The Journal of the Astronautical Sciences, Vol. 54, Nos. 3 & 4, July–December 2006, pp. 273–297

In My Estimation

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אשרי אדם מצא חכמה ואדם יפיק תבונה׃[2]
Proverbs [3:13]

FELIX QUI POTUIT RERUM COGNOSCERE CAUSAS.[3]
Publius Vergilius Maro (70–19 B.C.E.)

πόνος γαϱ, ὡς λέγουσιν, εὐκλείας πατήϱ.[4]
Euripides (480–406 B.C.E.)

Introduction

This article is based loosely on my keynote address, "Beyond Estimation" [1], presented at the Symposium. The symposium keynote address covered a variety of topics which I felt were important to Astronautics, but were not, with one small exception, part of Astronautics. Most of these do not appear in the present work. Some were inappropriate to a technical journal, even for a keynote address; others were too self-congratulatory. What remains, essentially, are three short essays: (1) an expansion of my very short history of Spacecraft Attitude Estimation (still confined to my own limited perspective); (2) an essay on what I believe to be the true role of the Arts in Engineering education and research, only slightly different from the proceedings version; and (3) a new section, my advice to young researchers in Astronautics, a suitably valedictory closure. Thus, the journal version of my keynote address is very different from the version in the proceedings, for which reason I have given it a different title. But the exposition here is not less significant. My attitude towards the Liberal and Fine Arts and the principles behind my advice to young researchers are what have informed my career and my life in Astronautics.

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[2]Happy [the] man [who] findeth wisdom, and [the] man [that] getteth understanding.
[3]Happy [he] who has been able to know the causes of things.
[4]Hard work, they say, is the father of fame.

One thing that I have not abbreviated are the mostly classical quotes at the beginning of each section. I am not particularly erudite, and my knowledge of the classical languages is very limited. All the same, I find that the ancients were able to speak with a force and concision that we have lost today. I also think that it is important to stress that the past has important lessons for the present. Voltaire remarked that who does not learn the lessons of history is condemned to repeat it.[5] He should have pointed out as well that no one has ever learned the lessons of history.

In many ways the present work is a companion piece not only to the proceedings version of the keynote address but also to my 2001 Brouwer lecture [2], printed (with slight abridgment and some updating) at the end of this issue. That work concentrated on the history of the development of the QUEST algorithm, the technical accomplishment for which I am best known, between August 1977 and October 1978. Some overlap with the keynote address is unavoidable, but I have tried to keep it to a minimum. The present work adheres much less to the Aristotelian unities of space and time and covers also a much broader range of topics than did my Brouwer lecture. For good or for bad, both works are very personal.

I hope that the topics I discuss will please my readers. Naturally, any opinions I express, and there is much of that, are mine alone.

The Youngest Quadrant

Slow are the beginnings of Philosophy.
Henry David Thoreau (1817–1862)

Les commencements ont des charmes inexprimables.[6]
Jean-Baptiste Poquelin (Molière) (1622–1673)

When I first began working in the area of Spacecraft Attitude Estimation in the late 1970s, the field was in a very underdeveloped state. This is not surprising, since the space age, which had begun with the launch of Sputnik (October 4, 1957), was not yet two decades old. Clearly, Spacecraft Attitude Estimation as a field was younger still. Even more interesting is the fact that Attitude Estimation was largely undeveloped even without the qualifier "spacecraft."

If I examine the four quadrants of Astronautics, which I divide as either Dynamics or Estimation and as either Orbit or Attitude, and look at who and when the early important work was done in each quadrant,[7] I find a curious result (see Table 1).

My table is simplistic, to be sure. Newton, Kepler, and Euler certainly had their antecedents, and the development of Mechanics by Newton was the culmination of a long and continuous process [5]. Most striking, however, is that while three of the quadrants are populated by several intellectual giants of old and can boast of 250 or more years of development, the south-east quadrant is sadly deserted. There were, apparently, no eighteenth- or nineteenth-century contributors to attitude

[5]"Celui qui ne connaît pas l'histoire est destiné à le répéter," François-Marie Arouet (Voltaire) (1694–1768). The quotation may be apocryphal. It has been ascribed also to Marx (Karl, not Groucho).
[6]Beginnings have inexpressible charms.
[7]Despite his enormous contribution to Mechanics in general, the reader may be surprised that I do not cite Hamilton as a founding father of Attitude Dynamics. This is because he never wished to understand the connection between quaternions and rotations [3, 4].

TABLE 1. Founders of Astronautics

	Dynamics	Estimation
Orbit	Newton (1642–1727) Lagrange (1736–1813) Hamilton (1805–1865) Einstein (1874–1955)	Kepler (1571–1630) Lagrange (1736–1813) Gauss (1777–1855)
Attitude	Euler (1707–1783) Cayley (1821–1895)	(gone fishin')

estimation of even modest calibre. In May 1977, when I formally entered the Astronautics community, there was one lone young university professor, John Junkins, who had just begun to publish some work on attitude estimation, but the field would not become his focus.[8]

There is a good historical reason for the emptiness of my quadrant. During the great classical period of Mechanics and Astronomy there was simply no great problem in Attitude Estimation waiting to be solved. There was no equivalent to the Kepler problem to entice a Newton, no equivalent to the problem of the orbit of Ceres for a Gauss of attitude estimation, no equivalent to the problem of the spinning top for an Euler or a Cayley. The closest one came to Attitude Estimation in those halcyon days was the problem of determining the libration of the Moon, hardly a great watershed of Physics, which must have required some measurement of the deviation of the orientation of the Moon from its mean orientation relative to the Earth-Moon line. But this example was too primitive to really be called attitude estimation in the way that Lagrange and Gauss truly did orbit estimation. Thus, there is no great hoary founder of attitude estimation. My field would have to depend on rather more puny personages for its advancement, people like you and me.

The earliest spacecraft, beginning with Sputnik, had no attitude determination system[9] (ADS) at all. They were little more than metal beachballs with an omnidirectional transmitter, beeping down on us from low Earth orbit. The next stage of spacecraft development had attitude control systems but did not have attitude determination systems. For attitude information one simply relied on the attitude control system to do what it was supposed to do. One of the earliest attitude control systems simply employed a switchable but otherwise permanent electromagnet, which would more or less align an antenna with respect to the local vertical near the North Pole, where it was most needed. At best, an attitude determination system consisted of a primitive Sun sensor, hardly more than a solar cell, to provide crude information on the Sun angle, and a magnetometer to verify antenna pointing at the North Pole. The outputs of these sensors were sufficient to compute a single-axis attitude, that of the antenna. These techniques were developed for the first real satellite application, the Navy's Transit navigation system (also called NAVSTAR), built and supported by The Johns Hopkins University Applied Physics

[8]This was fortunate for me, else there would have been nothing for me to do.
[9]By attitude determination system I mean sensors or sensors plus software; by attitude estimation, software alone, i.e., the algorithms.

Laboratory[10] (APL), which began to be launched in 1961. After the first few, these employed gravity-gradient stabilization to keep the antenna pointed Earthward over the entire orbit. The Navy's desire to verify the pointing of the Transit spacecraft led to the inclusion of a vector Sun sensor and a vector magnetometer in the on-board hardware. This was the first three-axis attitude determination system. The first closed-form three-axis attitude estimation algorithm, invented for the Transit system by Harold Black and published in 1964 [6], was also noteworthy.[11] This was the algorithm which has come to be known as the TRIAD algorithm[12] [7], certainly the first great achievement of Spacecraft Attitude Estimation.[13]

For near-Earth spacecraft very important contributions in the 1970s to Spacecraft Attitude Estimation came from NASA Goddard Space Flight Center, especially from Paul Davenport and Eugene J. Lefferts, both mathematicians originally, whose names will appear again later in this section. These two people were responsible not only for devising algorithms for attitude estimation but also for producing the ground-support software for the spacecraft. A large part of the software effort was carried out by contractors, of which the largest were IBM Federal Systems Division until 1970, and beginning in 1970 (and for a very long time afterward) the Computer Sciences Corporation. Roger Werking was the person in charge, in particular, of attitude ground operations at NASA/GSFC as well as of a very large fraction of the ground-support software. Though not himself a creator of new attitude estimation methods, he was, nonetheless, responsible for the creation of many of these through his contractors. The QUEST algorithm was created in this way. Roger Werking's importance for the development of attitude estimation methods in the 1970s and 1980s was very great. Davenport, Lefferts, and Werking all had an enormous influence on my own development in Spacecraft Attitude Estimation, and Lefferts, in particular, on that of Markley.

