbmodels.larc.nasa.gov/ , accessed 10/31/2017.
[4]http://turbgate.engin.umich.edu/, accessed 10/31/2017.
[5]http://wmles.umd.edu/, accessed 10/31/2017.

APPENDIX - List of Talks from the University of Michigan / NASA Symposium on Advances in Turbulence Modeling

- Keynote: Status of Industrial Turbulence Modeling (Florian Menter, Ansys)
- *Session A - New Ideas for Turbulence Modeling*
  - A Structure-Based Model for the Transport of Scalars in Homogeneous Turbulent Flows (Constantinos Panagiotou, U. Tokyo)
  - Reynolds Stress Closure for Non-Equilibrium Effects in Turbulent Flows (Peter Hamlington, U. Colorado)
  - One-Point PDF Closure Model Applied to Attached and Separated Flows (Michael Stoellinger, U. Wyoming)
- *Session B - Reynolds Stress Transport Modeling*
  - Plenary: Reynolds Stress Modeling of Turbulence (Suad Jakirlic, TU Darmstadt)
  - Perspective on Turbulence Modeling Using Reynolds Stress Models : General Approach (Bernhard Eisfeld, DLR)
  - Perspective on Turbulence Modeling Using Reynolds Stress Models : Modication for Pressure Gradients (Tobias Knopp, DLR)
  - Initial Efforts to Improve Reynolds Stress Model Predictions for Separated Flows (Chris Rumsey, NASA Langley)
- *Session C - RANS or Hybrid Development*
  - Development of a One-Equation Eddy Viscosity Turbulence Model for Application to Complex Turbulent Flows (Ramesh Agarwal, Washington Univ)
  - RANS Model Development at LLNL for the Prediction of Turbulent Mixing (Brandon Morgan, LLNL)
  - A Framework for Multicomponent, Reynolds-Averaged Navier Stokes Modeling of Hydrodynamic Instability-Induced Turbulent Mixing (Oleg Schilling, LLNL)
  - Influence of a Quadratic Constitutive Relation on Detached Eddy Simulations (Jim Coder, U Tennessee)
- Plenary : Uncertainty Quantification in Turbulence Modeling (Robert Moser, U. Texas)
- *Session D - Experiments*
  - Plenary: Experiments to Aid Understanding and Modeling of Turbulence (Alexander Smits, Princeton)
  - Quantitative Characterization of Pressure-Related Turbulence Transport Terms Using Simultaneous Nonintrusive Pressure and Velocity Measurement (Xiaofeng Liu, SDSU)

- Modern CFD Validation for Turbulent Flow Separation on Axisymmetric Afterbodies (Kevin Disotell, NASA Langley)
- Development of a Benchmark Problem for Modeling Transitional Unsteady Flows: a Combined Experimental/Computational Approach (Todd Lowe, Virginia Tech)

- *Session E - Data-Driven Methods*
  - Plenary: Data-Driven Turbulence Modeling: Challenges and Progress (Karthik Duraisamy, U. Michigan)
  - Field Inversion and Machine Learning for Predictive Turbulence Modeling (Anand Pratap Singh, U. Michigan)
  - A Machine Learning Approach for Turbulent Scalar Mixing with Applications in Film Cooling (Pedro Milani, Stanford)
  - A Physics-Based Machine Learning Approach for Predictive Turbulence Modeling (Heng Xiao, Virginia Tech)
  - Data-Driven Turbulence Modeling Applied to Separated Flows (Nicolo Fabbiane, ONERA)

- *Session F - Flow Solution Technologies for the Future*
  - Plenary: Turbulent Flow Solvers Perspectives on HPC and Numerical Methods (Juan Alonso, Stanford)
  - Resolution Requirements for DG-LES (Shervin Sammak, U. Pittsburgh)
  - A Self-Contained Filtered Density Function (Arash Nouri, U. Pittsburgh)

- *Session G - DNS/LES & Applications*
  - DNS / LES of Turbulent Separated Flows (Ponnampalam Balakumar, NASA Langley)
  - Novel Uses of DNS with Turbulent Separation for RANS Models (Gary Coleman, NASA Langley)
  - Challenges for RANS Models in Turbomachinery Flows (Gorazd Medic, UTRC)
  - Appropriate Differential Reynolds Stress Modeling for Turbomachinery Flows (Christian Morsbach, DLR)

- RCA Workshop / Vision 2030 Discussion (Chris Rumsey & Mujeeb Malik)

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14. ABSTRACT
In July 2017, a three-day Turbulence Modeling Symposium sponsored by the University of Michigan and NASA was held in Ann Arbor, Michigan. This meeting brought together nearly 90 experts from academia, government and industry, with good international participation, to discuss the state of the art in turbulence modeling, emerging ideas, and to wrestle with questions surrounding its future. Emphasis was placed on turbulence modeling in a predictive context in complex problems, rather than on turbulence theory or descriptive modeling. This report summarizes many of the questions, discussions, and conclusions from the symposium, and suggests immediate next steps.

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