Engineering Turbulence Modelling and Experiments - 4
W. Rodi and D. Laurence (Editors)

Strategies for turbulence modelling and simulations

P. R. Spalart[a]

[a]Boeing Commercial Airplanes, P.O. Box 3707, Seattle, WA 98124, USA

This is an attempt to clarify the many levels possible for the numerical prediction of a turbulent flow, the target being a complete airplane, gas turbine, or car. These levels still range from a solution of the steady Reynolds-Averaged Navier-Stokes equations to a Direct Numerical Simulation, with Large-Eddy Simulation in-between. However recent years have added intermediate concepts, dubbed "VLES", "URANS" and "DES". They are in experimental use and, although more expensive, threaten complex traditional models especially for bluff-body flows, where three-dimensional simulations in two-dimensional geometries are flourishing. Turbulence predictions face two principal challenges: (I) predicting growth and separation of the boundary layer, and (II) providing accurate Reynolds stresses after separation. (I) is simpler, but makes much higher accuracy demands, and appears to give models of higher complexity almost no advantage. (II) is the arena for complex RANS models and the newer strategies. With some strategies, grid refinement is aimed at numerical accuracy; in others it is aimed at richer turbulence physics. In some approaches the empirical constants play a strong role even when the grid is very fine; in others, their role vanishes. For several decades, practical methods will necessarily be hybrid, and their empirical content will remain substantial. The law of the wall will be particularly resistant. Estimates are offered of the grid resolution needed for the application of each strategy to full-blown aerodynamic calculations, feeding into rough estimates of the feasibility date, based on computing-power growth.

1. INTRODUCTION

The turbulence problem is of course far from solved, whether in terms of mathematical and intuitive understanding, or in terms of obtaining engineering accuracy in machines that depend on viscous fluid dynamics. Technological fields of global importance such as the airliner and automobile industries revolve around such devices. This economic stake motivates relentless, imaginative, and expensive efforts at turbulence prediction by any plausible approach. This should not defeat common sense, as argued elsewhere [1], and we must have visibility of when a method may progress from experimental to established and useful in engineering (research uses of simulation are another matter). Chapman made such predictions in 1979 [2], which still carry weight although his view of turbulence prediction in the 1990's is now recognized as optimistic.

This paper focuses on the *numerical* prediction of *turbulent* flow regions. The equally difficult problem of transition prediction is mentioned only in passing. Physical testing methods in the transportation industry are beset by their own severe transition- and turbulence-related difficulties. Tests with scale models usually imply both lower Reynolds numbers and higher freestream turbulence levels (in addition to blockage, bracket and mounting issues, and aero-elastic differences). The resulting scale effects can be misleading, with unforeseen reversals of the normal trend (by which higher Reynolds numbers bring better performance), especially as competing companies seek optimal aerodynamic designs; such designs have narrow margins and magnify the sensitivity to viscous effects. Thus, industry demands accuracy from Computational Fluid Dynamics (CFD), but not perfection.

Also note that, whether in the airframe, turbine engine, or automotive industry, turbulence is not the only obstacle in CFD. Major numerical challenges remain between the state of the art and the routine calculation of flows over even moderately complex 3D geometries. These challenges relate not only to computing cost, but also to solution quality, particularly in terms of gridding. Presenting turbulence as the only "pacing item" in CFD might benefit research funding, but it is not accurate. On the other hand, many more capable people are engaged in grid generation, solvers, and pre- and post-processors, than in turbulence. Our effort may be unbalanced, although more duplication occurs on the programming side (it is easier to show progress in programming, let alone in code exercising, than in modelling). Sharing large codes is more difficult than sharing turbulence models, for which the equations (normally!) fit on one page. With a few exceptions, models have been freely published.

The numerical strengths of CFD increase by the year thanks to the progress of computers and algorithms, whereas turbulence modelling *can* stagnate. If that is the case long enough, modelling will become the pacing item in important types of CFD in a matter of a decade or less, at least in the Reynolds Averaged Navier-Stokes (RANS) mode. It is then very sensible to examine approaches that trade "intelligence" (in the sense of powerful turbulence theories) for computing effort (unfairly described as "brute force"). It is my purpose to provide a viewpoint on such methods, which I predict will proliferate, and make a major contribution. The most stimulating issue may be the share between empiricism and numerical force in the eventually successful methods (§2). The concrete cost issues are addressed through a table attached to §3.

2. PHYSICAL ASPECTS

2.1. RANS models

The field of classical RANS turbulence modelling is active. At a recent biennial international symposium, about twenty-five papers presented new models or new versions of models [3]. These were offered for outside use, with varying degrees of sincerity and completeness in the description. No student of turbulence has the time to give each of these serious consideration. The full range of RANS methods is receiving work; this unfortunately testifies that no class of models has emerged as clearly superior, or clearly hopeless. Activity is not even restricted to differential methods; isolated groups are refining integral boundary-layer solvers, to allow more three-dimensionality and more separation. The same seems to apply to algebraic models. Eddy-viscosity transport models, being the simplest models that can be applied with a general grid structure, are now used extensively. The step back from two equations to a single equation has clearly not crippled the approach [4–6], while tangibly reducing the true cost of solutions. Conversely, models with up to four equations are in contention [7,8]. Perot & Moin's is especially intriguing.