Deep-space missions began about the same time at the Jet Propulsion Laboratory. The attitude estimation algorithms for these were more primitive even than TRIAD, owing to the need for attitude to be calculated on-board autonomously by a computer less capable than those in our microwave ovens today. The earliest algorithms simply solved algebraically for the Euler angles from the smallest possible data set, as did the geometric method described in Wertz [8] by Lerner [9]. High mission costs, the extreme need for reliability, and the limitations of early on-board

[10]The Johns Hopkins University Applied Physics Laboratory, under special contract to the Department of the Navy, played a very significant role in the early development of Space Technology. Not only was it instrumental in the development of spacecraft-mediated geolocation with the Transit system, it was responsible also for the introduction of gravity-gradient stabilization as an attitude control method and magnetic spacecraft attitude control. The name associated with all of these early developments (except Harold Black's algorithm) is Robert E. Fischell, who went on after a long and fruitful career in space to one in biomedical engineering, developing a self-charging pacemaker, an artificial insertable insulin pump, an arterial stent, and (my favorite) an artificial sphincter muscle. The Air Force was also engaged in early space work, and NASA's Marshall Space Flight Center grew out of the Army's (and Wernher von Braun's) rocket program at the Redstone Arsenal. Unfortunately, much defense-related space work was and remains classified, and our history must depend largely on published work. By the same token the even closer connection of the military to the Soviet space program impedes our presenting an account of Soviet achievements in Spacecraft Attitude Estimation.

[11]It should be noted that Kalman filter work on spacecraft attitude determination had already begun by this time, but practical consequences would not appear for another fifteen years (see below).

[12]A mathematical presentation of the TRIAD algorithm and other algorithms prominent in this section can be found in reference [2], elsewhere in this issue.

[13]Great as this achievement may have been, Black had a great deal of difficulty getting his method published by the American Institute of Aeronautics and Astronautics (AIAA). At first the editor was unconvinced that it was of any importance. Then he objected to the length of the paper (about four journal pages). Finally, he allowed the publication of a technical note a page and a half in length.

computers were responsible for the lack of algorithm development at the Jet Propulsion Laboratory as noted in reference [2]. The great success of the Mariner, Voyager, and Pioneer missions speak for the soundness of JPL's judgment. The chief contributor to these early JPL algorithms was Hiroshi Otake. Once on-board computer resources improved significantly in the 1990s, the sophistication of attitude-determination systems for deep-space missions also increased dramatically. By the mid 1980s the simple but limited deterministic algorithm, the best that could be accommodated on the limited computer resources of the 1960s and 1970s, had been replaced by QUEST and a Kalman filter.[14] The availability of CCD star trackers also played a large role in this transition.

The next important advance came from an unlikely source. In 1965 Grace Wahba, a graduate student in Statistics at Stanford University, was working at the IBM Federal Systems Division in Palo Alto, California.[15] Here she posed her famous problem [10]. The immediate responses were not of much practical interest [11],[16] but the problem would eventually become the cornerstone for much of "modern" Spacecraft Attitude Estimation and the algorithms for a huge number of spacecraft attitude determination systems for the past quarter century.

Almost as soon as the Kalman filter was invented in 1960 it was applied to research in spacecraft attitude estimation [12]. The naïve wish to be able to estimate spacecraft attitude accurately from very inaccurate sensors had obvious, if hopeless, appeal. The earliest publication was by Farrell (1964) [13, 14] who used Euler angles for both prediction and update and had assumed torque-free dynamics for the prediction. A few years later (1969), Toda, Heiss, and Schlee [15], in their Space Precision Attitude Reference System (SPARS) used the quaternion for prediction and incremental error angles for the update. In addition, they carried out the prediction step of the filter using gyro outputs rather than relying on torque models. This was the pattern for almost all future applications of the Kalman filter to the estimation of spacecraft attitude. The Kalman filter of the Precision Attitude Determination System (PADS) of Farrenkopf and Iwens (1971) [16] was very similar to the SPARS filter but simply truncated the quaternion in the update step. The PADS filter has the distinction of being one of the first Kalman filters to be incorporated successfully in a spacecraft, namely the HEAO series in the late 1970s, but only as part of the attitude control system, not the definitive attitude determination system, which was ground-based. The Kalman filter would not become a successful part of specifically attitude determination systems until the 1980s.

The U.S. manned space program, which began in 1962, concentrated on the development of more accurate and more reliable attitude instrumentation rather than on algorithm development. The Mercury (1959–1963) and Gemini (1963–1966) programs relied on three-axis gyros for attitude determination with horizon scanners providing additional roll and yaw information. In the Apollo program (1961–1972), for which successful manned flights began in 1968, sightings of two stars using a star telescope were used to realign the three-axis IMUs (inertial measurement units, i.e., three-axis gyros), which provided continuous three-axis

[14] I'm getting ahead of myself here, but a strictly chronological history would be less interesting.
[15] Note that my Brouwer lecture, as presented in 2001 [2], offered incorrect information for the nature and location of Dr. Wahba's employment at the time she posed her famous problem. See the revised and corrected version of my Brouwer lecture [2] elsewhere in this issue for further details.
[16] Except for the most important achievements, I will cite only secondary sources as much as possible in order not to overburden this section with references.

attitude determination. For the Skylab space station (1973–1979), the attitude determination system consisted of three-axis gyros with gyro drift corrected by regular updates from two star trackers and a fine Sun sensor. The gyro-star-tracker system was the basis for attitude determination for the Space Transportation System (the Space Shuttle), which began launch in 1981. Like all previous manned missions, the Shuttle flew on gyros, but a reset of the gyros was actuated every twelve hours based on data from two star trackers. The Shuttle IMUs employ the spinning-mass gyros of 25 years ago. Despite the age of the technology, these have provided excellent accuracy and have proved to be very reliable. The American-supplied attitude determination system for the International Space Station (ISS, in operation since 2000) has three redundant three-axis GPS attitude determination systems as well as redundant three-axis IMUs. Gyro resets are accomplished with a Kalman filter. The parallel Russian-supplied attitude determination system for the ISS replaces the GPS attitude-determination units with a large assortment of star-trackers, infra-red horizon scanners, Sun sensors, three-axis magnetometers, and three-axis gyros to provide continuous three-axis attitude (presumably via a Kalman filter) with an accuracy of about 0.5 deg/axis.

By the mid 1970s, the Apollo Mission was over, and we (including the Soviets) had sent the many Mariner, Voyager, Pioneer, and Venera spacecraft to Venus, Mars, Jupiter, and Saturn. Hundreds of spacecraft had been placed in Earth orbit. Man had gone to the Moon! The great romantic triumphs of the space age had been accomplished. For attitude estimation in the early 1970s the focus was more on the refinement and gradual improvement of algorithms. The late 1970s were a transitional period. Several factors were important to that transition. First, there was the general improvement in attitude sensors, especially the introduction of the NASA standard star tracker (the Ball Brothers CT-401) in the SAS-3 mission (launched 1975), which led to a greater focus on three-axis attitude estimation, rather than on spin-axis attitude estimation, which was all that was required for most earlier unmanned spacecraft. Mission requirements were also increasing, leading to the need for more optimal (with respect to achievable accuracy) and more efficient algorithms. Lastly, there was the microchip revolution, just getting underway in the late 1970s, and which would change everything in the 1980s. The lack of computing power in the 1960s and 1970s was, possibly, the chief impediment to the development of Spacecraft Attitude Estimation (see also reference [2]).

