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BRIAN SPALDING: CFD & REALITY

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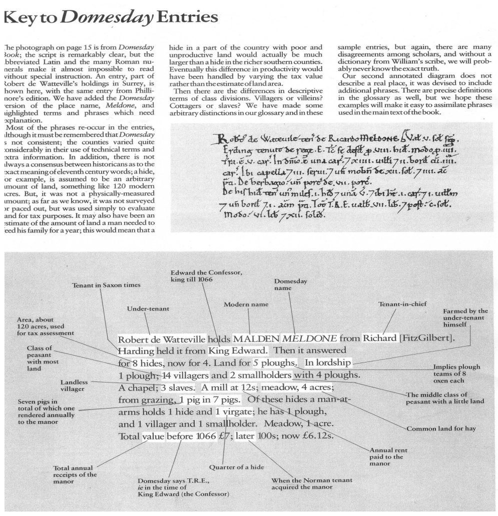
ABSTRACT: Brian Spalding did not invent CFD. He did not even coin the name. But more than anyone else, he created the practice of CFD – its application to problems of interest to engineers. The author has been associated with, and an integral part of the team led by, Prof. Spalding that developed the basic engineering practice that came to be known as the Imperial College (IC) approach to "CFD". Most of today's commercially available CFD software tools trace their origin to the work done by the IC group in the decade spanning the mid-60s and mid-70s.

This talk traces the key moments of the CFD developments at Imperial College and the role played by Brian Spalding as a leader of, and as an active contributor to, the IC Group. His key insights during this decade often made breakthroughs possible and re-directed the focus at critical moments. The talk will also explore the opportunities missed by the IC Group during this decade of breakneck progress in CFD.

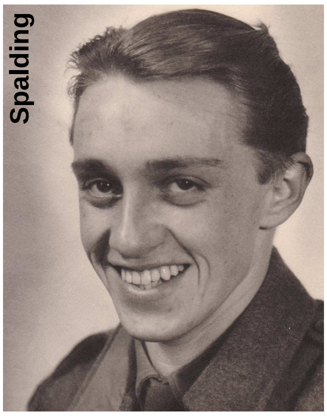
INTRODUCTION

I first met D. Brian Spalding (popularly known as DBS) in 1965. If you search the web for DBS today, other than the Aston Martin DBS V12, one of the items that prominently pops up is **D**eep **B**rain **S**timulation – an innovative development in Neurology. How appropriate! Of course I did not know that when I first met him. My association with him has certainly been brain stimulating and has truly changed the course of my life and the point of view with which I view science and engineering.



D. Brian Spalding was born on 9th January 1923 in New Malden in the now picturesque suburbia of London. It is remarkable that the town finds mention in the Domesday Book (Meldone in those days). The towne then was held by Hardings, Wattevilles and FitzGilberts. No Spalding there. Had Brian's ancestors held it, total annual receipts for the manor would have been about £7 which would have been a tidy sum and Brian may have gone on to make his mark in the financial markets of the City rather than in helping create an entirely new branch of engineering science today called CFD.

Spalding attended Kings College School from the age of 9 to 18 and was then admitted to Oxford University where he obtained his B.A. in Engineering Science at the Queens College in 1944. He



then worked at Shell for a year. In 1945 he joined the newly established Rocket Propulsion Establishment (RPE) of the Ministry of Aircraft Production. The RPE had no rockets yet; it was set up to develop the rocket technology in response to the success of the German V2 Missile. Soon thereafter Brian was dispatched by the RPE to Germany to learn the secrets and intricacies of rocket engines. During 1945-1946 he was at the Luftfahrtforschungsanstalt Herman Goering (Herman Goering Institute of Aeronautical Research) at Voelkenrode, near Braunschweig and its out-station at Trauen on the Lueneberge Heide.   The V2 team, led by Werner von Braun, was already in the American zone; but the British collected ten members of a different group which had developed the motor for the Messerchmidt 163 rocket-propelled airplane, the propellants of which were hydrazine hydrate and hydrogen peroxide. They brought this team to Trauen and set them to work converting their rocket motor to burn kerosine and liquid oxygen. The work continued until 1946, at which time the Allies agreed that no further such work was to be done in Germany. The Trauen team was then transported to England to continue its work at RPE, which was little more than a collection of huts on a disused airfield. Brian was their mentor, and indeed lived with them in one of the huts until his marriage in 1947 to Eda Goericke, who, having formerly worked at a hydrogen-peroxide-making plant in the Harz Mountains had moved to Voelkenrode when the war ended.

Somewhat later, the reconstruction of the UK Scientific Civil Service resulted in Brian's being transferred, much to his disappointment, to the Metrology Department of the National Physical Laboratory (NPL). Since this was at the beginning of the cold war, one wonders if this career move had anything to do with Brian’s membership in the communist party during his student days (Burgess and McLean affair hit the news shortly thereafter in 1951). It certainly ensured that he could do no harm to the British national interests. This proved to be a blessing in disguise because during this time Brian became thoroughly familiar with instrumentation and the art and science of measurements. This would stand him in good stead during the next stage of his career. It also resulted in his not standing, as he otherwise would have done, by the side of Johann Schmidt, the leader of the German team still working at RPE, at one shocking moment. That was when an explosion of the kerosine-fuelled rocket motor, strong enough to break apart the bolts holding the window through which he was watching, exploded and killed him instantly.

In 1948 Brian got an ICI Fellowship to go to Cambridge University (Pembroke College) for a Ph.D. With his RPE background, he knew that wanted to do research on the combustion of liquid fuels. The Head of Department, John Baker, appointed A.L.L.Bird as his supervisor since he had some interest in diesel engines. Bird and Spalding had very little to do with one another. Brian knew more about engines and liquid fuels than perhaps his supervisor. In any case Bird’s idea of recent publication often meant 20 years old. Bird tried to get Brian to use some old apparatus and Brian protested to Baker. Perhaps, prophetically, he knew of Brian’s tendency to go out on a limb. He advised Bird to “give him enough rope to hang himself”. Thereafter Brian was on his own, Bird retired soon thereafter and since the regulations demanded that every Ph.D. must have a supervisor, a new recruit to the staff, Dudley Robinson was appointed his supervisor. As it turned out it was Brian who wound up advising Robinson on what to do for research! Brian can rightly claim to have had a Virgin Birth – as far as his Ph.D. is concerned. He did have two mid-wives in attendance: Will Hawthorne and E.S. Sellers were the examiners for his Ph.D in 1952.

