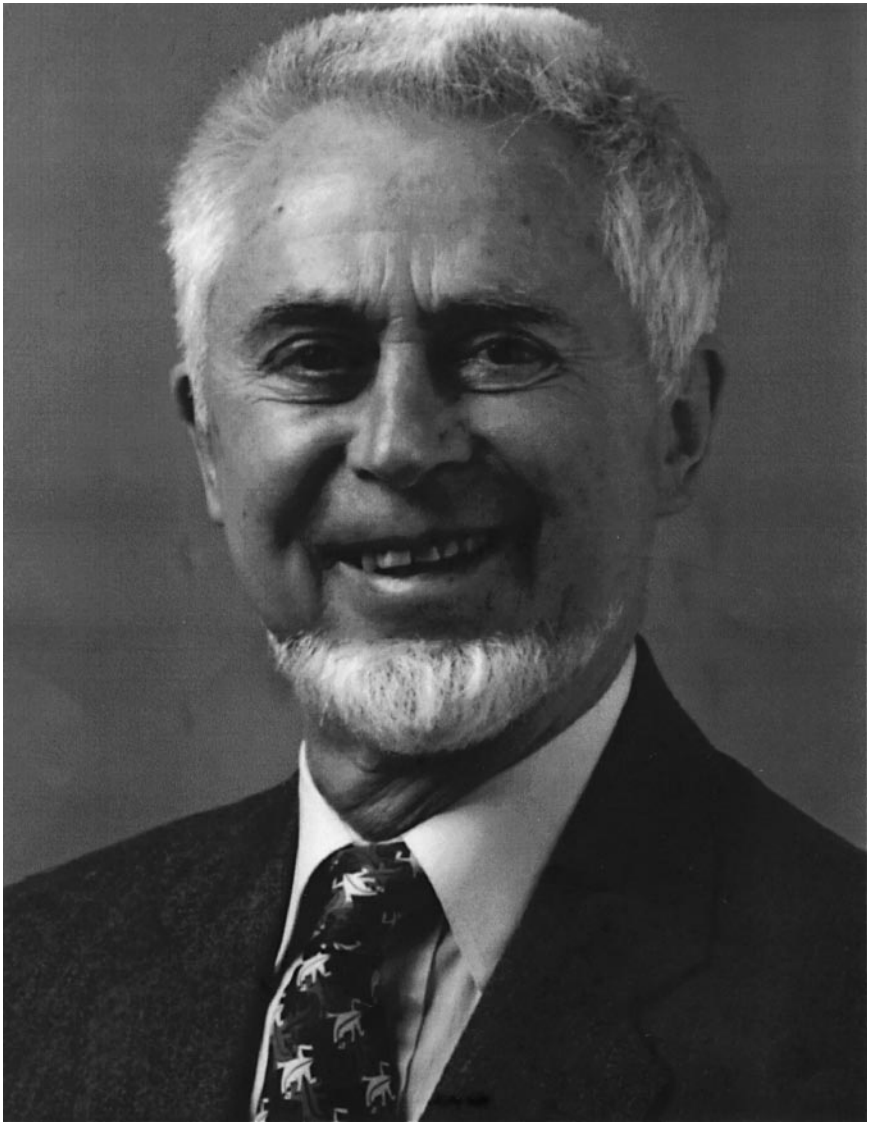

Milton Van Dyke

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MILTON VAN DYKE, THE MAN AND HIS WORK

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INTRODUCTION

I was moved and honored when the Editors of the *Annual Review of Fluid Mechanics* asked me to write a biography of Professor Van Dyke. I did my Ph.D. with Milton in the Department of Aeronautics and Astronautics at Stanford during the late 1960s and early 1970s. Since the first time that I met him almost 35 years ago, I have admired and respected him as a scientist but also have enjoyed his kindness, modesty, and wit. A few years ago, several of Milton's students organized a birthday celebration for him. His wife, Sylvia, wrote a biography of him as part of the informal proceedings volume. In writing this article, I have drawn liberally from that biography as well as from other written and oral recollections of Milton.

I also discuss some of Milton's technical work. The emphasis and perspective on these works is my own, including, of course, any possible misinterpretations. Prior to becoming a full-time graduate student at Stanford, I had worked in the Re-entry Aerodynamics Group at Lockheed Missiles and Space Company in Sunnyvale, California. Thus, I was quite familiar with several of Milton's papers on supersonic and hypersonic flow. Some of this work and follow-up work by other NACA and NASA scientists was used by us virtually on a daily basis. In addition to Milton's technical mathematics and fluid mechanics and his personal history, I highlight his extensive "public service" work. These largely unsung contributions bring many benefits to the worldwide fluid mechanics community. Perhaps most obviously, Milton co-founded the *Annual Review of Fluid Mechanics* more than 30 years ago. He has been the principal guide of this most important repository of our professional knowledge ever since.

For the most part, I tell Milton's story in chronological order. I deviate from this occasionally when I think doing so adds to the narrative.

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THE FORMATIVE YEARS

Milton was born on the 1st of August, 1922, in Chicago. The family was there just briefly. Milton's parents each had a strong academic orientation. His father had graduated in mechanical engineering from Penn State University. Milton's mother went to the University of Minnesota. She worked her way through college teaching mathematics and graduated Phi Beta Kappa. Milton's father spent most of his working life teaching mechanical engineering. The Great Depression made it difficult for him to find permanent employment, and Milton attended public school in seven different, mostly small, towns in the West and Midwest. Milton's mother was a brilliant woman who would have enjoyed a college teaching job. Unfortunately, only substitute teaching was possible for her, owing, probably, to a combination of discrimination against women and the depression-era viewpoint that the few jobs available should be distributed as widely as possible among families.