The limits of three-axis attitude determination three decades ago can be observed most clearly in the justly celebrated book edited by James R. Wertz, *Spacecraft Attitude Determination and Control* (*SADC*) [8], written by 35 employees of the Attitude Systems Operation at the Systems Sciences Division of the Computer Sciences Corporation (CSC), in Silver Spring, Maryland. *SADC* has been an essential reference for Spacecraft Attitude Estimation since its first appearance in September 1978. What attitude estimation can be found in its nearly 900 pages, however, is almost entirely spin-axis attitude estimation. F. Landis Markley, one of the major contributors to *SADC,* has a section of not quite eleven pages on the attitude representations [17], which served the attitude community well for more than a decade and twenty-five years later is still a sufficient reference for most of that community. Gerald M. Lerner has a section on three-axis attitude determination [9] (only eight pages) in which he summarizes nicely the three-axis attitude estimation technology of the day (only iterative least-square estimation is absent, to appear later in *SADC* [18]) and gives a succinct account of the geometric method, the

TRIAD algorithm[17] and Davenport's q-method. And that's it. The chapter in Wertz on "State Estimation Attitude Determination Methods" contains no three-axis attitude estimation at all. The attitude Kalman filter receives very short shrift in *SADC* [18] (the SPARS and PADS work (see above) are not even cited), a reflection of the fact that Kalman filtering had not yet proved itself to be a practical technique in applications to spacecraft attitude determination. In fact, early naïve attempts to utilize the Kalman filter in spacecraft attitude determination mission support in the mid 1970s had led to divergences and disaster. The practical application of the Kalman filter to attitude estimation would not come until several years after the book's appearance. Until then, Kalman filtering of spacecraft attitude was mostly of only theoretical interest, although this interest was keen.[18]

This deemphasis of three-axis attitude determination in *SADC* is a reflection also of the particular experience of the book's authors, who wrote almost entirely about their experience supporting missions for NASA Goddard Space Flight Center in the 1970s,[19] most of which, as I have said, employed spinning spacecraft.[20] This situation was already changing when I joined CSC. During my tenure there, most of it after *SADC* had appeared, nearly all of the spacecraft I supported required the determination of three-axis attitude.[21] Nonetheless, *SADC* constituted my education in Spacecraft Attitude Determination, and for a year and a half before its appearance I devoured voraciously a succession of typewritten manuscripts and proofs.

SADC was a solitary light shining in the darkness of the youngest quadrant, containing much information on sensors, data analysis, what little was known of batch three-axis attitude estimation, and, of course, spin-axis attitude estimation. In it you will find a reference to Markley's work on Attitude Control but none of his innovations in Attitude Estimation. They were soon to come. For my own work you will find only a single sentence at the end of Lerner's section on three-axis attitude determination: "Variations on the q-method which avoid the necessity for computing eigenvectors are described by Shuster [1978a, 1978b]." This, of course, was the QUEST algorithm [7], my very first task in attitude estimation, executed with much clumsiness and trepidation [2], and which would likely not have received mention at all had I not been working at CSC. In any event, my contributions to Attitude Estimation weren't worth more than a single sentence at the time.[22]

[17]Called the "algebraic method" in reference [8], and sometimes the "Sun-Mag algorithm" (for obvious reasons) in internal reports. I am responsible for the name TRIAD [7].

[18]The aversion toward implementation of the Kalman filter in attitude mission support by the attitude operations section of NASA Goddard Space Flight Center (GSFC), the result of some bad experiences, has been mentioned many times in reference [2]. Several theoretical studies were supported by other sections of that organization. My work on the Kalman filtering of spacecraft attitude began as a NASA/GSFC study task on the Landsat-D spacecraft then in development.

[19]The last additions to the manuscript were in June 1978.

[20]The impetus for *SADC,* in fact, had been to create a handbook for NASA/GSFC attitude support, which explains its particular emphasis.

[21]Spin-axis attitude estimation, nonetheless, is always an important component of early mission support for unmanned spacecraft. Generally, at the time of orbit injection, unmanned spacecraft are spinning, and the estimation of the spin-axis attitude is a necessary step in the process of attitude acquisition. Often, the sensors for three-axis attitude determination cannot be activated or employed effectively until the attitude acquisition process has been completed.

[22]The single-sentence insertion was made only in final proof corrections to *SADC,* soon after QUEST was unveiled at CSC in April 1978 [2]. The many bells and whistles that made QUEST such an attractive attitude estimation tool would not appear until after the publication of *SADC.*

Perhaps, the most important theoretical advance of the transitional period[23] was Davenport's invention of the q-method (developed in 1977 but never published by him.[24]) The q-method was the father of QUEST (1978) [7], which has become the most popular algorithm for batch three-axis attitude estimation. Davenport's q-method begat also a host of still later batch algorithms for three-axis attitude estimation [11]. Markley's very significant SVD algorithm (1988) [11] and his FOAM algorithm (1993) [11], however, are the immediate descendants of Wahba's problem and do not use quaternions (although there is other inheritance from QUEST). Attitude Kalman filter research essentially marked time in the late 1970s. A very important paper in that area was the attitude Kalman filter of Murrell (1978) [19], which was the direct descendant of that of Toda et al. (1969) [15], and one of the antecedents of the 1982 paper of Lefferts et al.[25] [12]. The principal contribution of Lefferts et al. [12] was to package the existing attitude Kalman filter work nicely, to fill in some of the blanks, and to review the work of the previous two decades, tasks which it carried out sufficiently well that earlier publications on this topic are seldom cited now. It is interesting to note that Murrell's Kalman filter work and QUEST had their first public presentations as adjacent papers in the same session of the 1978 AIAA Guidance and Control Conference,[26] just one month before the appearance of *SADC*. The year 1978, it would seem, was pivotal for Spacecraft Attitude Estimation.

It is important to stress the significance of the Attitude Systems Operation at the Computer Sciences Corporation Systems Sciences Division for the development of Spacecraft Attitude Estimation from the early 1970s until the end of the millennium. In an era when a great many Ph.D. scientists and mathematicians were looking for jobs outside academia, CSC discovered that these highly-educated but possibly misdirected individuals, the author among them, made excellent analyst-programmers. Roughly half of the 50-man CSC Attitude Analysis Department[27] had Ph.D.s, especially in Physics.[28]

Ph.D. physicists are raised in a culture of teaching, research, and publication, and those inclinations do not necessarily desert them when they enter industry. Given the more than a half-dozen missions in active development at any one time in the 1970s at NASA Goddard Space Flight Center, the principal customer of my CSC division,[29] the need for improved attitude estimation methods, and the special character of the technical staff, it was inevitable that CSC would be drawn into attitude estimation research.[30] *SADC* was certainly a response to two physicist needs.[31] The

[23]It is hard to pin down exact or even consistent boundaries for the transitional period. Davenport's q-method (1977) was certainly the starting point for the modern algorithms, but *SADC* was a point of closure for the early period as well as a gateway to the current period. In a way, so was Lefferts et al. (1982) [12].

[24]The earliest archival publication of the q-method was by Lerner [9], the next by me [7], which was also the first publication in a journal. See also footnote 26 of reference [2] in this issue.

[25]The most significant contribution of Murrell's paper was to present details of the noise propagation. It is through reference [12] that this noise propagation scheme reached general notice and has since become popular.

[26]Markley's work on the Solar Maximum Mission (SMM) control laws was also presented in that session.

[27]The entire Attitude Systems Operation boasted around 150 employees while I was there. The CSC Systems Sciences Division's total support of NASA Goddard Space Flight Center (including orbit, scientific, and many other activities) comprised over 1000 people in the 1980s. One-hundred-and-fifty people was a huge number in one place to be working on spacecraft attitude estimation, unequaled, perhaps, anywhere in the world or since.

[28]Three quarters of the sections of *SADC* were written by Ph.D. physicists.

[29]Which in 1970 had won the NASA/GSFC mission support contract previously held by IBM Federal Systems in Bethesda, Maryland.

[30]Less so for attitude control, which was largely the domain of the spacecraft prime contractor or NASA.

[31]At CSC, *SADC* was generally referred to as "the textbook."

job also required that CSC analysts be involved in software development as well as launch and early mission support. Thus, CSC provided an excellent practical education for its attitude analysts and a research-minded environment as well. The rest you know. CSC continued to be an important contributor to the development of Spacecraft Attitude Estimation throughout the 1980s and 1990s, a remarkable achievement for a company that did not itself build spacecraft. The many graduates of "CSC College" continue to contribute strongly to the development of many areas of Astronautics.[32]

Following the publication of *SADC,* the pace of research on Spacecraft Attitude Estimation accelerated dramatically, in no small part because of that book. The 1980s and early 1990s were a period of rapid growth in the development of Spacecraft Attitude Estimation with further exploration of the Wahba problem and Davenport's q-method and the application of the new techniques to more practical problems like spacecraft sensor alignment estimation and attitude sensor calibration. In the 1980s and much of the 1990s it would seem that Markley and I provided most of the new methodology in Spacecraft Attitude Estimation. Our work has not abated, but beginning in the 1990s the number of active people in this field has increased greatly, and we contribute now a much smaller fraction.

