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Brian Spalding: CFD and reality – A personal recollection

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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 was associated with, and was 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 paper is a personal recollection of 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 paper also explores the opportunities missed by the IC Group during this decade of break-neck progress in CFD.

1. 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 Deep Brain Stimulation – 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 (Fig. 1) was born on 9th January 1923 in New Malden in the now picturesque suburbia of London. The town finds mention in the Domesday Book as Meldone and seems then to have been a typical rural English village. The fact that Brian grew up close to a pigsty in a semi-rural environment will later lead to a remarkable professional observation. Spalding attended Kings College School from the age of 9–18 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. Great Britain did not yet have any rockets at that time; RPE was set up to develop the technology in response to the success of the German V2 Missile. Soon thereafter Brian was dispatched 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 10 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 the mentor of the German team led by Johann Schmidt. Indeed he lived with the German team 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). An unanswered question remains if this transfer had anything to do with Brian’s membership in the communist party during his student days. This was the beginning of the cold war and the Burgess and McLean affair hit the news shortly thereafter in 1951. From the point of view of Brian’s career, 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 stood 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 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.

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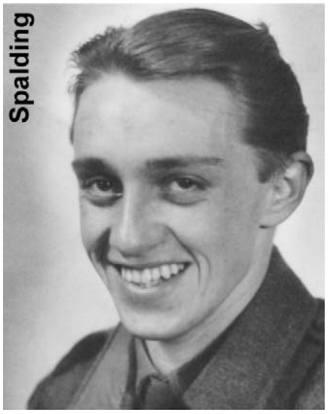

Fig. 1. Brian Spalding.

In 1948 Brian got an ICI (Imperial Chemical Industries, Ltd.) Fellowship to go to Cambridge University (Pembroke College) for a Ph.D. With his RPE background, he knew that he 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 early on demonstrated his independence and his tendency to go out on a limb. He had a fairly definitive idea of what he wanted to do – and it was not diesel engines. When Bird tried to get Brian to use an existing experimental rig, Brian protested to Baker. Baker 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. Having worked at RPE, Brian already knew more combustion than his supervisor and wound up advising Robinson on his research. Brian's previous experience at RPE and NPL came very handy for his Ph.D. research which was remarkable in that it was entirely self-motivated. Brian obtained his Ph.D. in 1952 with Will Hawthorne and E.S. Sellers as his examiners.

2. 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 [1]. It is a remarkable thesis in that it ‘‘unified" the key hydrodynamic concepts of von Karman [2], the heat transfer concepts of Kruzhilin [3] and the mass transfer concepts of Eckert [4]. 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 [5], and Semenov [6], 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 [7]. His other notable contributions in combustion include the 'centroid rule' [8] 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 [1], and a cooled porous burner for measuring flame speeds [9]. He also developed an electrical analogue of combustion [10]. 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. Spalding 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 Convective Heat and Mass Transfer [11] that has greatly influenced subsequent work in this field.

In late 1950 s Spalding turned his attention to the important issue of the role that wall shear plays in most engineering flows. 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 an 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 'wall law', covering viscous, transitional and logarithmic regions [12]. He was also not 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 [13]. This was his ‘‘grand" design built upon the insights of Taylor [14] to have a single theory that covered Boundary Layers, Wakes and Jets. 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. [15–17]). Soon thereafter, Spalding came to the conclusion that instead of searching for an optimum profile, one can ‘‘universalize" a profile by simply representing it as a set of piece-wise polynomial – or linear – segments and derive the weighting functions from the governing initial and boundary conditions. This freed one from having to find an ‘‘ideal" profile to fit a given flow. However it later became apparent that Spalding's search for a unified theory was not yet over since this approach was found to generate solutions that were occasionally spurious or even singular.

3. Convergence of our paths: 1965–1975

In 1965 Spalding occupied the Chair, Professor of Heat Transfer, at Imperial College. He was appointed to this chair in 1958 when it was created. He also headed the ‘‘Thermofluids" Section 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 (Fig. 2) 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 wanted 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 sprays, but I am happy with last year's ICI scholar – Suhas Patankar (Fig. 3) – so I will accept you as my student and we will figure out what to do once you get here". 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.