Ultimately Milton's father found a faculty position at Eastern New Mexico Junior College (now University) in the little town of Portales, New Mexico. Milton spent his three high-school years, grades 10–12, in Portales. He believes that much of the credit for his subsequent successes is due to the good education he received there. Milton, in turn, edited the high school newspaper and starred in the class play. He also became infatuated with several more unusual extracurricular activities. He joined the American Cryptogram Association, exchanging codes and ciphers, and techniques for solving them, with other enthusiasts around the country. He also became an avid reader of science fiction. He eventually wrote a short story of his own; it was almost accepted by Astounding Stories, the leading science fiction magazine at the time.

In those days Harvard awarded two National Scholarships each year to needy students in a handful of poor states, including New Mexico; Milton applied for, and received, a full 4-year scholarship and started university in 1940. Initially, he was surprised at the intensity and seriousness of many of the people he met at Harvard; it was very different from the easygoing small-town friendliness that he was used to.

Milton studied Engineering Sciences at Harvard. Among his professors were Richard Von Mises and Howard Emmons. Then, as now, Harvard was not an accredited engineering school. Milton says that this circumstance left him ample time to climb with the mountaineering club, play second violin in the school orchestra, and write and produce plays for the Harvard radio workshop. In any event, Milton completed his studies at Harvard in three years, graduated summa cum laude (with highest honors), and, as the school yearbook reveals, was elected to Phi Beta Kappa in his junior year. The yearbook also shows that most of Milton's Harvard classmates "prepared" at expensive private schools, primarily in the Northeast. The few that did not included Milton, who "prepared" at Portales High School, and Norman Mailer, Milton's classmate and fellow Engineering Sciences major, who "prepared" at Boys High School in Brooklyn. Milton is quick to point out that his shortened time at Harvard was due, in some measure, to the U.S. entry into World War II during his sophomore year. Engineering students were encouraged to finish quickly so that their skills could be used in the war effort.

Milton's job choices included working as a mathematician and code-breaker in Washington, D.C. or going to one of the National Advisory Committee for Aeronautics (NACA) laboratories. Milton felt strongly that he was an engineer rather than a mathematician. When the NACA recruiter agreed to allow Milton the job title of Engineer, in spite of Harvard's lack of an accredited program, his career path to fluid mechanics research was in place.

THE YOUNG SCIENTIST

Milton chose the new NACA Ames Laboratory, which was being built on the site of the Moffett Field Naval Air Station near the small town of Mountain View, California. Arriving in 1943, at first he worked with his bosses H. Julian (Harvey) Allen and Walter Vincenti on the 1- by 3.5-foot transonic wind tunnel. Milton recalls that much of his time was spent inside the test section because he was the only member of the group who could, or would, fit inside its 1-foot width. His first two reports were largely experimental; the first dealt with a search for low-drag airfoil shapes at high subsonic Mach numbers. Much of this work was done during World War II, but publication was delayed, perhaps because of wartime restrictions. The second dealt with swept wings and investigated, among other things, certain predictions of R. T. (Bob) Jones (Jones 1946). This series of experiments was performed in the newly built 1-by-3-foot supersonic windtunnel. Perhaps the most famous of the predictions was the result that if the normal component of the free-stream velocity on an oblique wing was subsonic, equivalent to saying that the swept wing lies within the Mach cone, then the wave component of drag will disappear. This result was confirmed by the experiments, as was the interesting prediction that it matters little whether wings are swept back or forward. In either case, the measured drag decreases significantly once the critical sweep angle is exceeded (Vincenti et al. 1948).[1]

[1]By the way, Bob Jones predicted that the basic insensitivity to sweep direction would also hold for an airplane with one wing swept forward and the other back. I remember Jones giving the Stanford Fluid Mechanics Seminar in Skilling Auditorium perhaps 20 years later, when I was a graduate student. He gave a theoretical discussion of the aerodynamics of an airplane with a straight wing that could be pivoted at any angle to the fuselage. Potential advantages included a less complicated hinge mechanism for variable-sweep airplanes and also the ability to align the wing with the fuselage to minimize hangar storage space. The audience showed proper respect to the eminent speaker, but I remember that the skepticism was palpable. At the end of his talk, he took out a small balsawood glider with a pivoting wing. He set the wing at about a 45-degree angle to the fuselage and threw the glider into the air. It flew just fine.

Young civilians looked out of place on a Navy base during wartime, so the aerodynamicists were all inducted into the “Ames Detachment.” Milton was ultimately promoted to the rank of Lieutenant Junior Grade in the Navy. Some of these Ames veterans eventually became professors at Stanford; I recall one of them telling me that their social lives improved markedly after they received their military uniforms during the war.

Milton made another important, albeit formally anonymous, contribution to the aerodynamics community during his first period at Ames. Perhaps the most used of all NACA publications was (and still is) the compilation of basic information on compressible flow, which first appeared as NACA TN 1428 in 1947 and was later released in final revised form as NACA Rept. 1135 in 1953. The earlier version was attributed to the Staff of the Ames 1- by 3-Foot Supersonic Wind-Tunnel Section, while the latter was authored by the Ames Research Staff. In fact, Milton was responsible for the material on thermally and calorically perfect gases, including shock waves, supersonic flow past wedges and cones, and the Prandtl-Meyer expansion. It seems that an entire generation of textbooks that discusses compressible flow follows the presentation of these topics as they are given in NACA TR 1135 (Ames Res. Staff 1953).