The early 1990s saw the development of a totally new attitude determination technique based on the Global Positioning System (GPS) [20]. GPS, for which launch began in 1978, was designed as a replacement for the aging Transit/NAVSTAR system, which continued to function until its retirement in 1996. The sensing of phase differences between three GPS antennae makes three-axis attitude estimation possible without recourse to other sensors. The level of accuracy (arc minutes) is intermediate between that of a modern arc-second-level star-tracker-gyro system (arc seconds) and the coarse three-axis attitude determination systems consisting of a three-axis magnetometer, an Earth horizon-scanner and a coarse vector Sun sensor, which provide a typical accuracy of 0.5 deg/axis. GPS attitude determination began to be employed on unmanned spacecraft in 1996, and, as noted earlier, is part of the International Space Station as well.

For the past decade, the development of Spacecraft Attitude Estimation has been largely of a theoretical nature, possibly because the basic practical applications have been largely exhausted. The arena for the theoretical development of the field has begun to drift toward academia. Practical high-accuracy spacecraft attitude determination, for the most part, seems to have converged on a single approach: a fixed-head star tracker and three-axis gyros, with the single-time attitude being computed (frequently in the star tracker) using QUEST [7] and, if greater accuracy is desired, the QUEST quaternions filtered using a technique developed almost twenty years ago, first in Brazil [21] and later (independently) in the U.S. for any single-time attitude estimate [22]. This last advance has become standard for the attitude support of deep-space missions.[33] Over the past decade, there has been an

[32]Former employees of CSC's Systems Sciences Division in the 1970s include five recipients of the American Astronautical Society's Dirk Brouwer Award (one of whom has also received the equally prestigious AIAA Mechanics and Control of Flight Award), a former director of the National Space Program Office of the Republic of China (Taiwan), and the current administrator of NASA.

[33]This approach was implemented, for example, for the near-Earth MSX spacecraft (launched 1996) and TIMED spacecraft (launched 2001) and for the deep-space missions: Galileo (launched 1989) to Jupiter, Magellan (launched 1989) to Venus, NEAR (launched 1996) to the asteroid Eros, Mars Pathfinder (launched 1996), Cassini (launched 1997) to Saturn, Messenger (launched 2004) to Mercury, and, most recently, on the New Horizon spacecraft (launched 2006), which will be our first spacecraft to rendezvous with Pluto.

explosion of batch three-axis attitude estimation algorithms based on Davenport's q-method, due mostly to Daniele Mortari [11], who seems to be the most active "wahbateer" these days.

More than any other quadrant, the early development of Spacecraft Attitude Estimation had been an American enterprise, though not necessarily by Americans. The reasons are clear. The development of this quadrant did not begin until after World War II, when there were only two superpowers with sufficient resources to support a large space program. The Soviet Union accomplished remarkable feats in space, often the first to do so. However, the development of a mathematical discipline like Spacecraft Attitude Estimation relies very much on free communication within a very large and geographically diverse group. The strong connection of the Soviet space program to the military and the high degree of secrecy which that entailed (even between different groups in the Soviet space program, which engaged in fierce competition), as well as the crushing bureaucracy, all militated against the necessary high intensity of intellectual exchanges during the Cold War, giving a huge advantage to the development of Spacecraft Attitude Estimation in the United States.[34] What algorithmic treasures lie hidden in classified Soviet journals and reports we may never know, and they could not participate in the general development of my quadrant. Since the end of the Cold War, the prominence for a long time of American institutions, particularly its international space journals, has had the good effect of providing an intellectual focus for my quadrant and good communication between its practitioners in all parts of the world. Very much now the astronauticists in my quadrant are a single family. May it always be so.

Spacecraft Attitude Estimation is now a mature field and an important component of virtually every professional meeting on Astronautics. There are now many very active space programs beyond those of the United States and the Soviet Union, for example, those of Argentina, Brazil, Canada, China, the European Union, France, Germany, India, Italy, Japan, South Korea, Taiwan, and the UK, to name only a baker's dozen. The number of journal articles on the subject has become substantial, as has the number of university professors who consider Spacecraft Attitude Estimation a fit subject for research. When I entered the field in 1977 with no background beyond a few weeks of rigid-body mechanics while a Physics graduate student, there was little to learn, and one learned it all very quickly. Now, almost 30 years later, it is hard to keep up with new developments. It is noteworthy (and pointed out frequently in reference [2]) that few of the contributions to Spacecraft Attitude Estimation before the 1990s were from engineers. The pioneers of the field were mostly physicists and mathematicians, who became engineers only by slow osmosis, if at all. With the enormous expansion of the field, this is no longer possible, there is simply too much to learn now, and the level of sophistication has also risen

[34]This is not to say that the United States did not classify some research on Spacecraft Attitude Estimation, as can be seen from the noteworthy Symposium on Spacecraft Attitude Determination of 1969 (see reference [15]), which had both classified and unclassified components, though I doubt that the classified attitude estimation algorithms were superior to those in the open literature, certainly not superior to what we have today. It has been my personal experience, from seven years of secret defense work, that in the U.S. it is mostly the hardware and data which are classified and seldom the algorithms, except in obvious areas like Cryptology or Information Coding, or if the algorithms are specific to military hardware. The problem for the Soviets was the deep and narrow compartmentalization of its secrecy. In the area of Communications Theory, the Soviets, who had a great superiority in this field, surprised the West by publishing a textbook in the 1970s, which contained much information that was classified here, from which we may infer that the Soviets had been teaching openly in their graduate schools what we had long been guarding secretly. Openness pays.

dramatically. The physicists and mathematicians one encounters in Spacecraft Attitude Estimation nowadays are mostly members of the old guard. The old guard continues to contribute a large fraction of the research papers on Spacecraft Attitude Estimation from government and industry, but it is becoming very old, and it cannot be many years before that contribution ceases.

What is the future of Spacecraft Attitude Estimation? In attitude sensing, certainly, the trend will be toward smarter sensors and greater standardization. Currently, for high-accuracy attitude determination, the system of choice consists of a three-axis gyro package and a star tracker capable of observing 25 or more stars simultaneously and having as output the single-time quaternion, frequently imbedding the QUEST algorithm or a homologue in its internal dedicated microprocessor. The task of the spacecraft computer, among many other things, is to integrate these two units into a larger attitude determination system in which the single-time estimates from the star tracker are refined using the gyro outputs and a Kalman filter. Thus, one essential part of the process, the computation of the single-time attitude estimate, is frequently part of an off-the-shelf hardware item and no longer the domain of the attitude mission analysts.[35] At some point, perhaps, manufacturers will begin offering off-the-shelf systems incorporating the star tracker, the gyro system, and the Kalman filter. That may not happen for some time. For one thing, the manufacturers of star trackers do not manufacture gyros, but, I believe, the time may come, especially if the appropriate merger takes place. This would further reduce the domain of activity of the attitude mission analysts, perhaps not for the good. For coarse attitude determination in low-Earth orbit I suspect that attitude sensing will eventually be dominated by GPS attitude determination systems, partly for convenience but also because GPS attitude-determination systems are not subject to data loss due to Earth or Moon occultation, poor magnetic field models near the poles, Sun interference, or cold-cloud effects. We may never see an integrated off-the-shelf GPS-gyro package for spacecraft, because of the lower accuracy level of a GPS system, but all things are possible. It is a more likely occurrence for aircraft because of their greater numbers and greater need for standardization. For the moment we who work in space need to gain more experience in implementing GPS systems. Some aspects of GPS support, such as the resolution of phase ambiguities, are still active areas of research. Innovation will likely mitigate the complete migration of attitude analysis to the manufacturers of attitude sensors. All the same, greater standardization in spacecraft attitude determination systems is inevitable.

Ultimately, research in Spacecraft Attitude Estimation may become mostly an academic activity, having little effect on flight hardware or software or on mission planning. In part, this is inevitable because Spacecraft Attitude Estimation has become a far more sophisticated field than it was in the 1960s and 1970s, requiring greater mathematical ability than in the early days, and even then it was no accident

[35]There is a significant downside to this development, because it means that decisions on the capabilities of the dedicated star-tracker software may no longer be made by the real experts in attitude estimation. As an example, at this writing, after more than a decade of availability of CCD star trackers, no dedicated star-tracker software computes the star-tracker attitude information or covariance matrix, which is necessary for optimal filtering of the star-tracker attitudes or for combining them with attitude data for other sensors. The data needed to calculate these matrices outside the star tracker cannot always be exported efficiently in real time to the spacecraft computer or for telemetry.

that the most innovative attitude determination analysts were often crossovers from academic Mathematics and Physics. The time when mission analysts could simply concoct an heuristic seat-of-the-pants algorithm for attitude data processing without attention to rigor has passed, although, I regret, not completely. Unfortunately, what is passing also is the connection of researchers in spacecraft attitude estimation to day-to-day responsibility for real missions, with obvious consequences. The great achievements of the early years of attitude estimation, Black's (TRIAD) algorithm (1964), Wahba's problem (1965), the Toda-Heiss-Schlee attitude Kalman filter (1969), and Davenport's q-method (1977), were the product of mission needs. We must be watchful that the next generation of mission analysts in industry and government not rely too heavily on smart sensors to do their work for them, and we must be watchful also that university research on attitude estimation not become focused on the optimization of Lebesgue probability measures on compact Lie groups.