Having referred in the abstract to "Challenges (I) and (II)", I could add the following "Challenge Zero". Complete configurations often have laminar regions in their boundary layers; it is very helpful if a turbulence model can be "dormant" in such regions, meaning that its transport equations accept solutions with vanishing Reynolds stresses. Similarly, regions of irrotational and non-turbulent fluid, which are large in external aerodynamics, do not physically influence the turbulent regions such as boundary layers (weak freestream activity does have much influence on natural transition, but we leave transition prediction to a separate method). Again it is very helpful if the model accepts zero values in such regions, or small values without influence on the turbulent layers. At the same time, the model should allow the contamination of a laminar shear layer by contact with a strongly turbulent layer (contamination by moderate freestream turbulence is more subtle, and is within reach of only a few models). This all depends on the behaviour of the model at the turbulent/non-turbulent interface. In some models the stress level in the turbulent layer depends demonstrably on either the freestream values of the turbulence variables or, even worse, on the grid spacing at the interface. Few people have devoted attention to this question [9,10,5], and model descriptions sometimes make no mention of recommended freestream values (and also fail to demonstrate insensitivity). However, it happens that the k-ϵ, SST and S-A models, which all three pass the freestream-sensitivity test, can fairly be described as "popular". Possibly, their tendency to give the same answer in different codes is valued by the users. In the perennial question of the choice of a second variable in two-equation models, freestream sensitivity should be given a high priority. It is much more important than the value of some high derivative at a solid wall.

The model activation or "transition" from laminar to turbulent boundary layer is in fact more troublesome than the interface with irrotational fluid. Even though the S-A model was designed and tested for it, users still encounter premature transition. This occurs even with first-order upstream differencing, which at first sight should guarantee that unwanted nonzero values of eddy viscosity are "washed out" of the region upstream of the numerical trip. A factor in this is the steep variation of the eddy viscosity at transition [11]: a grid may be fine enough in both the laminar and turbulent regions but much too coarse at transition, creating oscillations which then propagate upstream. This problem can be solved with grid adaptation, but it remains an embarrassment to the model designers, especially since the transition process is not modeled accurately to start with [11]. The failure of most models to predict relaminarisation also causes frustration. While it is not reasonable to expect a model to predict transition in quiet environments, expecting relaminarisation is rather justified.

In terms of Challenge (I), the different classes of models are surprisingly even. Within that challenge, we can include the prediction of skin friction and boundary-layer thicknesses (which dictate the parasite drag in the absence of separation), along with separation (which creates pressure drag). Integral methods and algebraic models have been so well optimised that surpassing them with any Navier-Stokes model is difficult. Reasons include the grid needed, the intrusion of artificial dissipation, and the constraints placed on the turbulence model such as locality, performance in free shear flows, and simplicity. It is geometric complexity and the drive towards massive separation, not lack of accuracy, that are making integral and algebraic methods obsolete.

2.2. Simple RANS models

Appreciable improvements will be made to the simpler transport models, usually by adding new empirical terms such as for streamline curvature or for better anisotropy of the Reynolds-stress tensor (non-Boussinesq constitutive relations) which can, for instance, create secondary flows of the second kind in a square pipe [12]. This work is and should be very cautious, as it is highly desirable for new versions to preserve all or almost all of the past successes of a model. In other words, we hope for gradual progress on Challenge (II) but are unwilling to give up any of the accuracy and experience base in Challenge (I). For this reason, this author is very intent on limiting the number of versions of the S-A model, believing that it best serves the needs of the community. In addition, the rate at which new models or even versions are added to large 3D codes is unfortunately very slow. Codes become so large they are difficult to manage, and frequent changes in computer architecture divert the attention of the code custodians. This could cause a model to become entrenched, if it was first on the "market", and to dominate even after its accuracy has been surpassed.

There is little dispute that the ultimate potential of eddy-viscosity models does not include separated flows over 3D geometries. In fact, 2D bluff bodies are sufficient to make them fail, even with sharp corners (certainly in steady mode, see §2.4 for the unsteady mode). The models are just too simple and replete with empiricism, and are trained in such a small pool of simple shear flows, that they have no reason to generalize to complex flows. We should however heed a remark of Hunt [13], which I slightly paraphrase:
"It is important to note that in most flows (including those over bluff bodies) where the duration of a distortion is smaller than the intrinsic time scale of the turbulence, there is insufficient time for the turbulence to affect the *mean* flow and therefore an erroneous turbulence model has little effect on the *mean* flow. Thus, fortuitously, in most turbulent flows one-point models of turbulence only affect the mean flow calculations where the models are most appropriate (namely in shear flows where the intrinsic time scale is smaller than the distortion time scale)".

Hunt appears to place all one-point RANS models, of any complexity, in the class of turbulence treatments that have "no reasonable claim" to provide accurate stresses in complex flows, but in many cases do "little enough damage". I can easily accept this assessment of my own models.

An example is given by Ying *et al.*, who compare measured and calculated Reynolds stresses over a multi-element airfoil [14]. This is the type of flow Hunt had in mind. As the shear layers (boundary layer and slat wake) pass over the trailing edge of the main airfoil element, the streamlines have a modest deflection, as part of the abrupt merging with the stream from below the trailing edge. The strain-rate tensor has an excursion that is *not* modest, and propagates to the calculated Reynolds-stress tensor. In contrast the measured Reynolds stresses show no such excursion, and their behavior is consistent with that of a "conserved quantity", with only a slow evolution in the streamwise direction. The anomaly in the computed stresses is due to the eddy-viscosity approach (the eddy viscosity is conserved, instead of the stresses). It certainly makes the experiment-computation comparison more delicate. On the other hand, the velocity profiles downstream show no clear sequel of the stress excursion, as predicted by Hunt.