Upon decommissioning from the Navy after the war, Milton won a National Research Council scholarship for graduate study and enrolled in the Aeronautics Department of Caltech in September, 1946. By June 1949, he had received the M.S. degree and the Ph.D. (magna cum laude). He did his Ph.D. research with P. A. Lagerstrom. His thesis was entitled A Study of Second-order Supersonic Flow Theory. It was reprinted in somewhat revised form as NACA Report 1081 (Van Dyke 1952) with the same title. The thesis deals with improved analytical approximations for flows past thin and slender bodies through use of a novel method of iteration. It also incorporates a variety of innovative mathematical techniques (Van Dyke 1951, 1952).

After spending a postdoctoral year with Lagerstrom, Milton returned to the Ames Laboratory in 1950. He soon joined Max Heaslet’s Theoretical Aerodynamics group, working with R. T. Jones (who had moved to Ames from NACA Langley in Virginia), John Spreiter, and Harvard Lomax. Milton did additional work on higher-order compressible flow theory and began research on hypersonic flow.

It was also during Milton’s second tour at Ames that he focused his attention on the flow around blunt-nosed bodies in supersonic and hypersonic flow. Harvey Allen had discovered in the early 1950s that a blunt body entering the Earth’s atmosphere should be better able to withstand the severe heating enviroment than could a sharp-nosed object. This work was motivated by its relevance to the nascent space program and to military applications such as long-range missiles (Allen & Eggers 1958). Theoretical aerodynamicists all over the world had been struggling for a number of years with the tantalizingly difficult problem of predicting the simple-looking flow field ahead of a blunted object. This included finding the shape and location of the shock wave near the blunted nose where the flow is subsonic. It was clear that only a numerical solution was possible and, given the primitive vacuum-tube computers of the time, the solution would need to be an inverse one.

That is, a shock-wave shape was assumed, and the Euler equations were solved to find the shape of the body that produced it. Iteration would be necessary to solve the direct problem of determining the flow field for a body of given shape.

Milton's contributions to the blunt-body problem were twofold. He provided clear mathematical explanations for why the previous attempts to find a solution were unsuccessful. In addition, in collaboration with Helen Gordon, he created a finite-difference numerical procedure that worked successfully. Some previous attacks on the inverse problem used Taylor series expansion of the flow variables starting at the shock wave location. Milton explained that such an expansion must represent the flow both in the downstream "shock layer" and also in the physically fictitious analytic continuation of the downstream flow in the mathematical region ahead of the shock. Although the flow in the shock layer must be free of singularities, this need not be true in the upstream analytic continuation. Milton re-examined some series solutions and estimated that the singularities in the analytic continuation would typically lie closer to the shock wave than would the body itself. Thus the straightforward Taylor series could not form the basis of a successful computation, irrespective of how many terms were available (Van Dyke 1958a). Secondly, he knew that a numerical scheme that marches from the shock is inherently unstable. The flow downstream of the shock in the nose region is subsonic; hence the equations of motion are elliptic in character there. The situation is similar to solving Laplace's equation with boundary conditions given only on one side, rather than all the way around. This is the so-called Cauchy problem, the instability of which was studied by Hadamard in the 1920s.

The inherent instability of the Cauchy problem suggested that the prospects for a successful numerical solution to the inverse problem were poor. In spite of this, Milton's new scheme worked quite well. It produced solutions that were in close agreement with available Schlieren and interferometric wind-tunnel pictures as well as experimental body pressure measurements. The key was to use a shock-fitted orthogonal coordinate system and to take fewer, rather than more, finite-difference steps between the shock and the body. Because small errors, such as those due to finite computer word length, grow in geometric progression with the number of steps, the loss of accuracy from this cause will be less when fewer steps are taken. On the other hand, if a very small number of very large steps are taken, the finite-difference truncation error will become large. Balancing these competing effects to find the most accurate solution provided a criterion for selecting the optimal number of steps.[2]

The full algorithm for the Van Dyke–Gordon blunt-body solution was published in 1958, and extensive numerical results were given in the NASA report that appeared about the same time (Van Dyke 1958b, Van Dyke & Gordon 1959). The scheme formed the basis of subsequent numerical solutions produced by others,

[2]Bob Jones offered a simple physical analogy for Milton's discovery: A bicycle with a frozen steering column is inherently unstable, and the rider must ultimately fall to one side or the other. However if the goal is simply to cross the street and if the rider pedals hard enough, this can be accomplished before the bike topples over.

in the United States and elsewhere, over perhaps the next 15 years. The refinements introduced by others included further generalization of allowable shock wave shapes, automatic iteration to solve the direct problem, some consideration of real-gas effects, and calculation of static and dynamic stability derivatives. It was only after computer power had increased by several orders of magnitude by the 1970s that Milton's inverse scheme was replaced by truly direct schemes. These direct schemes were time-dependent and "captured" the correct shock position as the unsteady numerical solution settled down to its final steady-state configuration. It is fair to say, however, that the necessary theoretical predictions of the fluid flow during atmospheric entry, for an entire generation of spacecraft and missiles, including the Apollo vehicle used by the astronauts as they returned from the moon, were made using the Van Dyke–Gordon algorithm.

Considering the military relevance of the blunt-body problem, it was fortunate for the scientific community, and perhaps a bit surprising, that neither the American nor the Soviet security establishments thought to make the work classified. Through scientific contacts, Milton became friendly with a number of Russian scientists. Indeed, several years later during Milton's first sabbatical leave from Stanford in 1965, he spent three months working as a guest of Dorodnitsyn's computing center in Moscow under an exchange program between the United States and the Soviet Academies of Science. By this time Dorodnitsyn's group had also written a number of papers on the blunt-body problem. During this three-month period, Milton learned the language sufficiently well that he was able to lecture in Russian at Leningrad University.