The one saving grace of predictions, fortunately, is that they are often incorrect, especially in the long term. New needs and new sensor technologies will certainly arrive eventually, which will keep the field alive. A new national leader may lead us back to our early glory of the 1960s. We may take heart in the fact that while the better is often the enemy of the good, it is also the enemy of the mediocre. I have often heard it said that everything has been done now in attitude estimation except for highly theoretical work and small details. Similar remarks were made about Physics at the end of the nineteenth century. Don't believe it.

This is about all that one can write on the history and future of Spacecraft Attitude Estimation without discussing the detailed contents of publications. Attitude Estimation is in many ways a street waif, of ambiguous origin and growing up much too quickly. And yet it has turned into a handsome adult, highly sophisticated, and a joy to behold. It was a joy for me at the symposium to see so many of the people who have made Spacecraft Attitude Estimation into such a vigorous field.

The Arts in the Teaching and Practice of Engineering

τῷ σοφῷ ξένον οὐδέν.[36]
Antisthenes (444–371 B.C.E.)

γηράσκω δ' ἀεὶ πολλὰ διδασκόμενος.[37]
Solon (ca. 640–558 B.C.E.)

QUALIS ARTIFEX PEREO.[38]
Nero Claudius Cæsar Augustus Germanicus, Imperator (37–68)

We are often told, almost always by non-engineers and non-scientists, that instruction in the Liberal Arts and the Fine Arts should be an important part of Engineering education. The position of the non-technical person is that a liberal education will make engineers better people and better able to understand and

[36] To a wise man nothing is foreign.
[37] As I grow old, I ever learn many things.
[38] What an artist dies in me. (Literally: what an artist I perish.)

respond to the needs of society. American Engineering students, however, are unconvinced and mostly greet such assertions with ridicule and scorn. Both sides are wrong. Exposure to the Liberal and the Fine Arts is important to the education of engineers, but not just to make them better members of society. Such exposure is important, because it will make them better engineers.

There have been many studies of the value of the Arts in primary and secondary education, particularly as a motivational device, and there have been even experimental studies that show that intense exposure to Art develops superior spatial-visual coordination and other basic skills. I'm not sure how much this will influence the generation of secondary-school students which asks, like the illiterate dance student in the 1980 Alan Parker film *Fame,* why he must read the book when he can watch the video. I have no interest here in Art as a motivational device or as an aid in neurological development, valuable though these aspects of Art education may be. What I wish to discuss are the premises that creation in Art and creation in Engineering have much in common, and that the study of Art in *tertiary* education demands creative participation on the part of the student, while the study of Engineering most often does not.

Contrary to the misconceptions of most non-technical people and, probably, of very many Engineering and Science students, Engineering, Science and Mathematics are not purely deductive disciplines. Innovation in these disciplines, certainly, is not. Deduction tells us nothing about how we will determine the assertions we wish to deduce or by what deductive path we will deduce them from basic principles. At a still higher level, deduction does not tell us how even to choose our basic principles. Deduction is important to our discipline, because we cannot accept a theoretical assertion which cannot be deduced from basic principles. An inability or unwillingness to carry out the deductive process fully and rigorously often leads to very bad research. I have seen this many times. But deduction is probably not the most important activity of Engineering research. The most important activity is *induction,* how we determine the things we want to prove (or discover or design or invent)[39] and how we are going to prove, etc., these things from observation, analogy, and the magical element we call intuition. Unfortunately, we cannot teach induction, senior-year projects to the contrary. So we teach our Engineering and Science courses as deduction with an occasional dollop of historical and physical motivation, and we give the misimpression to our students and to the world that ours is primarily a rational deductive discipline.[40]

The other misconception, of course, is that Science and Engineering deal with immutable truths, which also is not exactly the case. We believe that such truths exist, of course, but not that we necessarily know them. In Science and Engineering we never have a complete theory and often we have to revise our ideas as our experience increases. The majority of people in our world do not understand this. Far too large a fraction of our population, when a scientific idea turns out to require revision, thinks this justifies the belief in such absurdities as intelligent design, creationism, or astrology. The true world of Science and Engineering is cloaked in ambiguity and doubt. Our job is to find the best approximation of truth amidst that ambiguity.

[39]Stated in other words, the most important part of research is not finding the solutions but finding the problems.

[40]For the largest segment of our successful Engineering graduates, it is even sufficient that they be good deducers, since their work will consist largely of the application of existing methods or in very repetitive experimentation.

How do we make our students (and ourselves) better inducers and better able to deal with ambiguity? Here, I believe, the best laboratories are the Liberal Arts and the Fine Arts. Serious novels, poetry, paintings, sculpture, etc., are not quantitative, and their art comes in no small part from ambiguity. My claim is that the Liberal and Fine Arts accustom us to working amidst ambiguity and uncertainty. This is not to say that professors in the Liberal Arts or Fine Arts can *teach* us how to deal with ambiguity or how to be inductive. But they can expose us to many tantalizing examples of ambiguity, and to a lot of sensations and to forms of perception which don't exist in the realm of Science and Engineering. As engineers and scientists, we generally brush off the Liberal and the Fine Arts, because they are not quantitative and lack any kind of repeatability, but this is insufficient justification for neglecting them.[41] The truths of the Arts are not quantitative, nor are they universal principles, but the experience of increasing our understanding through examination and reexamination is more accessible in the Arts than in Science and Engineering. The appreciation of a work of art is truly an inductive experience. Regretfully, my efforts to cultivate greater contact of engineers with the Arts have yielded only bitter fruit. I always told my Engineering students that they would become better engineers if they would read a poem every week, especially a non-narrative poem. You can imagine their response.

When we confront our students with a work of art, whether it be a (non-narrative) poem, or a (non-representational) sculpture or painting, that experience requires a creative (and often inductive[42]) effort on their part, not the creative effort of the artist who created these works, but creative nonetheless. The student must somehow go beyond the superficial form in order to appreciate the work. It is not simply a bunch of words, notes, colors, lines, curves, and surfaces. An artistic painting or photograph is not the same as a snapshot from the beach. A novel or a poem is not the same as a newspaper article or a product description. A sculpture by Michelangelo is very definitely not the same as a mathematical description of the surface of the statue. The key to appreciating Michelangelo will never lie wholly in an atlas of diffeomorphisms describing its shape. Some students, whether would-be engineers or humanists, cannot "get" what a work of art is all about, and we are unable to teach them how to "get" art. At best they learn a lot of vocabulary and how to recognize different periods or different artists, but they never truly experience art. They can only repeat what they have been told. The first person to say "your eyes are like the stars" was a poet. The first person to repeat it was trite. It is that way in Science and Engineering too. Perhaps many of our Engineering students and many of our liberal arts students are like this. Engineering and Science research, I claim, are the more difficult professions. In some ways, significant research in Science and Engineering, not simply repeating experiments for different substances or applying the same technique over and over again to different systems, is more demanding than "artistic creation," because one must be able to do two things well. Einstein, a poet of Physics, also played the violin and made amateur art films. Akito Arima, former president of Tokyo University and a notable nuclear physicist, is one of Japan's better known *haiku* poets. Few poets of the English language can solve even elementary Mathematics or Physics problems.

[41]Actually, there are repeatable truths related to the Arts, which come from the application of Science to the Arts, as in the computer-aided statistical analysis of literary texts, but this is repeatability in Science rather than in Literature. What makes literature art is lost in the noise of such analyses.
[42]Creativity and inductiveness are not synonymous, although creativity in Engineering is usually inductive.