Another wording of Hunt's line of thought is that quite a few flow regions that appear complex and 3D are dominated by "vortical inviscid" physics. It may well be the case for the "necklace" vortex at a wing-wall junction. Its characteristics may depend more on the upstream growth of the boundary layer, which a simple model can accurately predict, than on the Reynolds stresses inside it. This contrasts with the secondary vortices in a square duct, which are created by the turbulence. These vortices expose straight eddy-viscosity models, but their practical importance is modest. Thus, a simple model can "get credit" for the successful calculation of a new flow, merely because the Reynolds stresses it generates in the complex regions are not damaging; usually, it is just as well if the stresses are too weak. The chances that the flow feature will be smeared due to insufficient grid resolution are also higher in such regions, because the user's experience base or willingness to manually refine the grid is less than in attached boundary layers. Unstructured adaptive grids will address that problem, but only in the next generation of codes.

Hunt's optimism does not extend to primary bluff-body flows such as a stalled airfoil. In view of their limited prospects after separation, it is natural that most of the refinements applied to simple models will be aimed at their accuracy in boundary layers, including short separation bubbles and curvature, and a few thin shear flows.

2.3. Complex RANS models

I conclude that the simpler transport models will remain useful and receive slight improvements, but that "something else" must be found before CFD becomes general. It is a matter of debate whether higher-quality models will provide that answer. I am primarily referring to Reynolds-Stress-Transport (RST) models. Now these models have a much closer connection to the equations, and boast several exact terms. An RST model would remove the anomaly noted in §2.2 with the sudden distortion over the trailing edge [14]. With proper attention to invariance, RST models should generalise from their "training ground" to flows with curvature, or vortex and similar flows, much more reliably than eddy-viscosity models. On the other hand, they also contain many empirical terms particularly in the pressure and dissipation areas, adjusted by trial and error. For some of these quantities, data can be obtained only from DNS which has been limited to simple geometries, although progress is being made. In addition, precise term-by-term matching is often too much to ask for; compensating errors, for instance between the anisotropy of the dissipation and pressure-strain tensors, appear both common and acceptable.

In terms of the two challenges, RST models have a tentative advantage for Challenge (II), the separated and vortical regions [15]. They are usually far from "user-friendly" in the sense of Challenge Zero. For Challenge (I), incipient separation, no model can succeed without excellent empiricism, and it is no easier to impress such empiricism on a complex model than on a simple one [16]. In fact the exact character of certain terms puts them off limits to empiricism; in that sense, RST models are more difficult to "steer".

A classic case is the use of vorticity instead of strain in production terms [17]. This step is neutral in thin shear flows, since both reduce to the shear rate, but it solves the long-standing problem of excessive turbulence levels in the approach to stagnation points. In RST and two-equation models, using vorticity is not legitimate, because the exact production terms contains the strain rate instead. Typically, vorticity is used as a "temporary" expedient, which does nothing for the implicit hope that the dependence on empiricism will gradually decrease. The quandary seems intact as of 1998 [18].

Assessing true progress is made difficult by the constant modifications made in publications; the reader cannot be sure that the new version of the model can also succeed where the last version did. Another concern is the persistence of controversies such as about the use of "wall-reflection" terms or the question of whether RST models reproduce curvature effects without additional empirical modifications. Similarly, Zeman's study of free vortices implies that even RST models need specific curvature/rotation modifications to reproduce the damping of the turbulence [20]. Not only does this make the hope of an elegant resolution to Challenge II seem very remote, but streamline curvature is not a Galilean invariant [21], and therefore Zeman's model for that flow is not application-ready. Separated cases which are problematic for simple models, for instance strong shock interactions are also problematic with complex models [19]. Possibly, solutions with any model suffer from numerical errors in strong interactions.

At the risk of minimizing the work of fellow modelers, I deem it unlikely that a single RANS model, even complex and costly, will provide the accuracy needed in the variety of separated and vortical flows we need to predict. The intellectual task of feeding all the available findings into a truly higher and durable version of a complex model is huge, and few model developers seem keen on doing it. Large groups tend to publish along "tentacles". This fits better with educational, institutional and funding needs than with the needs of the code writer who is in need of a robust, stable and understood formulation.

It appears that Reynolds averaging suppresses too much information, and that the only recourse is to renounce it to some extent, which means calculating at least the largest eddies simply for their nonlinear interaction with the mean flow. This step appears desperate to observers, especially the mathematically oriented ones, with some reason. Prof. Jameson remarked that "we should not compute 1-centimetre eddies over a Boeing 747". My Boeing colleagues keep wishing for a "first-principle-based" turbulence model.

2.4. URANS

The first candidate beyond complex RANS modelling has been called "Very-Large Eddy Simulation" (VLES) or "Unsteady Reynolds-Averaged Navier-Stokes" (URANS). The URANS acronym is more descriptive. Such calculations rely on typical RANS models but are deliberately unsteady; for instance, vortex shedding is allowed past a bluff body [22–24,18]. Durbin correctly notes that the Reynolds stresses created by the time averaging of the URANS solution overwhelm those carried by the model itself, in the separated region, and therefore remove much "responsibility" from that model [7]. Nixon's group used the acronym VLES for some very interesting work [25], but I would classify it as LES, and certainly not as URANS.

I first note that URANS implies a separation of time scales, between that of the shedding and that of the residual turbulence, which is not indicated in measurements of spectra. This is a little disturbing, although some URANS calculations produce a kind of chaos (non-periodic behavior) which widens the spectral peak. Nevertheless, the approach has plausibility, and certainly improves results for bluff bodies; in the boundary layers, there is no strong additional reason to distrust the models because they operate in quasi-steady mode. I also note that for bluff bodies, conducting unsteady calculations is optional *only* in somewhat artificial conditions: those in which a steady solution can be obtained by imposing symmetry, or a large time step, or using a Newton method for convergence. More frequently, a user that is after massive separation will find that the code simply cannot find a steady state, and that the only course is to operate in a time-accurate mode and analyze an unsteady solution, presumably a nearly-periodic one.