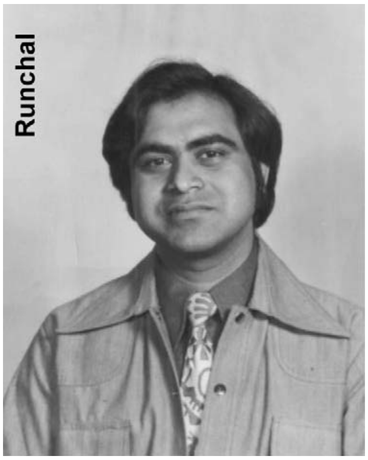

Fig. 2. Akshai Runchal.

Fig. 3. Suhas Patankar.

Brian was then busy with his ‘‘Unified Theory’’. At that time his approach was to use piece-wise profile methods that could approximate – to a given accuracy – any profile that might describe the flow. 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 Theory and piece-wise profiles. So far he had not been able to extend his Unified Theory to separated flows or flows with strong pressure gradients. 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 (DSC) were to be the focus of my attention. Micha Wolfshtein (Fig. 4) 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 – a flow with strong favorable pressure gradients. 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.

Fig. 4. Micha Wolfshtien.

A personal example from this time illustrates Spalding’s practical and single-minded unconventional approach. When I asked him what classes I should enroll in he told me there was no need to take any classes; he wanted me to concentrate on research and asked me to start with a thorough literature study of analytic and approximate methods for boundary layers, jets and wakes. This is remarkable in the context of a student who had freshly completed only his undergraduate studies from a university in another country and was enrolled in a Ph.D. program without a Master’s degree or any other professional experience. I wonder how many of today’s professors would wish that they had that freedom with their graduate students!

After a thorough review of the published literature I started using the piece-wise profile method to solve the BFS problem to extend the Unified Theory to elliptical form of Navier–Stokes equations. It is notable that the dominant thinking in the 1960’s was that there was no possibility of solving the Navier–Stokes equations with turbulence except for highly simplified one or two-dimensional flows. So for Spalding to even attempt to extend the Unified Theory to separated flows is remarkable in itself. During the course of this work, I began to come across papers that used finite-difference methods which bore a similarity to the Unified Theory in terms of piece-wise profiles but had a distinctly different flavor in terms of implementation. finite-difference methods for Navier–Stokes equations had been around for a long time: Thom [18] had used them well before the advent of the electronic computers.

After about 4 months, I had failed to obtain an acceptable solution to the BFS problem. I started to have my doubts and asked for a meeting with Brian (Fig. 5) to brief him on my progress with the literature review and my failure to make any headway with the Unified Theory for separated flows. I also mentioned the papers

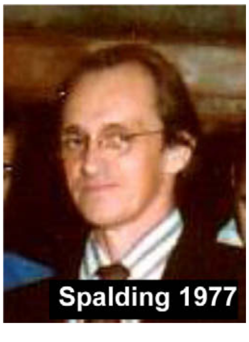

Fig. 5. Brian Spalding ~1977.

that had used finite-difference methods that reported success with separated flows at low-Reynolds numbers. Brian was quick to arrive at the key conclusion that there was no easy way in the Unified Theory to represent the key role that axial diffusion played in separating and reversing the boundary layer. The profile method therefore 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 a finite-difference computer program and was able to report success in solving the BFS and the 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 on different problems and that we had approached them from different view points – one from a high-Reynolds number and the other from a low-Reynolds number viewpoint. When we (Fig. 6) compared our notes, we realized that the main impact of the Reynolds number was on the nature of the matrix coefficients. With the central difference scheme that we were using, for any given grid, the matrix coefficients changed from positive-definite to non positive-definite at some critical Reynolds number and numerical instability ensued. Spalding then made the now famous analogy of how wind from the pigsty always stinks – or the wind from the north always brings cold. He knew this because he had grown up close to a pigsty. This led Spalding to propose 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). 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. This led to the formalization of the "Finite Volume" concept with its emphasis on volumes and fluxes rather than on the mathematical description in terms of distributions (basis-functions) and gradients. 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 general-purpose Navier–Stokes computer program to solve a diverse range of problems. 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 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. 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.