While working at Ames Laboratory, Milton applied for and was awarded a Guggenheim Fellowship and a Fulbright grant to spend the 1954–1955 academic year with George Batchelor at Cambridge University. His family, including his two young sons Russell and Eric, went along. During this sabbatical year Milton lectured extensively in England and Western Europe as an AGARD consultant. In Cambridge Milton became well acquainted with G.I. Taylor. It was a friendship that continued to the end of the older man's life. During the International Congress of Theoretical and Applied Mechanics in Stresa, Italy, in 1960, Milton offered G.I. a ride on the back of his Vespa motorscooter. G.I. needed to get to his lodging, which was a few kilometers away along the shore of Lake Maggiore. A French colleague snapped a photo of their departure. It would have made a splendid picture of one of the world's greatest scientists on a motor scooter; but unfortunately the picture was lost during development. Some years later, in 1970 I believe, G.I., who was in his eighties, was visiting the Van Dykes in their home on the Stanford campus. Milton invited his graduate students, including me, to their house to meet Taylor.[3] Milton is especially proud of the fact that he wrote appendices to two

[3]It was great to have the chance to talk to G.I. Taylor. I remember that I asked him a question about lifting-line theory that he answered. Then he told us that Lanchester, who divised lifting-line theory independently of Prandtl, had been a great friend of his. They went sailing together in a club called the Royal Yacht Squadron in the years prior to World War I.

of G.I.'s papers during the 1960s. The papers deal with the effects of an electric field on jets of conductive liquid. In each work, Milton formulated appropriate mathematical models that could be solved in closed form in limiting cases. In the papers, Taylor showed that the Van Dyke theoretical results agreed well with the experimental measurements.

While researching for this article, I noticed something rather remarkable. The Van Dyke & Gordon (1959) blunt-body paper is the very first NASA Technical Report. Also, Milton's other paper, entitled The Slender Elliptic Cone as a Model for Nonlinear Supersonic Flow Theory, starts on page 1 of Volume 1 of the *Journal of Fluid Mechanics* (Van Dyke 1956). I suspect that Milton would attribute these priorities to mere coincidence. On the other hand, it seems likely that the editors of each serial would have wanted to start it with an especially significant contribution.

In the spring of 1958, Paul Germain invited Milton to teach (in French) as a visiting professor at the University of Paris during the following academic year. Milton accepted even though he didn't speak any French, but he had six months to learn. The French teacher at Palo Alto High School opined that it would be quite impossible to achieve the necessary fluency in so short a period via night school classes. So Milton studied on his own using French books and newspapers and, most important, the songs of Jacqueline Francois for the introductory course and Edith Piaf for the advanced one. In Paris, he gave a 23-lecture course on *Théorie des Ecoulements Hypersoniques*. He also supervised the thesis of a student from Vietnam, with whom he spent so much time to improve his French, that Germain accused him of developing a strong Vietnamese accent. Years later when Professor Germain taught a course at Stanford, he told us that Milton "spoke French like Molière." One assumes that he meant Milton's choice of words, rather than his accent.

THE STANFORD PROFESSOR

While he was in Paris, Milton received good news from California. Fred Terman, the Dean of Engineering at Stanford, had brought Nicholas Hoff from Brooklyn Polytechnic to build an Aeronautics department, and Hoff invited Milton to join the faculty as a full professor; he accepted at once. As Sylvia tells the story, it was only many years later that Hoff mentioned that he had consulted Hans Liepmann at Caltech, who said, "Van Dyke has published a lot of papers . . . but he's a good man anyhow."

Milton met Sylvia in 1961 at the wedding of Jim Yakura, his first Ph.D. student. Sylvia, who is from England, was living in Lake Tahoe at the time. Several weeks later, Milton went to visit her at Lake Tahoe. Always the pragmatist, Milton took along a few books in case the visit didn't work out. It did work out, and they were married in Stanford's Memorial Church in June 1962. Sylvia is a wonderfully creative, kind, and caring person. When visiting with the Van Dykes, it is always delightful to see the affection that they have for each other.

After joining the Stanford faculty in 1959, Milton introduced a course on Perturbation Methods. The target audience for this course was the subset of graduate students in the Aeronautics and Astronautics Department whose principal interest was fluid mechanics. That department then, as now, only awarded graduate degrees. The course was not one of the core courses in fluid mechanics, meaning that the material that was covered was not part of the body of information that was required for the Ph.D. qualifying examinations. Thus the course was truly an elective, and students could take it or not as they chose. In spite of this, course enrollment was usually quite high; when I took the course in 1967 I believe there were about 30 students. These included members of the target audience, graduate students from other disciplines, and a number of part-timers who worked in companies near Palo Alto. I was, at the time, in the last category. The popularity of the course was due, I think, to the growing appreciation for the importance of perturbation methods in mechanics and the unique opportunity to learn these methods from one of the main developers. By this time Milton had also earned a reputation as an enthusiastic and innovative teacher.

After teaching the subject for four years, Milton wrote a book, *Perturbation Methods in Fluid Mechanics*, which was published by Academic Press in 1964 (Van Dyke 1975). *Perturbation Methods* is a remarkable book. It is written clearly and concisely, yet it adequately covers the range of perturbation techniques. It is very suitable as a textbook for a graduate-level course. However it soon becomes apparent to the reader that much of the book is Milton's own original work. In fact some of this work has never been reported elsewhere.