A 2D URANS recognizes the role of time, but not of the spanwise coordinate z. By now, DNS and LES results for bluff bodies [26–29] have established that ignoring the z-dependence is not safe, although there are examples of successful 2D simulations with rather large separation [30]. At least in LES and DNS, 3D solutions in 2D geometries are highly justified. We denote them by "3/2D" in the table by §3. A z-dependence obviously belongs in a thorough study at the URANS and higher levels, but its role is clouded by several facts. Let us assume a 2D geometry. Then, the spanwise boundary conditions are arbitrary; periodic conditions are very plausible, but some studies use reflection conditions at the side boundaries. The size of the spanwise domain is also arbitrary. Systematic tests are costly, and a certain finding is that very narrow domains force the flow back to 2D, while very wide ones allow oblique vortex shedding, which is physically correct and has a noticeable effect. Intermediate domain widths could be explored for a *long* time. This issue will resolve itself in practice, in the sense that actual geometries are 3D, but it is an obstacle to a clear understanding of the nominally 2D flow.

2.5. LES and DES

Away from boundaries and without chemistry, Large-Eddy Simulation is well understood, and there is probably little to gain by refining the SGS models. In the wall regions, I have described most of the current LES work as Quasi-Direct Numerical Simulation (QDNS) [1]. By this I mean that these simulations resolve the near-wall "streaks", and the grid spacing is limited in wall units. A "true" or "full" LES, meaning that the Reynolds number based on the grid spacing is unlimited, appears to be a difficult goal (apparently the community switched to QDNS, although the pioneering work of Deardorff [44] was full LES, in order to reduce empiricism in the near-wall treatment). The grid spacing in all directions (or at least in the two directions parallel to the wall) would scale with the boundary-layer thickness. In this area, huge gains are expected from improved SGS modelling. However, it is unavoidable that empiricism will be added; at the least, such a treatment would have to imply values for the constants in the logarithmic law. The method reverts to quasi-steady RANS behaviour near the wall, in the sense that grid refinement does not eliminate the influence of the SGS treatment rapidly at all (until an extreme refinement turns the method into QDNS).

A robust and accurate treatment of that kind is a plausible target. The streaks seem to be just as "numerous and universal" as the small eddies in free turbulence (the streaks are not isotropic, but calling the small eddies of free turbulence isotropic is misleading: the collection of eddies that are modeled as SGS in one grid cell at one time step obviously has preferred directions). It is only that the streaks have much more leverage than the small Kolmogorov eddies. It will be well worth the effort, for several reasons. First, the law of the wall is quite a robust feature of boundary layers, although we expect an erosion of its domain of validity, expressed as y/δ, in strongly stimulated flows (δ is the boundary-layer thickness). Second, in 3D flows, the skin-friction vector is very unlikely to vanish; thus, the law of the wall *could* retain its validity even under a separating boundary layer. Finally, most of the difficulty in RANS modelling of strongly stimulated boundary layers resides in the outer region. There, LES can clearly capture effects such as straining, cross-flow, and curvature. Therefore, LES addresses both Challenges, (I) and (II). However, it is at a considerable cost over RANS, and wall-bounded LES with the streaks modeled can be described as hybridized with RANS, although the implied empiricism is confined to a very shallow layer.

At a recent LES workshop, a variety of QDNS and full LES methods were applied to fairly simple geometries with sharp corners [45]. In spite of these helpful features, the conclusions were particularly mixed, and did not make LES or even QDNS appear very mature. The flow past a circular cylinder, even at Reynolds numbers of a few thousand, has also led to quite different levels of agreement with experiment in the last few years. SGS models also remain quite different between different schools.

In §3 I discuss why LES, even with the best wall-region treatment, is very far from affordable in aerodynamic calculations, and will be for decades. This is due to the large regions of very thin boundary layers, where δ is of the order of 0.1% of the airfoil chord c. It led us to propose Detached-Eddy Simulation (DES), a further step in the hybridization of LES [1]. The idea is to entrust the whole boundary layer (populated with "attached" eddies) to a RANS model, and only separated regions ("detached" eddies) to LES. It is consistent with the two positions that Challenge (I) is a reasonable one for RANS models whereas Challenge (II) is not, and that LES is well understood away from walls. We show below that it leads to a manageable computing cost [31].

A typical application of DES is to a wing with a spoiler or a landing gear. Large areas of boundary layers are treated efficiently with quasi-steady RANS. Behind the spoiler, the momentum transfer is dominated by large unsteady eddies which are candidates for LES on two counts. First, they are not as numerous as the "horseshoe" vortices in the outer part of a boundary layer (let alone the wall streaks) and second, they are geometry-specific. An additional benefit of DES is its unsteady information. Though useless for many purposes, such as the range of the airplane, it will sooner or later be of great use for structural or noise studies.

An attractive feature of DES is that it is simply formulated, and already being tested. This is not the case for similar concepts which have been informally envisioned. Many of them are zonal. DES is not, which I view as much preferable for routine use, and only requires a quick alteration of the S-A one-equation model. On the other hand deriving an efficient unsteady code, as needed for DES, from a steady one is not trivial. In a companion paper we present DES results for an airfoil at high angles of attack, a classical Challenge II example [31]. The agreement with experiment is surprisingly good, but no better than in the best examples of DNS and LES for bluff-body flows [27,28]. DES will require definite skills from the user in directing the grid resolution; however, RANS also benefits from careful grid generation. Presently, a few patient users are refining grids "manually" after exploring preliminary solutions [14], but I am afraid many solutions are under-resolved in the separated regions.