EARLY PROFESSIONAL CAREER – 1951 -1964

The origin of Spalding's later contribution to CFD goes back to his days at Cambridge University and his Ph.D. Thesis (Spalding, 1951). It is a remarkable thesis in that it "unified" the key hydrodynamic concepts of von Karman [1921] with the heat transfer concepts of Kruzhilin [1936] and the mass transfer concepts of Eckert [1949]. He synthesized these to create a general theory of heat and mass transfer with and without combustion. In the process he made a then unforeseen prediction that the chemical-reaction-rate constants had no influence on combustion until a critical rate of mass transfer was reached. This was later borne out by experiments. Spalding deduced these critical rates by adapting the concepts of Zeldovich and Frank-Kamenetsky [1938], and Semenov [1940], who had been concerned with the quite-different phenomenon of steady laminar flame propagation. This led to a general theoretical framework for the prediction of flame-extinction which was a breakthrough for combustion engineers [Spalding, 1955]. His other notable contributions in combustion include the 'centroid rule' [Spalding, 1957] which caused the predictions of a range of flame-speed studies to fall on to a single curve, the cooled-liquid-film burner for measuring combustion rates and an innovative method for measuring extinction conditions [Spalding, 1951] and a cooled porous burner for measuring flame speeds [Botha and Spalding, 1954]. He also developed an electrical analogue of combustion [Spalding, 1957b]. To my knowledge this was a novel and unique concept and I am not aware of other electrical analogues of combustion.

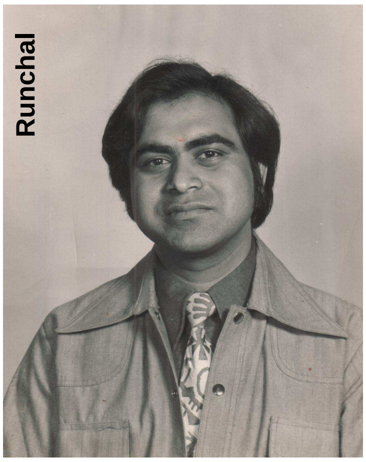
After completing his Ph.D. Brian stayed at Cambridge for a short time and was then recruited by Prof. Owen Saunders in 1954 to join as Reader in Applied Heat, in the Mechanical Engineering Department at the Imperial College, London. Spalding went on to do seminal work in combustion and made key and innovative contributions in evaporation burning of droplets. This work eventually led to the now universally adopted "B" factor and the Spalding Number. Spalding's efforts at unification led to his remarkable book on Heat and Mass transfer [1963] that has greatly influenced subsequent work in this field.

In late 1950s Spalding turned his attention to the important issue of the role that wall shear plays in most engineering flows. He found that the turbulent velocity profile for walls was conventionally represented by a three part profile, a "viscous" sub layer, a "transitional" layer and a "fully turbulent" layer. Spalding found a very unconventional, elegant, and simple solution: express Y+ in terms of U+ rather than U+ as a function of Y+. This key insight enabled him to develop a continuous-function 'wall law', covering viscous, transitional and logarithmic regions [Spalding, 1961]. He was also not quite comfortable with the conventional method of treating wall boundary layers, jets and wakes as distinct flows each with its own physics, mathematics and terminology. Since all these flows are primarily governed by shear, he argued that the underlying physics and mathematics must be represented in a uniform manner. This led to his Unified Theory of Turbulent Boundary Layers, Jets and Wakes [Spalding, 1964]. This was based on the remarkable insight that with a "universal" entrainment law and a suitable two-part profile to represent the wall and wake regions, all such flows can be "universally" represented. A number of his students worked on deriving the entrainment formulae and other input needed for the Unified Theory [e.g. Escudier and Nicoll, 1966, Jayatillaka, 1966, Escudier, 1967]. Soon thereafter, Spalding came to the conclusion that instead of searching for an optimum profile, one can "universalize" the profile method by simply representing the profile as a piece-wise polynomial – or even linear- segments and derive the "weighting functions" from the governing initial and boundary conditions. This freed one from the tyranny of having to find an "ideal" profile to fit a given flow. However it soon became apparent that Spalding's search for a "unified" theory was not yet over since this approach was later found to generate solutions that were occasionally spurious or even singular.

Throughout his career a recurring theme – and prime objective - has been to invent predictive tools that are useful to, and easily used by, practicing engineers. He abhors piece-meal solutions to problems. So "unification" is an important goal; whether that is the unification of flow, heat and mass transfer concepts or that of seemingly different shear flows. Another recurring theme is a readiness to challenge the prevailing wisdom and explore unorthodox ideas. His simple solution of obtaining the adiabatic flame speed (which is unobtainable from any practical experiment) as the limiting case of vanishing heat transfer and obtaining Y+ in terms of U+ are good examples of his unconventional out-of-the-box thinking. He has an intuitive feel for the importance of the existing ideas to his goals and he is able to boldly adapt and built upon the work of others. He also has a tremendous knack of expressing his ideas in clear and cogent terms to reach a wide audience of different backgrounds. He developed a clear methodology to express heat and mass transfer concepts and he can be credited to some extent for unifying the terminology and language used by chemical and mechanical engineers which was different before he arrived on the scene.

CONVERGENCE OF OUR PATHS: 1965 - 1975

In 1965 Spalding occupied the Chair, Professor of Heat Transfer, at Imperial College (Now Imperial College of Science, Technology and Medicine). He was appointed to this chair in 1958 when it was created. He also headed the "Thermofluids" Section of the department which was later renamed Computational Fluid Dynamics Unit. Though digital computers had been around for a couple of decades, early 1960's coincided with the 'advent" of the computer as a widely available tool and led to the developments that eventually gave rise to what is today known as CFD.