For many of the readers of this review, a description of *Perturbation Methods* hardly seems necessary because it has been, for many years now, a standard reference work. The emphasis in the book is on singular perturbation problems, those for which a straightforward expansion, based on the smallness of a parameter, loses validity in some region of space. One of the early chapters might be considered as the centerpiece of the book. A familiar problem in fluid mechanics is considered in detail because it exhibits many of the features that appear in a wide class of problems in mechanics. The perturbation solution for two-dimensional potential flow over an airfoil is the model problem. A typical airfoil has a thickness that is much smaller than its chord; thus, the ratio of these lengths is a suitable small parameter. Yet, the plausible assumption that a thin airfoil disturbs the otherwise uniform flow only slightly is obviously violated at the front and rear stagnation points. Thus the thin-airfoil approximation to the solution fails in the neighborhood of these points. Two methods are introduced to achieve a uniformly valid approximate solution. These are the method of matched asymptotic expansions and the method of strained coordinates. The first of these is the more generally applicable method for problems in mechanics, and much of the chapter is devoted to its implementation for airfoils of various shapes.

The book includes the first appearance of what has come to be known as Van Dyke's Asymptotic Matching Principle. Its simple form is

the m-term inner expansion of (the n-term outer expansion)
= the n-term outer expansion of (the m-term inner expansion).

The rule refers to a situation where we have *n* terms in an outer expansion that is valid everywhere except in a small inner region where the expansion fails to be a good approximation. These may be *n* terms in a power series or other expansion in ϵ where the small number ϵ is a ratio of two length scales. These scales are a measure of the size of the inner region and the outer region, respectively. This outer expansion would have been obtained by simplifying the full problem in the limit of the inner length scale becoming small while the outer scale is held fixed. Similarly, we also would have an *m*-term expansion that is valid in the inner region and was obtained by assuming the inner length scale to remain fixed while the outer scale becomes very large. The process of matching, using this rule, allows information to be transferred from one region to the other. This information is used to resolve indeterminacies in one or the other expansion, such as finding the values of otherwise unknown constants. Once the two expansions are known for particular values of *n* and *m*, it is possible to combine them to form a uniformly valid composite expansion that is, in Milton's words, an "imperceptively smooth blending" of the two local approximations.

Milton originally believed that the Asymptotic Matching Principle was merely another way of stating the intermediate matching concept of S. Kaplun and used by Lagerstrom, Julian Cole, and others. According to this principle, matching is performed in some intermediate or overlap domain for which another approximate solution can be constructed. This intermediate solution can then be matched appropriately with both the outer and inner expansions. It became clear, however, that the two rules are fundamentally different. Van Dyke's principle is much easier to implement and can be superior in that matching to a given order of accuracy can be achieved using fewer terms. On the other hand, using the Van Dyke principle, there is some ambiguity in counting terms when logarithms appear in the series. More careful consideration is required then, as explained in the second, or annotated, edition of *Perturbation Methods* that appeared in 1975.

Perturbation Methods is a very heavily cited book. In 1980 it was named as a Citation Classic by the Institute for Scientific Information (ISI), the publisher of the periodical *Current Contents*. It had been cited approximately 1000 times in the journals monitored by ISI. I believe that this is largely because the book is the primary and best source of information about matched asymptotic expansions in mechanics and also includes the first published statement of the Matching Principle. It is the practice of ISI to ask authors of Citation Classics to supply some background information to be printed in *Current Contents*. In his very modest statement, Milton offered another reason for the popularity of the book, namely that it was cheap. That was, and still remains, quite true.

Milton had driven a hard bargain with Academic Press when negotiating the publication of *Perturbation Methods*. For the benefit of students, he succeeded in persuading their West Coast representative to add a phrase to the contract stipulating that the book should cost no more than three cents a page, equivalent to seven dollars for the entire book. Academic was furious and attempted to persuade Milton to delete that condition, going so far as to say, "You know, Professor Van Dyke, your book will have more prestige at ten dollars than seven!" The book went through five printings, selling about 8000 copies in all. Because Milton refused to allow a price increase, Academic let it go out of print even though it was still selling 500 copies a year. A clause in the contract required them to give the copyright back to Milton, who decided to republish it himself even though he had no experience as a book publisher. His new publishing company was called Parabolic Press. The name is particularly suitable for a book dealing with theoretical fluid mechanics. Milton has noted that many fluid mechanics phenomena are more clearly illustrated when the boundary shapes are simple, and the parabola is remarkably good for this purpose. Thus, as Parabolic Press, he published the Annotated Edition of the book in 1975. The new book contained all of the information in the Academic version plus 33 pages of notes and many new references. By 1997, that edition had sold some 9000 copies. Milton credits the people at *Annual Reviews* for supplying him with valuable information about the mechanics of book publishing. Milton mastered this skill very well; the printing and binding of the handsome Parabolic edition is at least as good quality as Academic's version.

Milton maintained the seven dollar price for many years; the price included shipping anywhere in the world. The following year, as a young lecturer in the Department of Applied Mathematics at Adelaide University in Australia, I used the book as a text for an 8-week honours course. Honours students there were quite gifted academically, and the book was a big hit. In those years Australian money was buoyant, so the local price was only about five dollars. I bought the books for the class and resold them to the students. Because it is an isolated captive market, books were always very expensive in Australia. I don't think the students ever believed me when I told them the price; I think they wondered why this crazy American was subsidizing the cost of their textbook out of his own pocket. Happily, 25 years later, the book is still available from Parabolic Press. It can be purchased easily on the World Wide Web, and it is still inexpensive. Milton says that he has never lost money on the book, but I have my doubts. A Russian edition of the book was published in the late sixties and sold 6000 copies. I think that the Russians never asked for his permission to publish, but they did pay royalties in rubles that could only be spent in the Soviet Union. There was also a Taiwanese pirate edition that was sold very cheaply to students in Asia. Given Milton's philosophy as a publisher, I doubt that this troubled him very much at all.