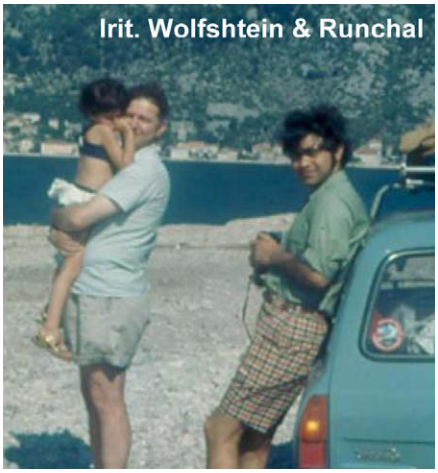

Fig. 6. Micha Wolfshtein, Akshai Runchal and Irit Wolfshtein.

Soon thereafter we came across a paper by Barakat and Clark [19] and we could see that the upwind approach had already been "discovered". Little did we know that upwind and one-sided differences had been around far longer. A paper by Courant et al. [20] 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. Also Burggraf [21] published his now classic paper on square cavity where he reported success at low-*Re* numbers but failure to obtain solutions 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 [22,23]. 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 due to high numerical diffusion.

Later on we started becoming wise to the pitfalls of upstream differences and this led to some work on numerical diffusion. Wolfshtein [24] published a technical note where he showed that numerical diffusion is related both to the speed of the flow and the angle of the stream-lines to the grid. Spalding [25] proposed an exponential method and in a companion paper Runchal [26] proposed the "hybrid" method [26] that automatically blended the central and upwind difference methods based on the local Peclet number. The hybrid method eventually became the established practice for all Imperial College codes.

Around 1967, I became fascinated with the Gauss Theorem and the integral approach to derive the algebraic analogue for the Navier–Stokes equations. This had to do with the classic "fight" between the "differential" approach of Newton and the "integral" approach of Leibnitz. One advantage of the integral approach is that it does not require smoothness or the existence of a 2nd order derivative. This is an important consideration for flows with sharp discontinuities such as shocks and contact surfaces. 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 [27] in terms of Taylor Series whereas I submitted mine with the integral approach [28]. Most Finite Volume codes today use the integral approach due to the simplicity that results from reducing the volume integral to a surface integral.

By mid 1968 both Wolfshtein and I had completed our thesis work. I took a hiatus and went to spend a long and productive summer in Cambridge (Massachusetts) to consult with Northern Research on the application of CFD to aircraft compressors. 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 (Fig. 7) who had just finished and modified it to measure flow behind a BFS at very

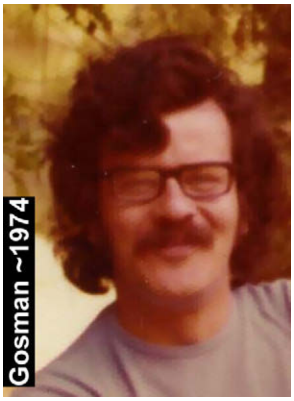

Fig. 7. David Gosman ~1974.

high Schmidt numbers. Though I did not like 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 [28] 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 our group. Both Wolfshtein and I were leaving Imperial College and Spalding 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 code from me and Wolfshtein. That code, called ANSWER, made it to the first book on CFD [29] that dealt with Elliptical form of Navier–Stokes equations. The authorship of this book illustrates the strict rule that Spalding followed that all joint publications carry the names in alphabetic order. Though Spalding was the prime contributor, his name appears near the end. It was also in 1969 that Spalding incorporated the first CFD based consulting company – CHAM (Combustion Heat and Mass Transfer) Ltd.