I graduated with a B. Sc in Engineering in 1964 and in 1965 won an ICI scholarship in India that gave me the choice to go to any college in the UK for my Ph.D. I decided to work on drying of sprays – a subject of much interest to ICI and other companies - that involved both heat and mass transfer. Since Spalding was one of the most respected researchers in heat and mass transfer, I wrote to him to accept me as his Ph.D. Student. The essence of his reply was: "I am not interested in working on drying of spray paints, but I am happy with last year's ICI scholar – Suhas Patankar – so I will accept you as my student and we will figure out what to do once you get here". I guess I have to thank Suhas for working hard! I suspect another reason may have been his soft corner for an ICI scholarship since he himself had completed his Ph.D. under an ICI Fellowship.

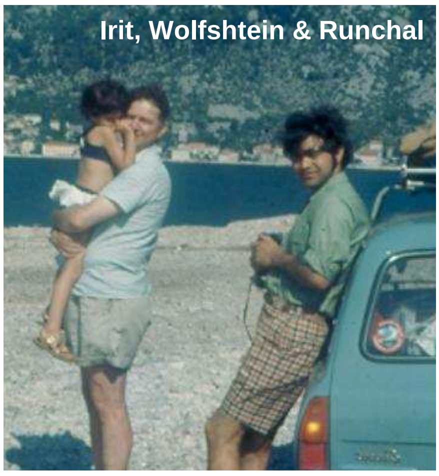
Once I got to London, Brian was busy with his "Unified Theory". This was his "grand" design built upon the insights of Taylor [Morton et al., 1956] to have a single theory that covered Boundary Layers, Wakes and Jets. At that time his approach was to use profile methods except that he proposed piece-wise profiles that could approximate – to a given accuracy - any "ideal" profile that might describe the flow. He had been working towards it with a series of students and had had fair amount of success. His previous students had already determined "optimal" entrainment functions, log-law constants and heat and mass transfer resistance required to describe a wide range of flows. Patankar had had a fair amount of success on the theoretical side in building a general purpose "integral-profile" computer code based on piece-wise linear segments.

Spalding was at that time confident that most flows of engineering interest can be represented by his “Unified” method and piece-wise profiles. Shear plays a key role in separated flows, including those where the boundary layer is destroyed by, say, an adverse pressure gradient, or a geometry that induces separation. Brian therefore asked me to extend his Unified Theory to such flows. Flow behind a Backward Facing Step (BFS) in a channel and that in a Driven-Lid Square Cavity were to be the focus of my attention. Micha Wolfshtein had already joined the group in October 1964 and Brian had asked him to tackle the problem of the Impinging Jet on a Flat Plate. These extensions would have firmly established the Unified Theory not only for “parabolic” flows such as the boundary layers but also for “elliptic” flows with strong pressure gradients, recirculation and impingement.

I asked Spalding what classes I should enroll in. His answer, and it illustrates Spalding’s practical and single-minded unconventional approach, floored me. He told me there was no need to take any classes; he wanted me to concentrate on my research. This to a student who had freshly completed his undergraduate studies from some university in another country and enrolled in a Ph.D. program without any master’s level degree! He asked me to do a thorough literature study on analytic and approximate methods for boundary layers, wakes and jets. I wonder how many of today’s professors would wish that they had that freedom with their graduate students!

I started reviewing the published literature and summarized well over a 100 papers and also started using the piece-wise profile method program to solve the BFS problem. I also discussed the problem in detail with a few persons in the mathematics department of the Imperial College (who advised me to quit and find a more worthwhile career since the greatest mathematicians had failed to solve the Navier-Stokes equations with turbulence!). During the course of this work, I began to come across papers that used Finite-Difference methods which were similar to the idea of Unified Theory in terms of piece-wise profiles but had a distinctly different flavor in terms of implementation. I was surprised to learn that the Finite Difference methods for Navier-Stokes equations had been around for a long time: Thom [1928] had used them well before the advent of the electronic computers.

After about 4 months, I had no success in predicting BFS with the Unified Theory and started to have doubts that something was not right. I asked for a meeting with Brian and briefed him on my progress with the literature review and told him of my failure to make any headway with the Unified Theory for separated flows. I also told him that I had seen papers that used Finite-Difference methods which seem to have reported success with separated flows at low Reynolds numbers. Brian was quick to arrive at the key conclusion that profile methods as used at that time had a “fatal” flaw. There was no easy way in the Unified Theory to represent the role of the axial diffusion terms that played a key role in “separating” and “reversing” the boundary layer. So the profile method solution procedure needed to be modified. It quickly became apparent that it will be simpler and more general to use the Finite Difference (FD) method rather than modify the Unified Theory. I started working on writing an FD computer program. Soon I started reporting success in solving the BFS and DSC problems.



At one of our subsequent meetings, Brian mentioned that Wolfshtein was reporting success along similar lines and asked us to get together. We soon realized that we were essentially using the same approach – just the problems we were working on were different and that we had approached the problem from different view points – one from a high-Reynolds number and the other from a low-Reynolds number viewpoint. We realized the limitations of the violation of the positive-definite matrix coefficients and the consequent numerical instability for high-Reynolds number flows. Brian then made an analogy with how the wind from the north always brings cold - or that from the pigsty always stinks. I guess Brian knew this because he had grown up close to a pigsty. These discussions led to Brian proposing the "upwind" concept. Brian also made an important physical analogy of likening the upwind and FD method to a series of tanks (control volumes) and tubes (grid). Though we did not realize it at the time, this later led to the formalization of the "Finite Volume" concept. With these two changes we were soon "free" of the Reynolds number constraint and the tank-and-tube analogy changed our approach to thinking in terms of fluxes rather than the state variables. Once formalized, this eventually frees one from the limitations of the Taylor's Series and equating "order" with 'accuracy". With these changes we started assembling a "joint" Navier-Stokes solver hammered out of our specific requirements. Like many other codes of that era, this was based on the stream-function and vorticity (ψ-ω) variables.