Some similarities between DES and past treatments of the wall region in LES [41,42] have led to comments such as "DES contains nothing new". These comments stem from a narrow focus on the classical applications of LES, such as channel flow. There, it is correct that DES is no more or less plausible than methods which blend simple buffer-layer models and simple SGS models. For instance, one could well use an eddy viscosity that is the smaller of the one given by the mixing-length approximation, with Van-Driest damping, and the Smagorinsky model. The capability DES has in addition is hpwever clear: to treat the entire boundary layer in RANS mode, to the standard of the better engineering models. A mixing-length model does not have this capability; the lowest level that does is an algebraic model such as Cebeci-Smith. Algebraic models do not lend themselves to complex geometries, unstructured grids, or to function under detached eddies; therefore, a one-equation model is the simplest type that makes DES practical. DES is young and has yet to be tested in a channel, with an LES grid; I fully expect reasonable results, but cannot predict how accurate the additive constant C in the logarithmic law will be. Note that in such a simulation, DES relaxes the restrictions on the wall-parallel spacing in wall units, such as Δx^+, but not the wall-normal spacing which will have to be of the order of $y^+ = 1$.

I can formulate, but am not pursuing, still another hybrid concept. It is zonal and would consist in treating the "unchallenging" regions of the boundary layer with RANS, and the Challenge-(I) regions with full LES. The method would switch to LES upstream of separation, which tentatively makes another step in accuracy. The difficulties are: the "artificial intelligence" of identifying where the switch should be located; the generation of quality turbulent eddies for the RANS region to dispatch into the LES region (the regions might have to overlap). This is a concept that would live much more easily in a 2D boundary layer than on a 3D object. I believe that the "eddy seeding" problem is much less severe with DES, because a separated shear layer is exposed to vigorous new instabilities, thanks to both the removal of the wall confinement and what I loosely call "absolute instabilities" [43] (in contrast, the RANS-to-LES switch in the other concept would occur in a region of "convective instabilities").

2.6. DNS

The value and requirements of Direct Numerical Simulation are well known. Few DNS projects have been conducted at a "full" Reynolds number, but the attachment line of swept wings is an example. DNS was applied at the (local) Reynolds number of the flow on an airliner. This is a case of "microscopic" simulation, in which it is justified to isolate a very small region of the flow (the justification relies on experiments). I once received dubious praise for simulating "a milli-second over a postage stamp"! Simulations of homogeneous turbulence and of other boundary layers could also be described as microscopic. DNS of a whole device is normally out of the question. It is a beautiful research tool; in fact I believe its reach is sometimes under-estimated, due to a misguided insistence on simulating at the "right" Reynolds number.

The argument, which has long been a minority one, is the following. When asking a fundamental question in turbulence, assume we have the choice between a DNS and an LES having the same cost. The DNS will have a slightly larger range of scales in each direction (certainly less than double), because of saving the SGS model cost. The LES will have a higher Reynolds number; if a QDNS, the difference will be less than a doubling. The LES will assume that the unresolved eddies have a simple enough behaviour to be modeled;

for instance the great majority of the Reynolds shear stress will be resolved. If so, the same-cost DNS can run at a Reynolds number sufficient to sustain turbulence. Extrapolating the DNS results to the LES Reynolds number can also be done with confidence (especially if the DNS is possible over a range of Reynolds numbers). An extrapolation "after the simulation" is inexpensive and can be refined, much more easily than the LES can be re-run with an different SGS model. By that standard, we could have counted one run for a thorough DNS study, as opposed to maybe three runs for a thorough LES study, which changes the cost balance somewhat. Three to four runs is typical in LES studies, many of which are presented as comparative tests of SGS models and/or as tests of LES itself, by comparison with experiments. In contrast I believe a DNS study can be free-standing [32]. In addition, the extrapolation can then reach any Reynolds number (this amounts to the view that turbulence is more predictable, the higher the Reynolds number, which is not shared by all). Atmospheric scientists never consider DNS for the Planetary Boundary Layer, but fundamental PBL questions can very well be asked with DNS and extrapolation [33], and their attitude is counter-productive.

One good reason for doing QDNS is the comparison with a laboratory experiment that is moderately out of reach of DNS. This occurs typically when the experiment was designed to allow measurements of the smallest eddies; physical limits restrict the possible range of scales, but not as severely as DNS does.

Below, I classify DNS as requiring "no empiricism". This does not imply that the DNS of a complex flow is free of decisions once an accurate DNS code has been created. In the case of channel flow, the decisions consist in the grid spacing and the domain size. For both, the direction of "goodness" is clear: smaller spacing and larger domain. Homogeneous turbulence adds the influence of the initial conditions or stirring system, for which goodness is not simply a direction. There is an art, and people may disagree regarding its "color". Flows containing transition require many further decisions, regarding the freestream-disturbance and wall-imperfection content and the vibrations. This fine information is not found in the CAD file of an airplane or car. Engineering DNS would not be a "black box".

Recent Reynolds-number increases in DNS have been almost unnoticeable, partly because the "super-computers" have nearly stagnated, certainly compared with personal computers. For a really attractive new study, for instance to lock the value of the Karman constant, a factor of 5 or preferably 10 in Reynolds number would be needed. Therefore the DNS effort has, correctly, be directed instead at simulating more complex geometries, or simple ones with strong pressure gradients, three-dimensionality, rotation and curvature, complex deformations, heat transfer, combustion, shock waves, and noise [34–40]. Fully successful DNS studies of the supersonic boundary layer should appear very soon. The current standard includes "reasonable results" but not quantitative comparisons, a problem being that low-Reynolds-number supersonic experiments are lacking [35]. A definitive study of the interaction with a normal shock will certainly be of great interest to the airliner industry.