If a date for the "birth" of CFD is to be picked, 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 [29]. The Post-Experience Course at IC in 1969 reached a large number of researchers in the UK 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[1] 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 at Imperial College 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 in late 1970's which eventually became Fluent Corporation in 1983. Creare had acquired the software from Prof. Jim Swithenbank of Sheffield who in turn had acquired it from Imperial College.

4. The CFD decade at the imperial college: 1965–1975

The decade between 1965 and 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 (Fig. 8). 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 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 (LANL) under Frank Harlow (Fig. 9). The developments that occurred at Imperial College – after the emphasis shifted to three-dimensional flow and primitive variables in 1970 – owe heavily to the work done by Harlow. Harlow had experimented with a number of alternatives for solving the Navier–Stokes equations in primitive form. One of these was a pressure projection algorithm with a staggered grid and a pressure correction based on the continuity equation. However the pressure correction step required solution of the equations in transient mode. It was this development that provided the basis for the IC group to devise the SIMPLE algorithm that could be also be applied to steady flows.

Harlow's group worked on a wide variety of problems in fluid dynamics. His focus was mostly on transient flows with steady

Fig. 8. Jim Whitelaw and Brian Launder.

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[1] Combustion, Heat And Mass Transfer, Ltd.; later superseded by Concentration, Heat And Mass Transfer, Ltd.

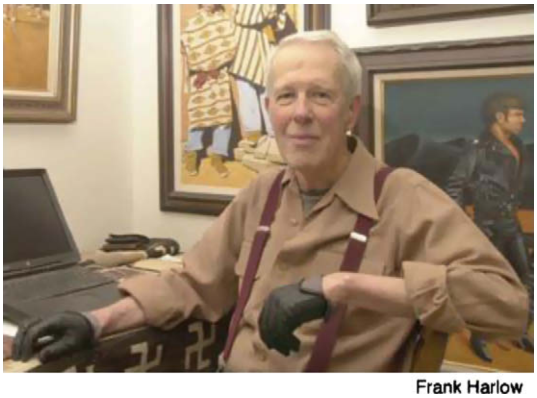

Fig. 9. Frank Harlow.

state as an asymptotic state of the flow. Many applications involved compressible flow or free surface. They often involved moving boundaries and multiple phases. Primary focus of the T3 group was on the physics and science of problems related to the weapons program at LANL. 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 to the IC group. 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 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 used 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 US 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 [30] had introduced a staggered grid and a decoupled pressure based on continuity equation for transient computations, and Chorin [31] 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 [32,33] 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 [34] 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 many 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 [35], Prandtl [36], Chou [37] and Rotta [38]. 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 [39] 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 the "ε" than for the "l" or "ω" formulations that were being investigated. Another reason was the ease of interpretation of "ε" compared to other variables; it was simply related 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 imme-

Fig. 10. Frank Schmidt.

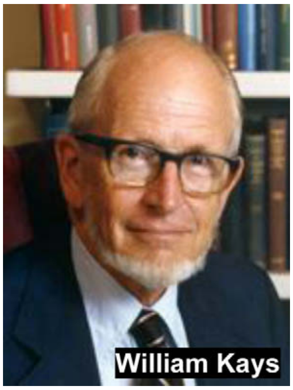

Fig. 11. William Kays.

diate interest to him. In fact later on he started espousing Multi-Fluid rather than the 2-equaion models for turbulence.

During this decade, the Imperial College group had distinguished visitors. These included J.C. Rotta (Goettingen), Frank Schmidt (Penn State; Fig. 10), William Kays (Stanford; Fig. 11), Chang-Lin Tien (Berkeley), Joseph Kestin (Brown), P.D. Richardson (Brown), Bill Reynolds (Stanford; Fig. 12), Philip Saffman (Caltech), Peter Bradshaw (NPL), David Pratt (Washington), Larry Caretto (Berkeley; Fig. 13), Graham de Vahl Davis (New South Wales; Fig. 14), Tony Hirt (Los Alamos; Fig. 15), Harry McDonald (United Aircraft Res. Lab.), David Dyers (Alabama), and many others. 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, Steve Cline with support from Bill Reynolds, William Kays and others was 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

Fig. 12. Bill Reynolds.