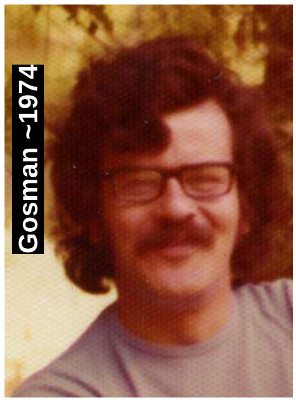
One can see the beginnings of the FD in what Brian was doing with his unified method. Instead of using "piece-wise" polynomials to construct a local value to convert the differential equations directly to algebraic equations (as in FD), he was using the "piece-wise" polynomial to represent a set of values (profile) and then integrating the differential equations to obtain the algebraic equations that will give the values of the constants of the profile. But in his characteristic fashion he used his insight to invent a "physical" rather than a "mathematical" approach to the problem. With his insight, it was easy for him to see that the focus of interest should not be "variables" but their "fluxes". With his engineering background and extensive work on the usefulness of the "control volumes", he quickly came to view each "node" of a finite difference grid as an independent "tank" which exchanges "fluxes" with other tanks by "tubes". Brian's re-invention of the upwind scheme similarly had a "physical" insight into the mathematical approach. Once the focus is fluxes, upwinding is straightforward: fluxes come from somewhere; they have a distinct speed and flow in a certain direction.

Soon thereafter Brian sent a paper by Barakat and Clark [1965] for my review and we could see that they had "upstaged" us on the upwind "discovery". Also Burgraff [1966] had published his now classic paper on square cavity where he reported success at low Re numbers but had failed to obtain solution beyond Re=400. We thought we should publish our work before we were trumped up by another claim. In our new-found enthusiasm, we were blissfully ignorant of the pitfalls of upwind. This led to our first papers on finite difference methods with the IC approach [Runchal, 1967, Runchal and Wolfshtein, 1969]. The second paper is also a good example of why not to publish a paper in a hurry since it contains results for Re=1000 for driven square cavity which were proven to be wrong. Little did we know that upwind and one-sided differences had been around far longer. A paper by Courant et al. [1952] had used upwind concept more than a decade earlier and mathematicians had extensively explored the properties of one-sided and central difference methods for far longer. However in those days the interaction between mathematicians and engineers was somewhat limited.

Later on we started becoming wise to the pitfalls of upstream differences and this led to some work on "numerical" diffusion. Wolfshtein [1968] published a technical note where he showed that false numerical diffusion is related both to the speed of the flow and the angle of the stream-lines to the grid. Spalding [1972] proposed an exponential method to replace "upwinding" but eventually we settled on a "hybrid" method [Runchal, 1972] that automatically blended the Central and upwind difference methods based on local Peclet number.

Around 1967, I became fascinated with the Gauss Theorem and the integral approach to derive the algebraic analogue for the Navier-Stokes equations. I had by then heard of the classic “fight” between the “differential” approach of Newton and the “integral” approach of Leibnitz. Wolfshtein and I had many discussions over the competing approaches and he correctly pointed out that the same set of algebraic equations can be derived from either. He eventually went on to write his thesis [Wolfshtein, 1968] in terms of Taylor Series whereas I submitted mine with the integral approach which is now more commonly called Finite Volume Method. Wolfshtein and I have continued to compare notes on the relative role and importance of the two approaches. Though we are both aware of the usefulness of each, we have continued to favor our original choices as a basic methodology to arrive at an algebraic analogue of the transport equations.

By mid 1968 both Micha and I had completed our thesis work. I took a hiatus and went to spend a long and productive summer in Cambridge (MA) to consult with Northern Research on the application of CFD to aircraft compressors. It also helped with my meager student’s finances – as a bachelor I had expenses that my married friends like Suhas and Micha did not have. I came back from Cambridge in September 1968 and Brian sprang a surprise on me. He informed me that London University did not grant a Ph.D. in Engineering solely on the basis of theoretical work! I took over the experimental rig of David Gosman who had just finished and modified it to measure flow behind a BFS at very high Schmidt numbers. Though I resented having to do experiments at that time, I wish today that that rule was in force universally. It taught me the respect for experimental data, its inherent uncertainties and the vagaries of the instrumentation. It also taught me that the “real” world of fluids is inherently unpredictable, never two-dimensional and never steady. I completed my experimental project in late 1968 and submitted my thesis [Runchal, 1969] to London University.



By the end of 1968, Brian had realized the potential of the developments that had taken place. He decided to organize a Post-Experience Course at Imperial College in 1969 targeted at both academic and industrial communities. Academic Press became interested in publishing the work done by the group. Both Micha and I were leaving Imperial College and Brian asked David Gosman who had just completed his thesis on experimental work to edit the book, and Sam Pun – another of his recent Ph.Ds - to take over the computer codes from me and Micha. That code, called ANSWER, made it to the book on CFD [Gosman et al., 1969] (Brian had a strict rule that all joint publications carry the names in alphabetic order). At the same time (1969) Brian incorporated CHAM Ltd. – that then stood for Combustion Heat and Mass Transfer, Ltd.

If I have to pick a date for the “birth” of CFD then 1969 was the year that ushered in the CFD as an engineering tool. The work done at Imperial College on Navier Stokes equations, and the computer codes to solve these equations together with generalized transport equations for any two-dimensional flow, became widely available in 1969 through the publication of the book (Gosman et al., 1969). The Post-Experience Course at IC in 1969 reached a large number of researchers in the U.K. and later abroad through a series of courses and seminars at various universities in the US and Europe. At the same time commercial services in CFD became available through CHAM in 1969. It should also be noted that the first conference with CFD at its theme was held at Monterey in 1968.

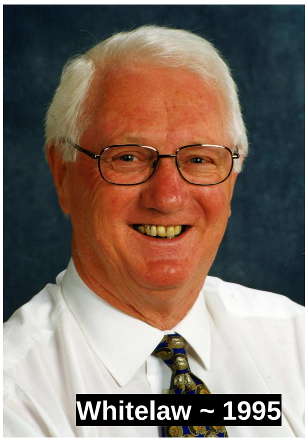
In late 1969, I accepted a teaching position at IIT Kanpur in India. I briefly returned to Imperial College in the summer of 1970 and then came back to join as Senior Research Fellow in 1972 and worked as Technical Director of CHAM till 1974. CHAM was the only consulting company providing commercial services in CFD and it stayed that way till Creare, Inc. started with Fluent software which eventually became Fluent Corporation in 1983. Creare had acquired the software from Prof. Jim Swithinbank of Sheffield who in turn had acquired it from Imperial College.