2.7. Role of grid refinement

In RANS and URANS, the equations possess a smooth exact solution, and the numerical solution approaches that solution as we refine the grid. The aim of grid refinement is numerical accuracy. In contrast, in an LES, the Sub-Grid-Scale (SGS) model adjusts to the grid so that the smallest resolved eddies match the grid spacing. In a finer grid, resolving eddies to a smaller size gives the large energy-containing eddies more eddies for genuine nonlinear interactions, making them more accurate. The aim of grid refinement is now *physical* instead of *numerical*, to use simple words. This distinction is tracked in the table in §3 (several methods had to be labeled "hybrid", as their aim is different in different flow regions). Another description of it is that when the aim is numerical, the turbulence/SGS model does not depend on the grid spacing but when the aim is physical, it does. A consequence is that in URANS, no amount of grid refinement will override the influence of the empirical content of the turbulence model. In contrast, in a method with the "physical" aim, grid refinement weakens the role of the modeled eddies and thus improves the fidelity of the simulation. A 20% change of the Smagorinsky constant in a well-resolved LES is minor, but a 20% change in the Karmnan constant is not.

The character of grid refinement has implications for adaptive grid approaches. When the aim is numerical, error estimates are constructed and guide the refinement. When the aim is physical, error estimates are not simple. In fact the question of the "order of accuracy" of LES has apparently not been asked. In the classical LES, with cut-off in the inertial range, the resolved kinetic energy converges to order 2/3. However the full kinetic energy can be recovered and converges much faster, as does the Reynolds shear stress, because it has a steeper spectrum, possibly –7/3 instead of –5/3. The shear stress has more impact in practice, and is probably a better measure (in isotropic turbulence, the low end of the spectrum is a fair model of the "more relevant" quantities, and must converge much faster than to order 2/3). Grid adaptation in LES will be a field of study; we could adapt instantaneously (follow eddies) or gradually, based on the history of a region in creating small eddies. Truly balancing the allocation of numerical effort within a complete flow will be a great achievement. So far, our approach to grid design in LES has been *very* intuitive. An example is the preference for cubic grid cells, away from walls. This will not be viable in an industrial tool.

3. COST ASPECTS

The principal definitions and assumptions which entered the estimates in Table 1 are the following. The target flow is that over an airliner or a car. The acronyms have all been used above. "Aim" refers to the aim of grid refinement, numerical or physical (§2.7). The Reynolds-number dependence refers to the number of grid points. The step from "strong" to "weak" Reynolds-number dependence indicates a change from a slow logarithmic dependence similar to that of the skin-friction coefficient, to a strong one similar to that of the boundary-layer thickness in wall units. "3/2D" refers to simulations which are 3D even if the geometry is 2D. When the geometry is 3D, 3/2D means that the grid spacing scales with the shorter dimension of the device, and does not "take advantage" of high aspect ratios. A clear example would be a wing flap: a 3DRANS will cluster points near its tips but use a loose spacing elsewhere, whereas a 3/2D method will space point by a fraction of the flap chord all along. The step from "strong" to "weak" empiricism indicates, quite arbitrarily, that the only remaining adjustable constants are those in the Law of the Wall.

For the grid spacing, RANS and DES figures are based on current practice. The requirements are well understood for the spacing normal to the wall. In the other directions, the geometry is assumed to have only a moderate number of features such as flaps and spoilers. Under "DES" I include both strict DES as defined in [1], and other hybrid methods which are likely to be actively developed in the next few years, with the general expectation that they will treat the simple attached boundary layers with RANS. For such methods, a grid block of the order of 64^3 points appears adequate to resolve a separated region, since we used about this many points on the stalled airfoil [31]. At higher Reynolds numbers, the cost increase is modest, since only the normal grid spacing needs to be reduced. Thus, the grid increase over 3DRANS is plausibly in the millions of points, not tens of millions, and 10^8 is fair for the grid count. For LES we had estimated 10^{11} for a clean wing [1], leading to a few times more for the whole aircraft. The DNS estimate is based on grid patches with an area of 100 wall units, and agrees with that of Moin & Kim [46]. The number of steps uses the same grid information and CFL numbers of order 1, and assume the simulation needs roughly 6 spans of travel for an airplane.

The readiness estimates are based on the "rule of thumb" that computer power increases by a factor of 5 every 5 years. This will be disputed, but another rule has been a factor of 2 every 2 years, which is not much faster. You are free to apply your favorite rate of progress, starting from the assumption that a very expensive problem today costs about 10^{15} floating-point operations. "Readiness" roughly means that a simulation is possible as a so-called "Grand Challenge". Industrial everyday use will come later.