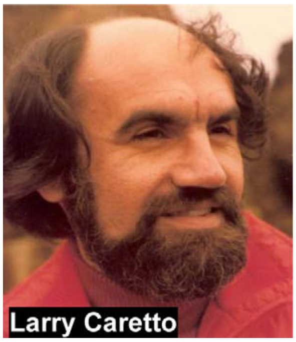

Fig. 13. Larry Caretto.

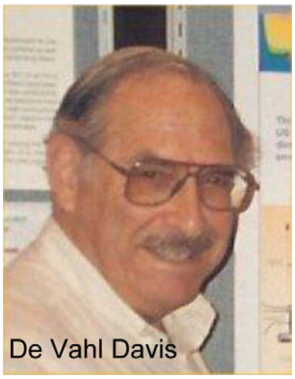

Fig. 14. Graham de Vahl Davis.

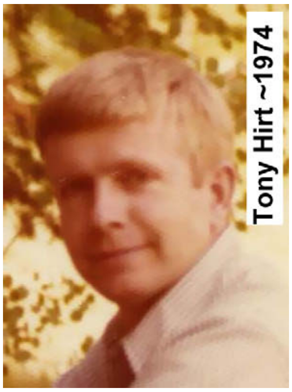

Fig. 15. Tony Hirt.

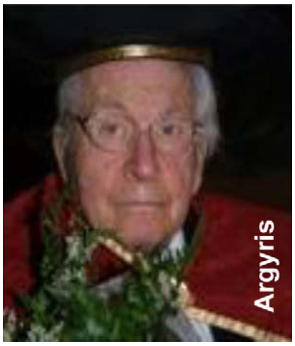

Fig. 16. John Argyris.

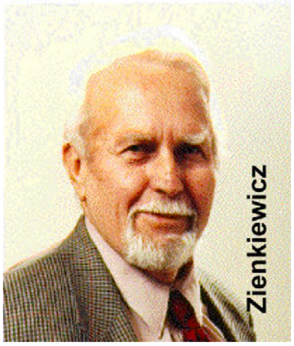

Fig. 17. Olgierd Zienkiewicz.

predictions with experimental data. These activities led to the IC CFD technology being widely known around the world.

The IC group also interacted with Argyris (Fig. 16) and Zienkiewicz (Fig. 17) about the merits of different approaches to CFD and their espousal of the finite-element (FE) method. Though IC group acknowledged some advantages 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 adopted 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 unstructured and boundary-fitted grids. With today's technology, the differences are more of semantics than of substance. It can be shown that FD, FE and FV, with appropriate assumptions, can all lead to identical algebraic equations. However profound conceptual difference remain; FD and FE both use a mathematical approach based on differential form of equations whereas FV uses a physical approach based on integral form of equations that stresses fluxes of variables.

5. The missed opportunities

The single-minded and focused approach followed by Spalding had some drawbacks. Spalding 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 it is the off-the-path ideas that lead to greener pastures. Of course one may also waste a lot of effort in looking for greener pastures.

During this period Spalding'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 no attempt was made to find a "limiter" for this scheme. Raithby [40] (Fig. 18) 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 coupled primitive variables SIVA algorithm [41] because of the focus on the segregated SIMPLE algorithm. It was subsequently shown by Vanka [42] and others to be a superior method for a class of strongly coupled flows. Another example is the early abandonment of the co-located grid, Runchal [43] because it was felt that co-located grids offered no distinctive advantage over staggered grids. Subsequently Rhie and Chow [44] 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 [24] did some tentative work on the subject but it was left to Hirt [45] to produce a formal and heuristic method to define these effects.