REFLECTION ON THE CFD DECADE AT THE IMPERIAL COLLEGE: 1965-1975

Whitelaw & Launder

The decade between 1965 -1975 was a very fertile period for CFD. These were the heydays of "CFD" at Imperial College. In retrospect it was a unique and amazingly productive period. The group under Spalding included two young and dynamic faculty members: Jim Whitelaw and Brian Launder. This group of three, working symbiotically, transformed the theory and practice of fluid mechanics. Spalding working with his students and associates transformed the emerging field of computational fluid dynamics from an esoteric and mathematical branch of science to a fully developed tool for practicing engineers. Whitelaw and his students turned the emerging Laser Doppler Anemometry into a proven and preferred experimental method for measuring flows. Whitelaw worked on the experimental side and many of his students used the CFD methods to verify their experimental results. Brian Launder and his students were active in the field of Turbulence. They went on to make significant contributions in the theory and experiments of turbulent flows. All three sub-groups used CFD and experiments in a highly synergistic manner to advance the theoretical and experimental knowledge base of Fluid Dynamics. By 1969 the CFD group consisted of more than 30 researchers and there were weekly seminars mostly given by a member of the group. To my knowledge it was the largest CFD group in the world at that time. I wonder if even today there is a larger group of researchers focused on CFD under the guidance of a single person. Though significant CFD work was going on at various locations around the world, the only other large group at that time was the T3 at Los Alamos National Laboratory under Frank Harlow.

Launder - 2007



Whitelaw ~ 1995

Harlow's group worked on a wide variety of problems in fluid dynamics. His focus was exclusively on transient flows with steady state as an asymptotic state of the flow. Many applications involved compressible flow or free surface. They often involved moving boundaries and multiple phases. His focus was more on the "physics" and "science" of Fluid Dynamics. The primary focus of Spalding's group was on "engineering" flows of interest to the industry. Most of these flows could be treated as steady and incompressible – at least to a first approximation. Moving boundaries were not of much interest. Multiple phases were approached by the IC group in an ad-hoc manner or as equivalent single-phase with approximations such as a void-fraction. Compressible and transient flows were treated as "extensions" of the steady, incompressible flow. These philosophical and practical differences had a profound effect on the "world-view" of the two groups and their approach to CFD. Harlow's approach was by far the more rigorous and often stayed closer to the physics of the problem. Los Alamos at that time had some of the most sophisticated computational resources in the world. The computer resources generally affordable by the industry were significantly limited. It was computationally expensive to use the Los Alamos

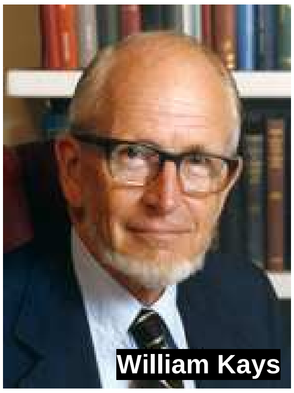
Frank Harlow

methods developed for transient flow to compute the steady state flows. The computer programs developed at Los Alamos were available as listings in technical reports or on personal request. Harlow made little attempt to distribute them to outside researchers. He was more interested in innovative research than in disseminating his technology or spending his time teaching others how to use it. The excellent and path-breaking work done at Los Alamos was not widely known outside a select research community. It was not till Tony Hirt became leader of the T3 Group, around 1973, that computer programs developed at Los Alamos became generally available to outside researchers though the U.S. Department of Energy distribution sites.

With Spalding's focus on engineering application, he looked for alternatives and tools that will allow his methods to work efficiently with limited computer resources. Computational economy was a major concern and a driving force. He often made bold assumptions and used his keen insight to separate the essential from the inconvenient. The technology developed by his group was made widely available through personal contacts, a post-experience course, distribution of the computer programs, and publication of books. It is important to note that Spalding has always emphasized that a poor solution is better than no solution. It is countered by some that no solution is better since it will not lull one to the dangers inherent in a poor solution. However Brian has shown that with insight, some caution, and testing against empirical data, one can obtain useful engineering information from an approximate solution even though one is aware of the shortcomings inherent in it.

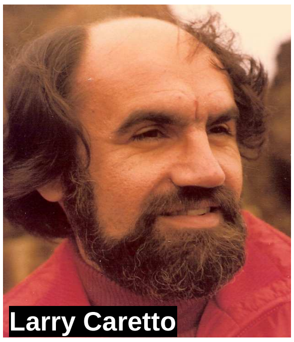
Around 1970 Brian became convinced that the ψ-ω approach had no distinct advantages for 3D flows. He was quick to abandon it and turned to primitive variable form of the Navier-Stokes equations. Characteristically again, he used the available tools and technology to create something entirely new and useful for engineers. Just as he "unified" the boundary later, heat and mass transfer concepts for his Ph.D. work, he set about to change CFD by combining existing concepts with key insight and bold assumptions. By this time Harlow [1965] had introduced a staggered grid and a decoupled pressure based on continuity equation for transient computations, and Cholesky [1967] had pointed out that any scalar can be used in lieu of pressure. The key advancement of splitting the pressure contribution into two stages was obvious since Patankar and Spalding [1967] had already used this approach successfully for parabolic flows where the axial pressure can be decoupled from the component that governs the cross-axial velocities. Patankar and Spalding [1972] combined these insights and arrived at the SIMPLE algorithm that revolutionized the CFD practice. The depth of that insight and achievement can be gauged by the simple fact that most of the successful commercial CFD codes even today employ SIMPLE or its variations at least as one of the available options.