Milton's experience in self publishing has served as an inspiration for some others in the fluid mechanics community. Some years ago, C-S. Yih, another pillar of the community, told me that Parabolic was the model for his own West River Press, the publisher of the second edition of his graduate text on fluid mechanics. He also credited Milton for his encouragement and for teaching him how to become a publisher.

There are a number of little gems in *Perturbation Methods*. One of my personal favorites concerns lifting-line theory. This is a theory, due to Prandtl and Lanchester, for predicting the lift of a wing of given shape in an assumed inviscid flow. The lift is reduced because of the "downwash" due to the trailing vortex sheet behind the wing. The theory leads to a singular integro-differential equation for $\Gamma(y)$, the spanwise circulation distribution, the integral of which gives the total lift of the wing. This equation is called variously the "fundamental equation of lifting line theory" or, in somewhat older books, the "fundamental equation of finite-wing theory." I have no doubt that the fundamental equation was a principal design tool for the first two generations of aerodynamicists. In my mind's eye, I see rooms full of "computers," that is people with, at most, brass-wheel calculating machines whose task it was to solve the discrete system of linear algebraic equations for wing design of civilian and military airplanes. In almost an off-handed way, Milton remarks in the book that it is unnecessary to solve the integral equation and that answers of equivalent accuracy can be obtained by a simple quadrature. This follows from his formulation of lifting-line theory as a perturbation expansion based on the smallness of the chord of the wing compared with its span. Equivalently, one assumes that A, the so-called "aspect ratio" of the wing, is large. The simple result, replacing the integral equation, is

$$\alpha_e(z) = \alpha \left[1 - \frac{1}{2A} \, \mathrm{P}\!\!\!\int_{-1}^{1} \frac{\mathrm{h}'(\zeta)}{z - \zeta} \mathrm{d}\zeta \right],$$

where α is the geometric angle of attack and $\alpha_e(z)$ is the effective angle of attack that each wing section "sees." Here the shape of the wing is given by the function $h(z)$, the semi-span is normalized to one, and the integral is of the usual principal-value type encountered in potential theory. Though not an issue of great importance in these days of very fast computers, in the first half of the twentieth century, when lifting-line theory was an important design tool for wings, Milton's discovery would have reduced the amount of human computation very considerably. In particular, for a numerical solution where the wing is discretized into N spanwise slices, the computational labor to estimate the lift would be reduced by a factor of order $1/N$. Further application of matched expansions allowed Milton to carry the perturbation solution to the next higher order of approximation. At this order the predicted lift for a wing of elliptic planform, the standard test case, agrees well with the numerical solution of the unapproximated potential flow problem. This good agreement is demonstrated for aspect ratios as small as two.

To the fluid mechanics community, Prandtl's invention of boundary-layer theory is usually considered his greatest achievement. Prandtl understood the scalings required to simplify the problem in the inner and outer regions and recognized that he had achieved the leading-order approximation in a process that could be extended to higher order in some way. It remained for Milton to show that the problem was susceptible to systematic attack via the formalism of the method of matched asymptotic expansions. In a series of papers that appeared in the 1960s, he developed a higher-order boundary-layer theory; in fact he can be considered the world authority on this subject. A good overview of the mathematics of boundary layers is given in the chapter on viscous flow at high Reynolds number in *Perturbation Methods*. The book also contains work on the influence of the coordinate system on singular perturbation problems. Milton develops an idea, due originally to Kaplun, that so-called optimal coordinates may be found. In such a coordinate system, which can be found for some problems, an asymptotic approximation would be valid everywhere. In some ways, this coordinate transformation is similar to the result of another perturbation method considered in the book. That is the method of strained coordinates, sometimes called Lighthill's technique or the Poincaré-Lighthill-Kuo method.

Apropos boundary-layer theory and matching, Milton recently wrote a historical review on the origins of the subject (Van Dyke 1994). The oldest reference is to the work of Laplace who predicted the static axisymmetric shape of a small sessile drop of liquid resting on a horizontal surface. The pressure within the drop is determined by gravity and surface tension leading to a nonlinear ordinary differential equation for the droplet shape. In work reported in 1805, Laplace found solutions to simplified problems for the shape, both away from the droplet edge and also in the neighborhood of the edge when a dimensionless measure of surface tension is small. He then determined the unknown constant in the solution away from the edge by matching with the edge solution in exactly the modern way. Thus Milton credits Laplace with inventing matching, but he also reports on other nineteenth-century applications of the method to various problems in mechanics and classical physics.

I first met Milton in the early fall of 1966. I had just become a part-time graduate student in the Aeronautics Department at Stanford. Purely by luck, Milton turned out to be my academic advisor. Aerospace engineering was the high-tech industry of the 1960s and was easily the most important industry in the Santa Clara Valley in those pre-silicon days. To those of us working in industry, Milton was a "star," arguably the brightest star. Thus, I was quite surprised by how youthful he was; he looked barely older than the graduate students. His manner was welcoming and unhurried, and he carefully explained the things I needed to know.

I took my first course from Milton the following year. It was Hypersonic Flow Theory and thus was closely related to my work at Lockheed. This was necessary if I wished to be reimbursed for the cost of the course; it was only later that the company switched me to a program with less restrictive rules. Then I was able to take Milton's two other regularly scheduled courses: Perturbation Methods, as already discussed, and Symmetry and Similitude in Fluid Mechanics.