4. OUTLOOK

Progress in numerical methods and computers is intensifying the challenge for turbulence treatments, to provide a useful level of accuracy in massively separated flows over fairly complex geometries at very high Reynolds numbers. This is desirable in the near future, between 5 and 10 years, and not only on a research basis; industry is more than ready for this capability. In addition, the needs of non-specialist users and automatic optimizers dictate a very high robustness. Flows with shallow or no separation appear to be within the reach of the current steady RANS methods or rather their finely calibrated derivatives, incorporating modest improvements such as nonlinear constitutive relations. For such flows, transition prediction with generality, accuracy, and robustness may well

Table 1
Summary of strategies

Name	Aim	Unst.	*Re*-dep.	3/2D	Empiricism	Grid	Steps	Ready
2DURANS	num	yes	weak	no	strong	10^5	$10^{3.5}$	1980
3DRANS	num	no	weak	no	strong	10^7	10^3	1985
3DURANS	num	yes	weak	no	strong	10^7	$10^{3.5}$	1995
DES	hybr	yes	weak	yes	strong	10^8	10^4	2000
LES	hybr	yes	weak	yes	weak	$10^{11.5}$	$10^{6.7}$	2045
QDNS	phys	yes	strong	yes	weak	10^{15}	$10^{7.3}$	2070
DNS	num	yes	strong	yes	none	10^{16}	$10^{7.7}$	2080

prove more challenging than turbulence prediction.

With massive separation, it appears possible we will give up RANS, steady or unsteady. This will probably be the major debate of the next few years. The alternative is a derivative of LES, in which the largest unsteady geometry-dependent eddies are simulated and (for most purposes) "discarded" by an averaging process. We have to balance our ambitions with cost considerations, and I tentatively provided a table summarizing the issue. A major consideration is whether LES is practical for the entire boundary layer, and I strongly argued that it will not be, in the foreseeable future. This forces hybrid methods, with quasi-steady RANS in the boundary layer. I have effectively "defined" LES as a simulation in which the turbulence model is tuned to the grid spacing, and RANS as the opposite. Other more subtle definitions probably exist, but this one seems to classify almost all the studies to date. Speziale's hybrid proposal involves the grid spacing and the Kolmogorov length scale but, surprisingly, not the internal length scale of the RANS turbulence model; thus, it is difficult to classify [47]. Variations on the now-running DES proposal clearly have a wide window of opportunity.

The plausible spread of hybrid methods highlights the durability if not the permanence of a partnership between empiricism and numerical power in turbulence prediction at full-size Reynolds numbers. This demands a balance in funding and in publication space. Since hybrid methods offer much leeway in the boundary between "RANS regions" and "LES regions", the more capable the RANS method is, the lower the cost of the calculation will be. Therefore, the switch to LES in some regions does not remove the incentive to further the RANS technology. This scene also raises the issue of which core of experiments will be the foundation of the empirical component of the system. As ever, we will need simple flows for calibration of the RANS sub-system, and more complex flows for validation of the full CFD system.

Acknowledgements

I am grateful for the comments of J. Crouch, W-H. Jou, G. Miller, A. Secundov, and M. Strelets, and for years of informal teaching by P. Bradshaw.

REFERENCES

1. P. R. Spalart, W.-H. Jou, M. Strelets and S. R. Allmaras, 1997. Comments on the feasibility of LES for wings, and on a hybrid RANS/LES approach. 1st AFOSR Int. Conf. on DNS/LES, Aug. 4-8, 1997, Ruston, LA. In "Advances in DNS/LES", C. Liu and Z. Liu Eds., Greyden Press, Columbus, OH.
2. D. R. Chapman, 1979. Computational aerodynamics development and outlook. *AIAA J.* **17**, 12, 1293.
3. F. Durst, B. E. Launder, F. W. Schmidt, J. H. Whitelaw, M. Lesieur and G. Binder, eds., 1997. 11th Symp. Turb. Shear Flows. Sept. 8-10, Grenoble, France.
4. A. N. Gulyaev, A. N. Kozlov, V. Ye and A. N. Secundov, 1993. Universal turbulence model ν_t-92. Ecolen Science Research Center Preprint No. 3, Moscow.
5. P. R. Spalart and S. R. Allmaras, 1994. A one-equation turbulence model for aerodynamic flows. *La Recherche Aérospatiale*, **1**, 5.
6. D. C. Wilcox, 1998. Turbulence modeling for CFD. http://webknx.com.dcw.