Brian knew that from the point of view of practicing engineers, CFD will not be a really useful tool unless it dealt with the intractable problems of turbulence and chemical reactions. With his deep background in the physics and theory of flows (and his fluency in German and Russian) he built upon the work of Kolmogorov [1942], Prandtl [1945], Chou [1945] and Rotta [1951]. However the equations derived by these researchers were so complex, and so little information was available about the attendant constants that no attempt had been made to solve these equations of turbulence. Spalding was one of the first to realize that, with the availability of the digital computers, the set of equations developed by them could form the basis of practical predictive tools if one could derive the constants needed to quantize these equations. He turned to getting these constants from experimental data with bold assumptions about the "universality" (or better – usefulness) of these constants. This led to breakthroughs such as one of the first k, k-l and k-ω methods and the eddy break-up method for turbulence-kinetics interaction. About the same time Harlow and coworkers [1967] independently had come to the same conclusion and published their first paper on a 2-equaion (k-ε) model of turbulence. Working with Launder and others, Spalding adapted the k-ε method as a "preferable" tool primarily due to the computational advantages related to the fact that the so-called diffusion coefficient was more likely to be a constant for "ε" than for "l", "ω" of formulations that were being investigated. Another reason was the ease of interpretation of "ε" compared to other variables; it was simply equivalent to the energy dissipation near the wall. It was during this period that turbulence modeling became established as a practical tool. Spalding worked extensively on turbulent flows for a while but then moved away to concentrate on other fields of more immediate interest to him.

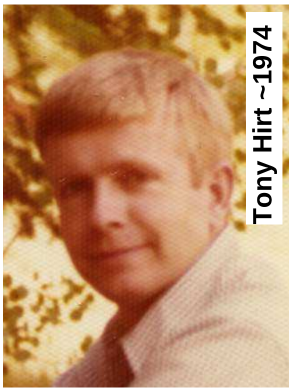

William Kays

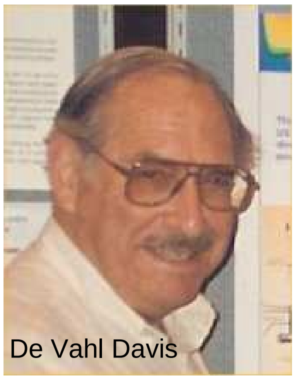

Frank Schmidt

During this decade, the Imperial College group had distinguished visitors. These included J.C. Rotta (Goettingen), Frank Schmidt (Penn State), C-L Tien (Berkeley), William Kays (Stanford), Joseph Kestin (Brown), P.D. Richardson (Brown), Bill Reynolds (Stanford), Philip Saffman (Caltech), Peter Bradshaw (NPL), David Pratt (Washington), Larry Caretto (Berkeley), Graham de Vahl Davis (New South Wales), Tony Hirt (Los Alamos), Harry McDonald (United Aircraft Res. Lab.), David Dyers (Alabama), and many others.

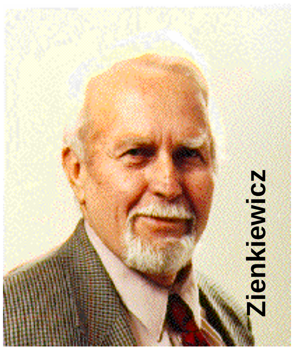

Larry Caretto

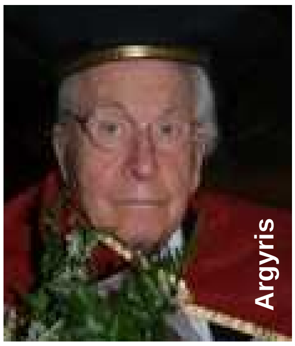

Bill Reynolds


Tony Hirt ~1974


De Vahl Davis

These visits and interactions were very valuable in dissemination of the IC CFD technology and its wide acceptance. Following a conversation with Jim Whitelaw at the 1970 International Heat Transfer Conference in Paris, Frank Schmidt organized a series of short courses at Penn State that were delivered by IC faculty and research staff. The courses started in 1972 and continued till 1994 and covered a number of subjects including computational methods for boundary layers, re-circulating flows, combustion and turbulence. These courses were along the line of the 1969 Post-Experience Course at Imperial College and were directed at both academic and industrial communities. Frank Schmidt, Jim Whitelaw and Brian Launder also arranged a series of very successful conferences on Turbulent Shear Flows starting in 1977 that in a modified form survive to this day. Starting around 1970, Bill Reynolds and William Kays were instrumental in arranging a number of "Olympiads" where competing researchers presented the results from their computational methods for boundary layer flows and turbulence. The methods were then formally "judged" in terms of agreement of the predictions with experimental data. These activities led to the IC CFD technology being widely known around the world.


Zienkiewicz


Argyris

The IC group also interacted with Argyris and Zienkiewicz about the merits of different approaches to CFD and their espousal of the Finite-Element (FE) method. Though we could see that there were some advantages to the use of FE method, the overwhelming feeling was that the method was unsuitable for high-Reynolds number flows and lacked clear theoretical basis (since the Hamiltonian does not exist for non-linear systems).

This, at least to some extent, has been vindicated by subsequent developments since most of the current commercial technology has adapted FV approach. It must be stated, of course, that over the years, both approaches have borrowed ideas from each other. Most visibly, the FE has moved away from minimizing a Hamiltonian and has implemented "upwind" methodology whereas the FV has adopted the FE approach of unstructured and boundary-fitting grids. With today's technology, the differences between the FE, FD and FV are more of semantics than of substance. It can be shown that all three, with appropriate assumptions, can lead to identical algebraic equations.

The Missed Opportunities

The single-minded and focused approach followed by Spalding had some drawbacks. Brian has a distinctive trait that once he is convinced of the usefulness of an approach, he is able to completely focus on the path that lies ahead and completely ignore any idea that might sidetrack him from that path. This is a common trait of genius and of high achievers. However the drawback of this approach is that sometimes "off-the-path" ideas lead to "greener" pastures. Of course one may also waste a lot of effort in looking for greener pastures.