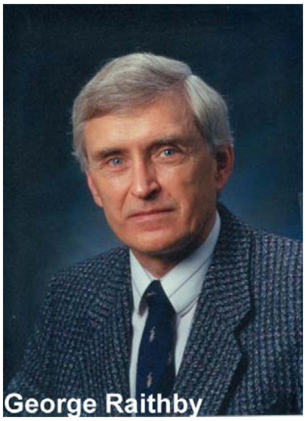

Fig. 18. George Raithby.

Another important example of a missed opportunity was the "vorticity-fluctuation" (k–ω) method for turbulent flows. Vorticity fluctuation seems a more intuitive and elegant representation of a cascade of turbulence eddies than dissipation. It is easier to envisage "vorticity" rather than "dissipation" being transported and diffused. After early and extensive exploration of it, the focus shifted and stayed with the k–ε model because diffusion coefficient for the dissipation equation was easier to define. It was Saffman [46] and Wilcox [47] 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 [38], 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.

6. The post: 1975 period

Spalding stayed at Imperial College till 1988 when he retired to devote his full attention to the development of the PHOENICS code and the consulting activities of CHAM. PHOENICS debuted in 1978 and 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.

Spalding subsequently went on to invent highly useful and simple tools and techniques such as the IPSA Inter-Phase Slip Algorithm, IPSA, [48] for predicting multi-phase fluid flow, a simple algorithm to determine the wall distance (needed for many turbulence and radiation computations) for complex geometries [49], a multi-fluid approach to turbulence [50], a multi-fluid approach for turbulent combustion [51], a novel approach for integrated radiation computations [52], and a methodology to unify fluid and solid mechanics [53]. He integrated all these diverse phenomena and brought them within the scope of CFD. All these are examples of Spalding'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 of flow, heat and mass transfer 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. Spalding'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 Spalding always asks is: "Can this be useful to a practicing engineer?"

7. Synopsis

Brian's impact on engineering practice is incredibly broad and truly amazing. He has worked in diverse fields of engineering and science over his career. Most people today know him for his CFD work. Yet the fact is that before he started with CFD in 1964, he had already made his mark in combustion [7], heat and mass transfer [11] and boundary layer theory [14]. He then made seminal and enduring contributions in CFD, turbulence and multi-phase flows. He was the first to formally propose that, in the context of numerical solution, *all* 2nd 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 post-1980 work on turbulence, multi-phase, solid-fluid interaction and wall distance computation has not yet seen the same popular adoption as these but he has pointed to important path-breaking research that may bear fruit in the future.

Throughout Spalding's 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 build 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.

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 multi-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!

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 [54] 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. If you consider the period between 1965 and 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 significant professional contributions and 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 [34] 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 [18], von Neumann [55], Southwell [56], Courant et al. [20], 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 [57] 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 two-equation models of turbulence. The basic equations were written down by Kolmogorov [35] and Prandtl [36]. Rotta [38] had added to the knowledge in the 1950s and elaborated on the concept of length scales and frequencies. Davidov [58] 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 [39] developed an epsilon equation and demonstrated its use in a 2-equation eddy viscosity scheme for a few flows. Hanjalic (Fig. 19), 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 – $\langle uv \rangle$) to predict flows where shear stress and strain vanished in different locations [59]. Bill Jones [60] extended the k–ε 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 [61] book on the subject and the seminal review of Launder and Spalding [62] that led to the formalization, "standardization" and acceptance of the k–ε model as a general tool for engineering practice.

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.

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.

Brian to me is the best example of Richard Feynman's famous saying: "*What Do You Care What Other People Think?*" He says 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.

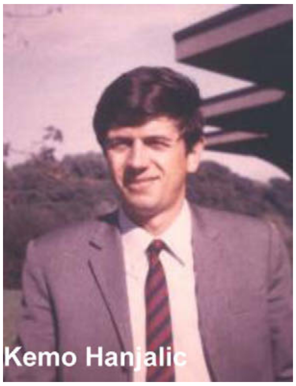

Fig. 19. Kemo Hanjalic.

Fig. 20. Brian and Colleen Spalding – 2008.

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 (Fig. 20) 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.

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