Art is not the only part of the Liberal and Fine Arts which has this benefit. I claim that so does the learning of a foreign language, especially a language very different from our own, whose vocabulary may have different semantic ranges and whose grammar may exhibit very different morphological and syntactic forms from our own, and which may differentiate words according to conceptual categories which most native English speakers find beyond exotic. Languages most separated from ours either in space or in time (or both) are likely the best. The puzzling out of meaning in such languages is not a very different exercise from puzzling out the meaning of Engineering results, and, certainly, it is a constant exercise in induction. I note with satisfaction the belief stated by many pundits that much of Mathematics is simply good grammar. Listening attentively to serious music, reading serious literature seriously, examining works of pictorial or plastic art, or even watching foreign films, all have something to contribute to our education as engineers. Better still is to engage oneself in the creative side of the Arts, however poor the results.[43]

Most important of all, we must remember that Engineering research is a creative act, and creation is always an expression of the imagination. Following someone else's mathematical proof or the progression of equations in Engineering or Physics is not an expression of the imagination. Creating that proof or those equations for the first time *was.* (Your eyes are like the stars!) The closest we come in Engineering courses to helping our students learn induction is in their homework problems, but not if these problems are only substitution problems, or repetitions of the text, or carefully guided problems of connecting the dots, which is the general case, especially in the more recent textbooks. If the problems do not require significant non-repetitive effort from the good students, then they are really of no value except to enhance short-term memory. Unfortunately, we opt too often for homework problems which do not require much thought, perhaps, because so few students can be expected to solve any other kind. As in the well-known sports maxim, the *art* of Engineering is learned in the *struggle* to get the answer, not in simply being shown it.[44] The activity of research in Engineering has far more in common with artistic creation or even with the appreciation of a work of art than anything which we usually teach in our Engineering classes. If we share his mother tongue, we might, if we were sufficiently talented, learn far more of an Engineering graduate student's promise as a researcher by having him write a poem or a short story than by examining him on Fluid Dynamics or Finite Element Analysis.[45]

We must also keep in mind that problem solving of any kind, whether it be in Mathematics, Physics, or Engineering, or working out the best fingering or phrasing for playing a piece of music, achieving a desirable rhyme and meter in a poem, developing technique in a sport, creating tension or resolution in music, literature, painting, or the plastic arts, understanding a work or a word in a foreign language, planning a trip, achieving a certain taste and texture in a culinary dish, is valuable in itself. If we focus our attention too strongly on solving problems in only our chosen discipline, in which the range of perception and expression is limited—especially so in Engineering and Science—then we lose suppleness in our thinking,

[43]My own pitiable experience has been that writing a short story feels rather like writing an article in Engineering or Physics, while writing a poem feels like proving a theorem in Mathematics.

[44]Interestingly, the homework in advanced courses in Pure Mathematics are generally of the inductive variety.

[45]This is not to say, as one colleague has already chosen to misinterpret my remark, that we should not test out students on their competence in Engineering but only on their ability to write poetry.

as well as the insights that often come from obscure analogies. The ancient Greeks knew better and saw Art in everything. So should we.

A recent study [23] by the National Academy of Science, the National Academy of Engineering, and the Institute of Medicine has proposed a very large investment in our K-12 and university Science, Mathematics, and Engineering programs in an effort to compensate for decades of erosion. Missing from that program, however, is any effort to improve the education of future engineers, scientists, and mathematicians in the Liberal or Fine Arts. If the purpose of that investment is to enhance innovation in our technical fields, then it may be very short-sighted to invest only in technical areas and exclude those areas which bring us into more direct confrontation with the creative process, with ambiguous concepts and data, and with more diverse avenues of perception. Innovation and creativity are most often the province of those with strong interests outside their profession.[46] Standardization, even at a high level, does not usually lead to greater creativity or innovation. The success of our future scientists and engineers may lie more in a greater diversity and liberalization of our education programs at all levels and in higher standards of performance rather than in standards of technical content.

I do not practice the science of research but the art of research. *Ars gratia artis mechanicae,* Art for the sake of Engineering, to modify the motto of MGM.

Advice to Young Researchers in Astronautics[47]

חכם יכול לילמוד מכל אחד אפילו מתם;
אבל תם אננו יכול לילמוד מאף אחד אפילו לא מחכם.[48]
Hebrew proverb

ῥᾶον παραινεῖν ἢ παθόντα καρτερεῖν.[49]
Euripides (480–406 B.C.E.)

Куда, куда вы удалились, весны моей златые дни?[50]
Aleksandr Pushkin (1799–1837), Evgenij Onegin

Given the somewhat valedictory nature of this talk, it is, perhaps, not inappropriate that I include some advice to younger colleagues based on my experiences good and bad over the years. This section was part of an early version of my keynote address, but I deleted it to save space.[51] This section is not intended as a survivor's manual. It has loftier goals for those young creative researchers who will not only survive but prevail, whether in industry, government, or academia. If a

[46]The four editors of this special issue are a good example: one is a pianist and composer, another has a very strong commitment to serious music and the arts, one was a committed gymnast, and one writes poetry.
[47]Especially to young attitude estimators. Some old and decrepid researchers, however, might also read this with profit, as may researchers in other fields.
[48]A wise man can learn from anyone, even from a fool; but a fool cannot learn from anyone, not even from a wise man.
[49]It is easier to give advice than to bear one's own problems.
[50]Where, where have you gone, golden days of my spring?
[51]The earlier version of this section also cited specific works as examples of bad practices, bad research, and unethical conduct. I will endeavor here, however, to be the Lord Chesterfield of Astronautics research, not the Louella Parsons.

previous section of this work looked primarily at the past of Astronautics, this one hopes to affect its future. Since I devote a very great deal of effort to my publications, it should not be surprising that much of my advice is about these. Some of my advice is specific to Astronautics; some even specific to Spacecraft Attitude Estimation. Most of it is very general.

Work hard and do good work! There is no substitute for this nor compensating factors. Also, to do really good work, you must be a little crazy. As the Romans said: *nullum magnum ingenium sine mixtura dementiae fuit* (there has not been great genius without an admixture of insanity), which need not imply that if you are a bit crazy, you will become a great genius, but it helps. My own career is certainly proof that persistence and hard work can compensate for lack of genius, unless one accepts Edison's definition of genius as one percent inspiration and ninety-nine percent perspiration, although my own ratio is certainly much poorer than Edison's.

Be focused! If there is anything that can be said for my own Astronautics research, it is that it has been sharply focused. To some degree this is a euphemism for its having been very narrow, but no matter; it has given me an area with which everyone associates my name. It is important, certainly, that your research have breadth. If you are just starting out, breadth will get you better job offers, or present you with a greater range of areas in which to obtain research funding. but depth is more likely to get you fame. If you are a young academician, both will help you get tenure. It is important that there be at least one area where you have real depth, although it will take a long time to develop. Except that I once did valuable research in Nuclear Physics and that I dissipate a lot of my time in cultural pursuits, I mostly lack breadth, and that lack has not been helpful to my career in Astronautics.

Errors have their place. The parameterization of measurement errors should almost always be with respect to a frame in which the sensor is at rest. Otherwise, sensor errors may depend unnecessarily on dynamical variables. (This is sometimes unavoidable.) The choice of the best frame for state-vector errors is more complicated but should be such that those errors are most meaningful. For a dynamical system, the parameterization of the "velocity" may be your best guide, since the velocity enters the dynamical equation in much the same way that a state error enters the integrated dynamical equation. In rigid-body dynamics, for example, if the angular velocity is generally referred to the body axes, so should be the attitude errors.

Be true to the group! One should always take advantage of the group properties of the physical variables. They are extra knowledge and will often lead to simpler and more physically meaningful results.

Observe constraints! If you are estimating a quantity that is subject to constraints (a unit vector, an orthogonal or symmetric matrix, etc.), make certain that your choice of parameters enforces these constraints. It may be convenient to put the constraint aside as an intermediate step, but the final result must be a properly constrained solution of the original problem.

Use clear and systematic notation! Do not introduce new notation just to be different. Avoid bad notation choices like using a different letter for the scalar component of the quaternion than for the vector components or for the entire quaternion. The notation q_{13} or $\mathbf{q}_{13}$ for the 3×1 array of the vector components of the quaternion is especially bad, since it could also mean the thirteenth component or vector, respectively. Using 0 for the scalar index of the quaternion is also inadvisable, since not every programming language permits a row or column index to have that value. Always enclosing the symbol for every rectangular matrix in brackets (once standard because of the limitations of typewriters) is also a bad idea, although it can add clarity in special circumstances, as can any other delimiter. Likewise for underscoring every column matrix.