During this period Brian's group explored and discarded many ideas that in hindsight would have proved fruitful. Late in 1960s we experimented with and abandoned what later became Vector-Differencing because it did not conserve "extrema". Since "hybrid" scheme was working reasonably well we did not make any attempt to find a "limiter" for this scheme. Raithby [1976] found a way to make Vector Differencing a practical option. The single-mindedness of the group was also responsible for the premature abandonment of the SIVA algorithm. This was a coupled solver [Caretto et el., 1972] for primitive variables but was abandoned because of the focus on the SIMPLE algorithm. It was subsequently shown by others [including Vanka, 1986] to be a superior method for a class of strongly coupled flows. Yet another example is the early abandonment of the co-located grid [Runchal, 1971] because it was felt that co-located grids offered no distinctive advantage over staggered grids. Subsequently Rhie and Chow [1983] perfected the co-located grid which is today a preferred option for unstructured grids and offers distinct advantage for complex geometries. We also failed to fully explore the impact of numerical diffusion and truncation. Wolfshtein [1968] did some tentative work on the subject but it was left to Hirt [1968] to produce a formal and heuristic method to define these effects.

Another important example of a missed opportunity was the "vorticity-fluctuation" method (k-ω) for turbulent flows. This obviously is a more elegant and intuitive representation of turbulence than a dissipation-based approach (which requires dissipation to be transported!). After early and extensive exploration of it, the focus shifted and stayed with the k-ε model because it was felt that the diffusion coefficient for the dissipation equation was easier to define. It was Saffman [1976] and Wilcox [2006] who eventually went on to establish it as a viable and preferable tool for certain class of flows. Of course, as is clear from the work of Rotta [1951], it can be shown that all two equations models are identical in that the same differential equation governs all such models except that they differ in the source terms and may have distinct numerical properties.

The Post- 1975 Period

Spalding stayed at Imperial College till 1988 when he retired to devote his full attention to CHAM and development of the PHOENICS code that CHAM had been marketing for over a decade. PHOENICS which debuted in 1978 was the first commercially available software tool in CFD. At this time the only other widely available CFD tool was the TEACH code from Imperial College which was severely limited in its scope and capabilities. PHOENICS provided a general framework for solving any problem within its scope and allowed users to extend the capabilities of the code through a formal framework that was included in its design.

He subsequently went on to invent highly useful and simple tools and techniques such as the IPSA Inter-Phase Slip Algorithm, IPSA, [Spalding, 1985] for predicting multi-phase fluid flow, a simple algorithm [Spalding, 1994] to determine the wall distance for complex geometries, (needed for many turbulence and radiation computations), a multi-fluid approach to turbulence. [Agonafer et al, 1996], a multi-fluid approach for turbulent combustion [Spalding, 1996], a novel approach for integrated radiation computations [Spalding, 1996], and a methodology to unify fluid and solid mechanics [Spalding, 2005]. He integrated all these diverse phenomena and brought them within the scope of CFD. All these are examples of Brian's "physical" approach to develop practical tools. These produce approximate and plausible results even when one knows that the actual physics is more complex but also more intractable.

"Unification" has stayed the primary goal of Spalding from his Cambridge days in early 1950s to the present. He has now added structural dynamics, turbulence and multiple phases to his plate. Brian's approach to research is always "intuitive" and "physical" as distinct from "theoretical" or "mathematical". He strongly believes in the "art of the possible" and is not encumbered by theoretical limitations that may exist. The question that Brian always asks is: "Can this be useful to a practicing engineer".

SYNOPSIS

So did Brian Spalding invent CFD? The answer is obviously not an unqualified yes. Origins of CFD can be traced as far back as 1928 or perhaps even earlier. Harlow at Los Alamos, and others in academia and research organizations, had already contributed immensely to what would later become CFD. They had already explored many of the key concepts that would be used or re-invented by the IC group. Spalding did not even coin the name. Formally the name was first used by Pat Roache [1972] in his famous book. But let us ask another question. Would there be CFD as we know it today without Spalding? And the answer is an unqualified No. More than anyone else, he created CFD as an Engineering Tool – the application of CFD to problems of interest to engineers. Most of today's commercially available CFD software tools trace their origin to the work done by Spalding and his group in the decade spanning 1965-1975.

He was an active leader and the key contributor of novel ideas that led to the development of CFD methodology that was efficient and "engineer-friendly". His key insights during this decade often made breakthroughs possible and re-directed the focus at critical moments. He has had over 100 graduate students. Many of these students have gone on to make significant professional contributions in their own right. If you consider the period between 1965-1972 when both Launder and Whitelaw worked closely with Spalding, the number of students who carried the torch of "IC" methodology is truly amazing for a group under the close supervision of a single person. Many of these students have gone on to make a distinctive mark on the world stage. Many well-known names in CFD, turbulence or combustion today have a 1st or 2nd generation IC connection.

The fact that any vector can be expressed as a rotational (vector) and irrotational (scalar) components has been known for ages. That pressure is related to the scalar component and vorticity to the vector is part of classical hydrodynamics. Harlow and Chorin independently suggested that we decompose velocity correction into a vector and an arbitrary scalar stage. Harlow's group used it to turn the continuity equation into an equation for pressure. But it was an inefficient mechanism for steady flows – of routine interest to engineers - since it required transient solution. It took the work of Patankar and Spalding (1972) to weave it into an economical and practical tool that could also be used for steady flows – SIMPLE. So we can not credit Spalding for pressure projection methods but we should recognize the manner in which he used and turned it into a practical and viable alternative for steady state flows.

Take another example. Finite-differences have been around for ages. Mathematicians have played with and described discrete spaces, Banach, Hilbert, etc, for ages. Long line of researchers – Thom [1928], von Neumann [1944], Southwell [1946], Courant et al. [1952], and others – applied it to fluids. Spalding started with the same tools, quickly realized the limitations of FD – and gradients-based Taylor Series - in dealing with non-linear equations that span from elliptical to hyperbolic. He could see the connections between the mathematics as it was written and the real physics where "quantities" (fluxes, heat etc.) move in a conserved manner. This eventually led to the FV approach. It turned out the IC group was not alone in using the FV approach; Harlow's group had essentially thought in FV terms and expressed their equations in terms of fluxes and conserved quantities. However they never expressed it by a simple visual analogy such as a "Tank and Tube". Neither did they resort to formal integration around a control volume for their published work. Edwards [1968] had used the essentials of the FV and (unique at that time) unstructured grid approach in a code called TRUMP at Lawrence Livermore starting in the mid-1960s. But I bet most CFD researchers, even today, do not even know of Edwards work. I certainly had not heard of him till 1978. So just like "upwinding", the FV was perhaps re-invented and Spalding is the one who should be credited for the wide-spread use of this technology.