Similitude was a very popular course. It reviewed and extended classical dimensional analysis. An important goal was to find similarity solutions, corresponding to a reduction in the number of independent variables that are needed to describe a particular flow. Milton presented new insights and powerful methods, such as the invariance of a problem under group transformations, as tools in the search for similarity. Similarity is particularly important in fluid mechanics where nonlinearity is the norm and phenomena of practical interest are usually quite complicated. Also, it is often the case that the problems we face as engineers are not completely defined. The theme of the course was to extract as much information as possible from what one is given in a particular problem. I recall Milton saying that our goal was to emulate the Armour Meat Packing Company who proclaim that they "used every part of the pig except the squeal." Milton advocated a "hands on" approach where possible. A problem assignment that I remember was to determine the frequency of rotation of a tumbling strip of paper as it fell through the air. For a couple of days the corridors were crowded with students intently following their rotating paper strips. Years later, the consensus of my contemporaries was that Similitude was the most useful course that they had taken at Stanford.

About this time, Milton and Bill Sears of Cornell jointly founded the *Annual Review of Fluid Mechanics*. Originally, the people at Annual Reviews had approached M.J. Lighthill to ask about starting a series in mechanics. Lighthill told them that mechanics was too broad a subject and that it should be restricted to fluid mechanics. Moreover, for several reasons, he suggested that they should approach Professor Van Dyke at Stanford. First, he was eminently qualified; second, he would be likely to accept the responsibility of editing the series; and finally, by happy circumstance, Milton was also located close to the Palo Alto office of Annual Reviews. This was the start of Milton's involvement with Annual Reviews, which has lasted over 30 years. Volume 1 appeared in 1969 and was edited jointly by Sears and Van Dyke.

After finishing much of my coursework, Milton said it was time for me to find a research advisor for my Ph.D. Trying to sound as casual as possible, I said, "How about you?" Milton agreed immediately. It is my belief that Milton accepted all students who wished to work with him. I could see that he tried hard to treat all students equally; thus accepting some and rejecting others would have been inconsistent.

At the time much of Milton's research effort was devoted to the computer-assisted generation of regular perturbation series in fluid mechanics and their "analysis and improvement." This is a fundamentally different and, in some cases, a more effective way of using the computer. A number of problems in fluid mechanics can be treated as regular perturbation expansions in a small parameter. By delegating the determination of the coefficients to a computer, these expansions can often be carried to quite high order. A good deal of information about the analytical structure of the solutions can be extracted from the series. Series improvement includes ways to recast series so that they may be summed successfully outside the original domains of convergence. A variety of techniques were found to be useful, including Domb-Sykes plots, the Euler transformation, series reversion, and Padé approximants. Successful application of these methods can produce striking results. Thus, for example, Milton used the computer to extend Goldstein's series for the Oseen drag of a sphere to high order. The nominally small expansion parameter is the Reynolds number *Re*, and the original series is valid only for *Re* less than approximately 4. Milton recast the series in several different ways. The improved series produced converged results for all finite *Re* and also improved upon known results for the the limiting case $Re \to \infty$ (Van Dyke 1970). Some years ago, Milton wrote a summary of these techniques for the *Annual Review*. He also discusses many applications in fluid mechanics in that article (Van Dyke 1984). A particular series expansion method arising in water-wave theory and due originally to Stokes eventually became the main subject of my dissertation.

About half of my graduate-student colleagues were also doing research in series methods. These included Walt Reddall, Dave Lucas, Gil Hoffman, and Ramesh Agarwal. The rest were involved with singular perturbation problems, primarily in aerodynamics. Another exception was my office-mate Hart Legner who revisited and extended Saul Kaplun's work on optimal coordinates.

Milton was quite generous with his time and carefully followed the work of each of his students. He encouraged us and was always ready to help us celebrate our achievements. His personal contribution to our work was quite substantial but was consistently downplayed. (In fact, Milton was never a co-author when student dissertation research work was published.) It seemed as if he was always in his office, and I don't recall ever making an appointment to see him. It seems remarkable to me now because, along with all his other responsibilities, he was supervising the work of about 10 research students. If Milton had someone else in his office, a person would be invited in anyway, introduced to the other visitor if necessary, and the conversation would be enlarged to include everyone. Milton was very productive during those years; yet I wondered when and where he was able to work. Sylvia gave birth to identical triplet boys in 1967. Their daughter Nina was four years old at the time. I suspect that the Van Dyke household was not always peaceful enough for scholarly pursuits in those days.

One drawback of the series method was the need to calculate the solution by hand up to sufficiently high order that all parts of the computer program could be exercised and validated. I remember doing this for a problem, spending at least a month on the effort, and still not being sure that I had done it correctly. I bemoaned my fate to Milton one Friday afternoon. By Monday morning, he had independently reproduced what I had done. For me, this was a remarkable demonstration of Milton's prodigious ability to do highly efficient and meticulous algebra.

Life as a graduate student at Stanford was almost idyllic 30 years ago. Lots of sunshine, bicycle paths, tennis courts, and a lake to swim in. However, the late 1960s and early 1970s were also the time of the Vietnam War. The war was unpopular on all university campuses, and, for much of this period, Stanford was a center of anti-war activity. Coincidentally, the Aeronautics Department had just moved into Durand, a beautiful new building with soaring columns and glass walls. Because many people equated aeronautical engineering with weapons design, Durand was a focus of student protest. A number of times, we were greeted by broken glass in the morning because rocks had been thrown at the building during the previous night. Like the majority of students and faculty at Stanford, Milton opposed the war; yet he did not succumb to the stridency that many of us found so appealing. There were times during this period that I saw him lose his natural and infectious optimism. As a man of conscience, he was deeply troubled because the protesters were not wrong in their position that scientists and engineers do create, either directly or indirectly, the weapons of war. Perhaps it took the reality of a cruel and unjustifiable war to bring that message home. I think he wondered whether this ethical dilemma was too high a price to pay for working in a field that he liked so much and that perhaps he should had done something different as a career. In reality, Milton's research and that of his students was always primarily theoretical and heuristic, rather than being applications oriented. Looking at Milton's publication history, I think I detect a further subtle shift towards classical problems starting about that time.