Put not thy trust in drawings! Drawings are very helpful in research. They excite our visual perception of the problem at hand, give us new insights, and are excellent ancillary devices for communication and teaching as well as mnemonics for important results. But drawings are also fraught with dangers, since it is often very difficult to interpret signs correctly from drawings. The misinterpretations can sometimes be very subtle. When my calculation almost "works" except for a sign inconsistency that I think I can fix later, that is usually the time for me to start doing the algebra microscopically. A word to the wise ...

Put not thy trust in others! Never trust a published formula. Always rederive it yourself. Not only do published formulas sometimes contain errors (I have published two errata so far), but we often understand the range of application of a formula only by deriving it ourselves and seeing then the hidden assumptions. If an article is not particularly well written, be prepared that there may be communication problems in the results as well. Always save your notes.

Thinking is better than computing. Often a simple analytical example is more illustrative and explains more than a numerical example.

Do not fall in love with the Kalman filter! While the Kalman filter is admirable as a computational device and of tremendous theoretical interest, it is not always the best framework for theoretical analysis. Sometimes a simpler approach, like batch estimation, can lead to more obvious insights, because the mathematics and physics are not obscured by the complexity of the filter equations. Simply proposing a measurement model, determining the measurement sensitivity matrix, and writing the ensuing filter equations is not necessarily research.

Be real! The best research often comes from real problems. I think that much academic research is, well, academic. If you are a young academician, form close ties with a local company or with a government facility. You may find that your best ideas, even general theoretical ideas, arise from their needs. These contacts may also become a good source of research funding, summer salaries, and jobs for your students.

A wise man can learn from anyone ... Listen carefully to the questions of people struggling to understand your work or that of others. Sometimes their difficulties stem from situations not considered in that work or contain the germ of an important new idea. This has been the case for me in both Physics and Engineering. Keep in mind that the famous cameraman Gregg Toland volunteered his services on *Citizen Kane* to the untried director Orson Welles, because, he said, beginners had always provided him with the best new ideas.

Don't always be practical! Sometimes impractical research can be very enlightening about the foundations of our field and even about the nature of real applications, but don't let it become your dominant research theme.

It is better to be right than "practical." Wrong work is not "practical," just because you can describe it simply without equations. Mathematical work is not "only of theoretical interest," just because it requires a lot of equations. Do not let yourself be influenced by the kind of person who favors simplicity over correctness.

Pay attention to small details! Sometimes small items of no obvious consequence can lead to important research. Be especially watchful of steps that you must gloss over in a research paper, which you think are intuitively obvious, but which you don't know how to show. They may be a sign of hidden gold for future (or current) research. Also, they may indicate that your earlier intuitive assumptions weren't quite right.

Develop intuition! We avoid mistakes by having good intuition. We develop good intuition by making mistakes.

Not all work is valuable. Just because a work is correct doesn't mean that it is valuable. It must be interesting as well. Regrettably, valueless research sometimes gets published.

Be a dilettante! It is worthwhile to approach some research as a dilettante, that is, to do the work on your own, on your own schedule and not be tied to contract or graduate-student timetables. Don't necessarily give your most original ideas to a Ph.D. student, unless you are certain that he is at least as smart and imaginative as you, because then the work will be done on the student's timetable and at the student's level of competence and creativity. To make the computer simulations a master's thesis after the theory has been worked out completely is another story. Research cannot all be part of the *business* of being a professor. Some of it must be a truly joyous personal activity not easily given to a subordinate. If we forget this, then we risk making research just a job.

Be unreasonable sometimes! Being unrelentingly reasonable or politically correct in life (and in research) makes Jack a dull boy. Sometimes ethics forces us to go out on a limb, and our quest for truth forces us to explore territory that others would rather we avoid. Polonius definitely had something to say here.

Knowledge is infinite; humans are finite. While it is good to study just to acquire knowledge, keep in mind that there is no limit to the amount of background one can acquire on a particular topic. Don't wait until you have complete knowledge of a topic before you begin research. We often learn what things we really need to learn *as* we do research. Sometimes it is even more efficient simply to "reinvent the wheel." Learning too much about a topic can make us unoriginal, because we will get stuck in the rut of previous work. That can happen even with our own previous work. (Trust me, I know.)

The most important research is often about finding questions, not about finding answers. As engineers or scientists not only must we find the answers to important questions, we must find the important questions too (see the previous section on the Arts). Computers and simulation can only be part of research. As Picasso once said, "Computers are useless. They can only give you answers."[52] That statement is not altogether true for us, but there is much truth in it nonetheless.

Simulation is a valuable tool. Simulation is valuable as a partial verification of your work, since simulation failure certainly indicates that something is wrong somewhere. It is valuable also for the illustration of your results and for determining the computational burden of an algorithm in real applications. In real-world applications in which analytical verification often is not easily attainable in available time, simulation may be the best we can do to gain some (if not complete) confidence in a method. Keep in mind, however, that simulation experience is data, not

[52]Los ordenatores son inútiles. Sólo pueden darte respuestas.

insight or intuition, which come from physical or mathematical understanding. However, it is often the pathway to insight and intuition.

Simulation is not a proof. I see too much shoddy work, sometimes even in the journals, "proved" by simulations. The worst sort of paper, in my opinion, is the kind in which the writer proposes arbitrarily several different mutually incompatible solutions to a problem and decides which one is best by simulation tests. Just because your Kalman filter residuals converge to zero or some small value doesn't mean that your work is correct. The correct approach may converge faster or to a smaller value, or the asymptotic error level may be very different from what it should be as a result of errors in the approximation. Simulations to "verify" theoretical results should not just show that the errors become very small but that they have the anticipated values.

Not all simulations are equal. Sometimes I see illustrative simulation which simply repeats the steps of the author's *ad hoc* prescription, and illustrates only that the author has programmed his simulations correctly, although the mathematics or physics or model he programmed may be wrong. Avoid this. Also do not perform simulations which simply show the inner consistency of your work while avoiding numerical comparisons of your work with a known correct or more complete theory.

Write as you go! I have discovered that writing up my work may be my most important research tool. Generally, it is when I write that I discover the things which I should have done that I didn't consider doing originally or just didn't know how to do (but thought I did).

Don't rush to publication! I find that my publications in progress generally improve with age, provided I continue to revise them. If you are an assistant professor seeking tenure, this tactic may not work for you. All the same, walk, don't run.

Do not build permanent monuments to bad work! Conferences are a good place to make known incomplete or not yet completely justifiable work; journals are not. (No place is a good place for work you know is incorrect.)

Don't defend your mistakes! If you have made a grievous error in a publication, especially in a journal article, don't try to cover up the mistake or, even worse, persist in it out of pride. A backlog of respect from previous good work may be squandered if you do, and you may be remembered more for your persistence in error than for more extensive good work. Better to publish an erratum or give notice of the error and correct it in a succeeding publication. I have done one or the other numerous times. No one will respect you less for having been honest.

Check your work! This doesn't mean only not finding errors when you reread your derivation or your computer program. Make certain that your new equations agree with trusted known results in special cases. In derivations, if you are able, derive your result in more than one way. In your computer output, check intermediate results. Check any properties your results should have. In simulation, test for simple models for which you know the answers or can easily calculate them by hand. Never just say: "I coded my equations, and this is what I got." In a batch estimation problem, do your estimate errors decrease as $1/\sqrt{N}$ as the number of measurements becomes larger? Are two-thirds of the estimation errors smaller than one sigma in magnitude? In a Kalman filter are your residuals statistically consistent with the computed residual covariance matrix? Always understand the errors, don't be satisfied just because they're small.

The world will remember only your archival publications. With few exceptions conference proceedings are eventually forgotten. Few people will go to the trouble of purchasing copies from the professional organization after the conference. If you wish your work to be remembered, publish it in a reputable journal. Note also that conferences *do not* review papers carefully. Don't betray your inexperience by boasting that your paper was presented at a "refereed" conference.

Good ideas often come quickly; good publications always require a lot of work. Some of my ideas come quickly, although I may spend a lot of time making the math come out right. (My first significant researches both in Physics and in Engineering were long, arduous, and frustrating.) Writing a journal article (and often a conference article) takes me forever. Good writing is actually part of the research. You don't understand a result until you can present it well.