More or less the same story repeats with the k-ε model. The basic equations were written down by Kolmogorov [1942] and Prandlt [1945]. Rotta [1951] had added to the knowledge in the 1950s and elaborated on the concept of length scales and frequencies. Davidov (1961) seems to have been the first to formally derive and propose a closed form equation for ε as part of a very complex 3rd-moment closure. Harlow and Nakayama [1968] developed an epsilon equation and demonstrated its use in a 2-equation eddy viscosity scheme for a few flows. Hanjalic, then a Ph.D. student with Launder, knew of the difficulties being experienced by his IC colleagues in using k-l and k-kl based variables to predict both wall and free flows. He adapted the coefficients in Davidov's epsilon equation, tested it first in a k-ε eddy-viscosity formulation and found the same form could mimic both wall and free flows. He then embedded it in a 3-equation model (k-ε-<uv>) to predict flows where shear stress and strain vanished in different locations (Hanjalic, 1970). Bill Jones (Jones & Launder, 1972) extended the k-epsilon model so that it could be applied across the sublayer right to the wall (enabling situations where the sub-layer was not universal to be predicted) while others made further contributions. Spalding till then exploring, k, k-l, k-kl, and k-ω models could see the advantages of a "standard" model and the order that it will bring to future research. He dropped further work on other models, and the IC group adopted k-ε as the model of choice. This resulted in the Launder and Spalding (1972) book on the subject and the seminal review of Launder and Spalding [1974] that led to the formalization, "standardization" and acceptance of the k-ε model as a general tool for engineering practice.


Kemo Hanjalic

It was due to Spalding's insight and single-minded focus on practical tools that these techniques became established and useful to engineers. He focused the attention away from the mathematics and to the physics of the phenomena and application to practical problems. In my view it will be true to say that Brian did not invent the "science" of CFD but he is still the person who honed the "art" and "technology" of CFD for engineering design and practice. If it was not for him, CFD may have stayed an esoteric science practiced in academic and research organizations.

Brian has worked in diverse fields of engineering and science over his careers. He has made seminal and enduring contributions in combustion, turbulence, heat and mass transfer and CFD. If you look specifically and only at CFD, Brian's contribution can be seen in inventing (or re-inventing):

1. Finite Volume Methodology
2. Unification of all 2^{nd} order convective-diffusive systems by a generalized transport equation
3. Upwind Numerical Scheme
4. Pressure-Projection
5. Staggered Grid
6. K-ε Model for Turbulence
7. Focus on turbulence energy for wall heat transfer
8. Eddy-Break-Up model for turbulence-kinetics interaction
9. IPSA for multi-phase flows
10. 6-Flux model and IMMERSOL methodology for radiation.

He was the first to formally propose that, in the context of numerical solution, all 2^{nd} order transport equations can be expressed and solved as a single generalized transport equation. Thus to some extent, he has achieved his life-long goal of "unifying" the treatment of fluid flow, heat and mass transport, and mechanical stresses by a single "universal" method in a "unified" manner. His other work on turbulence, multi-phase, solid-fluid interaction and wall distance computation has not yet seen the same popular adaption as these but he has pointed to important path-breaking research that may bear fruit in the future.

In Newton's words: Spalding stood on the shoulder of giants and saw farther than his peers. He foresaw that unifying flow, heat and mass transport will lead to practical tools for engineers. He foresaw that looking at the physics rather than the mathematics will lead to wider acceptance of CFD tools. He foresaw that CFD - once turned into to a design tool – would revolutionize engineering. He foresaw that there were commercial opportunities in CFD.

This pattern of "innovation" by "unification" and "adoption" of other's work with "bold insights" are the common and recurrent themes of his professional career. If I were to characterize one common distinctive thread in Spalding's thinking, it is that he thinks "physics" rather than "mathematics". Though his key contributions involve complex mathematical concepts, his breakthroughs have come because he looked at the physics behind those concepts. He was able to see the "trees" because he refused to get lost in the forest of mathematical equations and looked at the processes these equations were trying to express. He thinks like an engineer. He repeatedly asks the question: "What can I do that will help solve a given problem". To me this has been his key contribution above all. He labored on with "gaps" in our knowledge about 2-phase, turbulence, boundary conditions and such like. Each time he came across an "obstacle" he found an engineers' solution. If it worked – but was theoretically a bit shaky – it was good enough to get the job done. He proposed things like eddy break-up for combustion when there were so many gaps in our knowledge that no one thought you could attempt combustion with CFD!

Brian to me is the best example of Richard Feynman's famous saying: "*What Do You Care What Other People Think?*" He feels that his most distinguishing trait is that "he did what he did because he didn't know any better". He quotes Virgil: "Possunt quia possunt videntur" (They can because they think they can). He would like to change it to: "They can because they don't know they can't." At many stages in his life he simply did not know (or more probably, did not care) that others had already declared a problem to be impossible or too ill-posed to be solved. And they had gone on to prove it by elaborate mathematical and physical arguments. He simply found a way to devise a practical solution to deal with the essence of the problem.

Brian has immense intellectual capability to grasp the essence of a problem and suggest a resolution. More than once when I brought a problem to him that I was struggling to grasp, I found that he could see the underlying essence while I was having trouble enunciating it! Brian has a tremendous capability to not only generate new ideas but to bring out creativity in others who work with him. Other than his towering intellect and his keen insight, Brian has a single-mindedness that has allowed him to focus on the problem of immediate interest and achieve so much in his professional career. When Brian in focused on a problem, he is able to ignore everything else that is not immediately relevant to his purpose. I would dare to say that Brian looks at the world as divided in to two groups: those he can work with and those not relevant to his purpose.


Brian & Colleen - 2008

Here he is at his 85th Birthday – the way I remember him from some of my previous times with him: a glass in his hand and attentively listening to a lovely girl by his side.

Happy Birthday, Brian.

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