7. P. A. Durbin, 1995. Separated flow computations using the $k - \epsilon - v^2$ model. *AIAA J.* **33, 4**, 659.
8. B. Perot and P. Moin, 1996. A new approach to turbulence modeling. Proc. 1996 Summer Prog., Ctr for Turb. Res., Stanford.
9. F. R. Menter, 1992. Influence of freestream values on k-ω turbulence model predictions. *AIAA J.* **30**, 6, 1657.
10. J. B. Cazalbou, P. R. Spalart, and P. Bradshaw, 1994. On the behavior of two-equation models at the edge of a turbulent region. *Phys. Fluids A*, **6** (5), 1797.
11. P. R. Spalart, 1995. Topics in industrial viscous flow calculations. Coll: Transitional Boundary Layers in Aeronautics, Royal Neth. Acad. of Arts and Sciences, Dec. 6-8.
12. C. G. Speziale, 1987. On nonlinear K-l and K-ϵ models of turbulence. *J. Fluid Mech.*, **178**, 459.
13. J. C. R. Hunt, 1990. Developments in computational modelling of turbulent flows. Proc. ERCOFTAC Work., 26-28 March, Lausanne, Switz. Cambridge U. Press.
14. S. X. Ying, F. W. Spaid, C. B. McGinley and C. L. Rumsey, 1998. Investigation of confluent boundary layers in high-lift flows. AIAA 98-2622. To appear, *J. Aircraft.*
15. B. E. Launder, 1988. Turbulence modelling in three-dimensional shear flows. AGARD CP-438, Fluid Dyn. 3D Turb. Shear Flows and Transition, Oct. 3-6, Turkey.
16. F. S. Lien and M. A. Leschziner, 1995. Modelling 2D separation from a high lift aerofoil with a non-linear eddy-viscosity model and second-moment closure. *Aero. J.*.
17. M. Kato and B. E. Launder, 1993. The modelling of turbulent flow around stationary and vibrating square cylinders. Ninth Symp. Turb. Shear Flows, Kyoto.
18. G. Bosch and W. Rodi, 1998. Simulation of vortex shedding past a square cylinder with different turbulence models. *Int. J. Num. Meth. Fluids* **28**, 601.
19. T. Hellström, L. Davidson and A. Rizzi, 1994. Reynolds stress transport modelling of transonic flow around the RAE2822 airfoil. AIAA 94-0309.
20. O. Zeman, 1995. The persistence of trailing vortices: a modeling study. *Phys. Fluids A* 7, 135.
21. P. R. Spalart and M. Shur, 1997. On the sensitization of simple turbulence models to rotation and curvature. *Aerosp. Sc. and Techn.*, **1**, 5, 297.
22. S. H. Johansson, L. Davidson and E. Olsson, 1993. Numerical simulation of vortex shedding past triangular cylinders at high Reynolds number using a k-ϵ turbulence model. *Int. J. Num. Meth. in Fluids* **16**, 859.
23. S. A. Orszag, V. Borue, W. S. Flannery and A. G. Tomboulides, 1997. Recent successes, current problems, and future prospects of CFD. AIAA-97-0431.
24. M. L. Shur, P. R. Spalart, M. Kh. Strelets and A. K. Travin, 1996. Navier-Stokes simulation of shedding turbulent flow past a circular cylinder and a cylinder with a backward splitter plate. Third Eur. CFD Conf, Sept. 1996, Paris.
25. R. E. Childs and D. Nixon, 1987. Turbulence and fluid/acoustic interaction in impinging jets. SAE 87-2345.
26. G. E. Karniadakis and G. S. Triantafyllou, 1992. Three-dimensional dynamics and transition to turbulence in the wake of bluff objects. *J. Fluid Mech.* **238**, 1.
27. F. M. Najjar and S. P. Vanka, 1995. Effects of intrinsic three-dimensionality on the drag characteristics of a normal flat plate. *Phys. Fluids* **7** (10), 2516.
28. R. Mittal and S. Balachandar, 1995. Effect of three-dimensionality on the lift and

drag of nominally two-dimensional cylinders. *Phys. Fluids* **7** (8), 1841.

29. S. A. Jordan and S. A. Ragab. 1998. A large-eddy simulation of the near wake of a circular cylinder. *J. Fluids Eng.* **120**, 243.
30. J. C. Muti Lin and L. L. Pauley, 1996. Low-Reynolds-number separation on an airfoil. *AIAA J.* **34, 8**, 1570.
31. M. Shur, P. R. Spalart, M. Strelets and A. Travin, 1999. Detached-eddy simulation of an airfoil at high angle of attack. 4th Int. Symposium on Eng. Turb. Modelling and Measurements, May 24-26, Corsica.
32. P. R. Spalart and, M. Kh. Strelets, 1997. Direct and Reynolds-averaged numerical simulations of a transitional separation bubble. 11th Symp. Turb. Shear Flows, Sept. 8-10, Grenoble, France.
33. G. N. Coleman, J. H. Ferziger, and P. R. Spalart, 1990. A numerical study of the turbulent Ekman layer. *J. Fluid Mech.* **213**, 313.
34. M. Alam and N. D. Sandham, 1997. Numerical study of separation bubbles with reattachment followed by a boundary layer relaxation. Parallel CFD 97. Elsevier.
35. N. A. Adams, 1998. Direct numerical simulation of turbulent compression ramp flow. *Theor. Comp. Fluid Dyn.* **12**, 102.
36. G. N. Coleman, J. Kim and R. D. Moser, 1995. A numerical study of turbulent supersonic isothermal-wall channel flow. *J. Fluid Mech.* **305**, 159.
37. S. Gavrilakis, 1992. Numerical simulation of low-Reynolds-number turbulent flow through a straight square duct. *J. Fluid Mech.* **244**, 101.
38. Y. Na and P. Moin, 1998. Direct numerical simulation of a separated turbulent boundary layer. *J. Fluid Mech.* **370**, 175.
39. S. Ragab and M. Sreedhar, 1995. Numerical simulation of vortices with axial velocity deficits. *Phys. Fluids* **7** (3), 549.
40. K-S. Yang and J. H. Ferziger, 1993. Large-eddy simulation of turbulent obstacle flow using a dynamic subgrid-scale model. *AIAA J.* **31, 8**, 1406.
41. U. Schumann, 1975. Subgrid scale model for finite difference simulations of turbulent flows in plane channels and annuli. *J. Comp. Phys.* **18**, 376.
42. E. Balaras, C. Benocci and U. Piomelli, 1996. Two-layer approximate boundary conditions for large-eddy simulations. *AIAA J.* **34**, 6, 1111.
43. P. Huerre and P. A. Monkewitz, 1985. Absolute and convective instabilities in free shear layers. *J. Fluid Mech.* **159**, 151.
44. J. W. Deardorff, 1970. A numerical study of three-dimensional turbulent channel flow at large Reynolds numbers. *J. Fluid Mech.* **41**, 453.
45. W. Rodi, J. H. Ferziger, M. Breuer and M. Pourquié, 1997. Status of Large Eddy Simulation: results of a workshop. *J. Fluids Eng.* **119**, 248.
46. P. Moin and J. Kim, 1997. Tackling turbulence with supercomputers. *Scient. Amer.* **276, 1**, 62.
47. C. G. Speziale, 1998. Turbulence modeling for time-dependent RANS and VLES: a review. AIAA J. **36**, 2, 173.