Milton was encouraged by the success in self-publishing *Perturbation Methods* to fulfill a long-held dream. Once he had seen, in a little book shop on the left bank in Paris, a beautiful (but expensive) collection of black-and-white photographs from optical research, titled *Atlas de Phénomènes Optiques*, and realized that students of fluid mechanics needed a similar collection—but at a modest price. He requested experimental prints from colleagues around the world in all fields of fluid mechanics. He received about 1000, from which he published the best 400 in *An Album of Fluid Motion* (Van Dyke 1982). It has sold over 40,000 copies to date, which must be some sort of record for a scientific book. Designed jointly by Sylvia and Milton, it is a very handsome book and is reasonably priced. The album has been used in a number of fluid mechanics courses. Quite likely, it also has been purchased by less technically oriented readers. A Russian edition was prepared, fully equal in quality thanks to the loving care of Milton's friend G.I. Barenblatt, who scoured Western Europe for the best paper and ink and supervised the entire publication process. Noting that he had several orders from the Amish Country of Pennsylvania, Milton once commented that we may soon see hand-knitted quilts in the pattern of the Kármán vortex street.

The Album was the inspiration for The Gallery of Fluid Motion, which has become an integral part of the annual meeting of the American Physical Society (APS) Division of Fluid Dynamics. The Gallery was founded by Helen Reed, with Milton providing much of the initial guidance and enthusiasm. The best pictures submitted to The Gallery are published each year in a special article in *Physics of*

Fluids. At several APS meetings, I remember that Milton asked me to vote for the best entries.

This story would not be complete if I did not mention Milton's readiness to respond when he saw that people needed help. I am aware of a number of times when Milton helped colleagues with both scientific and personal problems. These efforts ranged from sending courteous notes containing suggestions on how scientific work could be improved to helping people to obtain justice. For example, because of his technical work, Milton was very greatly respected by the scientific community of the old Soviet Union. More than once, he used his prestige and his persistance to intercede on behalf of colleagues there who were being treated unfairly. Milton's prominence protected them from retribution by the authorities. I think that the cases that I know about are only a tiny fraction of the whole. Even this information was found out only with difficulty because of Milton's unwillingness to talk about such matters. I think he wishes to respect the privacy of individuals and also refuses to claim credit for what he believes to be simple acts of human decency.

Milton has supervised the research of about 40 students, most of whom were awarded the Ph.D. degree. Several students received the degree of Engineer. Milton's students have gone into private industry, government research laboratories, and academia. Each has been rewarded with an interesting career, and it is a pity that only a couple of them can be mentioned here. Notable among academics is Ali Hasan Nayfeh who completed his Ph.D. with Milton in 1964. Ali's dissertation developed fundamental concepts in the perturbation technique now known as the method of multiple scales. During his long and distinguished career, Ali has published extensively on the use of perturbation methods in a variety of application areas and has written several books. Another of Milton's students is Bob Rogallo, a NASA Ames scientist. Among his other achievements, during the 1970s Bob developed a Fortran-like parallel processing compiler called CFD to run on the ILIAC IV, the first massively parallel computer. Bob is one of the most respected scientists in the turbulence community; his pioneering direct numerical simulations provide one of the landmarks in this field. Bob is also the son of Francis Rogallo, the inventer of the famous kite that bears his name. Interestingly, Milton remembers that it was Francis who had recruited him to work for NACA so many years ago.

In 1997, Milton's students organized a symposium to celebrate his 75th birthday. Most of his students were there as well as colleagues and friends. Many of the celebrants are shown in the following photo (Figure 1). It was a wonderfully warm occasion. In addition to technical talks, people gave personal reminiscences about shared good times and the influence that Milton had on their lives. Milton's whole family was at the conference banquet where Walter Vincenti, Milton's closest friend, spoke about the early days at NACA. Milton's children told charming stories about their Dad. While all of these speeches were quite different, their totality is the saga of a most remarkable man.

Figure 1 Van Dyke Symposium, November 21, 1997. Row 1: Samuel McIntosh, Helen Reed, Joseph B. Keller, Nicholas Rott, Milton Van Dyke, Andreas Acrivos, Walter Vincenti, John Wehausen, Holt Ashley, John Spreiter, G.I. Barenblatt (*standing*); Row 2: Stephen Chang, Art Messiter, Ilan Kroo, Sanjiva Lele, Julian D. Cole, Richard S. Lee, Ali H. Nayfeh, Raul Conti, Soonil Nam, Theresa Schwartz, Leonard Schwartz; Row 3: Yulu Krothapalli, Hsien K. Cheng, Hsiao C. Kao; Row 4: Ramesh Agarwal, Robert Chapkis, Donald Baganoff, G.M. Bud Homsy, Bryn Homsy, Susann Novalis, Parviz Moin, Allen Plotkin, Tom Rivell; Row 5: Bob Underwood, Norm Whitley, Hart Legner, Kamyar Mansour, Walt Reddall, Stephen Stahara, Leen van Wijngaarten, Bob Rogallo, Robert Kadlec, Bernard Ross, Peter Bradshaw, Christo Christov.

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