Not quantity but quality! In Latin: *non multa sed multum,* or in Ancient Greek: *οὐ πολλὰ αλλὰ πολύ* (literally, in both languages, "not many but much"). The ancients already had it right. Don't publish trivial or repetitive work or publish your work piecemeal just to get more publications. To do so in order to be able to attend a conference is excusable, but even there, when I see a chain of papers with few new results in each, bloated by unenlightening simulations, I am not impressed. At the same time, putting too many topics in one long paper can make it opaque. In that case, it can often be made clearer by dividing it into two (or more) shorter publications. I have tended to err on the side of articles that covered too many topics.

Be pedagogical in your papers! My papers most often have a strong tutorial element (meant largely for me), and I have often been accused of writing a textbook in the journals. That accusation may be justified, but I also get a lot of citations. It is easy to overdo pedagogy, and hard to find the right amount. Work at it. When you write a paper, you are not only reporting what you did but also teaching your readers how to do what you did. The journey, one might say, is part of the result. This may be too much to ask of young researchers, but it is worth a try.

Good cooks leave good recipes. Also helpful when presenting a very new method is to give a detailed bulleted prescription in a later section or in an appendix where the steps are summarized one by one. Don't repeat long equations, but simply refer to them by number in the main text. Except in rare instances, publishing code in MATLAB® or C is probably not a good idea, and a typographical error may make enemies for you down the road. Descriptive code is safer, and reading your paper should not be an experience similar to puzzling out a computer program.

Always give credit where credit is due! If you use someone else's results in your paper, always cite them fully and unambiguously, making clear what parts of your paper are taken from theirs. It is always better to err on the side of being overscrupulous. Always check for earlier work before you publish a result. Given the bibliographic resources provided by the Internet, especially (in 2006) the AIAA, the IEEE, Google, and Google Scholar websites, there is no excuse for being unaware of any important related work published in the last two decades. When I see an article that has no references more recent than twenty years ago, I become suspicious that the work is old and obsolete or that a superior similar article by someone else has already appeared.

Pride goeth before a fall (≈ Proverbs [16:18]). Lack of pride goes nowhere. Worry not only about the value and correctness of your work. Be concerned also with its presentation, the writing, the drawings, the plots. Learn to write clearly. Every technical writer should read Strunk & White [24] once a year. Streamline and simplify the mathematics as much as possible. If you typeset your work, learn good typesetting style. *The Chicago Manual of Style* [25] is the common standard. Read extensively outside Engineering. The quality of expression from non-engineers is usually much better than ours. Remember, that when you write a paper you are telling a story. A good paper, like a good short story or a good film, has a clear beginning, middle, and end. Writing a scientific paper is not like writing the great American novel, nor is it like ordering a pizza.

Non illigitimi carborundum est! Do not let yourself be overly angered by unfavorable reviews of your submitted articles. Most reviewers are careful and thoughtful. Occasionally, there is the mean-spirited review. If a reviewer has misunderstood your paper, you should examine his review carefully in order to decide whether he was simply unequipped to review your paper (this happens) or your presentation wasn't clear enough (this happens more often). Innovative work is not always recognized right away. It is better to rewrite your article to make the nature or value of your innovation more apparent than to argue with the reviewer. He will probably not be the only reader to miss your point. Detailed reviews are worth gold, even if they are negative.

Good teaching in Engineering is *Research.* In the words of the philosopher Columbanus Sutor: *Docendo discimus,* by teaching we learn. Preparing a course in your area of research is very much a research-like activity, if you do it right, and a source of ideas. This advice is almost a corollary of the statement that good writing is part of research.

Expand your horizons, don't just change them! You may change your research area in order to collaborate with a more established colleague or because you have changed jobs. Try not to abandon your old research area completely. Keep it up, and look for problems in which you can apply your old skills to your new focus. You may open up a new research area that way. In the first year of my Engineering career, I presented the QUEST algorithm in a seminar titled "Applications of the Methods of Theoretical Nuclear Physics to Optimal Attitude Estimation." The attendance was standing-room-only!

Have courage! Do not be afraid to examine a topic, just because a respected colleague thinks such work is silly or that the problem has been settled. On the other hand, if you discover nothing important, move on. Most of my own early efforts in Astronautics did not lead to a publication.

We all screw up. This is true of the great and the small. Some of my most admirable colleagues and even the author have violated many of these items of advice, mostly in our youths. It is my least admirable colleagues who continue to violate most of them, even in their mature years. The sky will not fall if you decide that you have made a mistake, even several mistakes. Life and work require constant adjustment.

Life is not fair. No one said that research would be easy. Don't give up too easily on a problem, and don't work on an unyielding problem for too long without doing other things as well. Be prepared that you may not always be rewarded as you deserve, and sometimes colleagues resentful of your abilities and accomplishments may consciously try to do you damage or try to take credit for your work. Research is carried out by people and is subject, therefore, to the inequities of any human endeavor. To work hard and to do good work is often the best we can do. The most important praise we receive comes from within (and stays there). If we constantly produce work of high quality and have respect for ourselves, then eventually others will come to respect us. There is no other way.

Don't let the blues get you down! It is a truism that the most creative people are often the most susceptible to depression or even manic-depression. This is more frequent in the Arts than in Science, Engineering, and Mathematics, but it happens to us all the same. If you become depressed during a dry period, occupy yourself with other tasks, such as some less exciting work that has been on your shelf for some time, writing up unpublished work, studying a topic for which you had not previously found the time, writing a review of some research area (not necessarily your own and, perhaps, never to be published), software development, or preparing a new course. All activities which lead to a desirable result stimulate us and create the endorphins which will take us out of the blues. Freud always contended that work is the best therapy.

Research isn't everything. When we are engaged in research, especially when we are working on our dissertations, we think that research is everything, but it is not. There is joy in discovering a new result, but I think research (in Engineering especially) is most satisfying when it serves some immediate practical purpose as well. I have done a lot of Engineering research, mostly in industry, all of it very satisfying; but, if I look back at nearly thirty years in Engineering, my happiest were the first few, when research was not my objective nor part of my job description. Even for academics today, research is often only the icing on the cake, eaten in haste, and spoiled by deadlines, bureaucratic paperwork and proposal writing. Research has always been an important part of my life, but other aspects of my career have been just as rewarding if not more so.

When in doubt, do the right thing. We almost always know what the right thing is. Our moral dilemma generally is that we would rather, usually for selfish reasons, do something else. Morality requires courage and a willingness to give up something in order to do what is right. Loyalty, efficiency, and expediency are fine attributes, but they are not moral attributes. If we are to seek the truth, we must also be truthful ourselves in all things. Make the world a better place.

Take all advice with caution. All advice is based on the giver's personal experience and prejudices, the present advice no exception, and no advice can anticipate all situations. My advice includes practices that have worked for me and some others, and also warns against practices that I have found to lead to work which, in my opinion, is of diminished quality. Many of my close colleagues do not agree with every one of these items. Some things one must simply learn for oneself—the hard way. No writings can protect you from every disaster. If my counsels have made you think more about what you do, and especially if they have given you encouragement, then I am very pleased.

Above all, be happy in your work! Readers of my generation will recognize here the mantra of the Japanese commandant of the prisoner-of-war camp in the film *The Bridge on the River Kwai,* who hardly created a happy work environment. Research should be a source of joy, of exhilaration, and, in many ways, an act of love. If it isn't, then it may be difficult to endure the hardships that research entails. I abandoned a productive career in Nuclear Physics thirty years ago, largely because it stopped being fun. I never expected to do research in Astronautics—I was even looking forward to a break from research—but, it seems, research was unavoidable; it's my nature. May research bring you these same joys.

A Final Word

τὸν καλὸν ἀγῶνα ἠγώνισμαι.[53]
Timothy [4:7]

Read great literature (novels, short stories, plays, poems, and even children's literature); ponder great pictorial and plastic art; watch great cinema; listen attentively to great music; study foreign languages old and new and read their literature in the original as well as in translation; study Philosophy, History, religions (especially the ones that aren't your own), Psychology, Mathematics, Physics, and Biology; dine on exotic cuisines, travel; and engage in sports if you are able. Simply seek out every (legal) stimulation, mental and physical, that you can. Also, live a good life, be kind and caring toward your fellow man, teach our youth, learn to laugh more, do great work, and write good papers. Above all, *know who you are,* an exhortation dear to the ancient Greeks, *γνῶθι σαυτόν* (Know thyself!), which implies that you must first *think* about who you are.[54]

I thank all those who came to the symposium, or contributed to this special issue, or who have supported my activities through the years. Even more important than the sure knowledge that one's professional work has been appreciated is the assurance that one has friends. The presence of so many friends at the symposium and within the pages of this special issue has been more precious to me than rubies. The symposium made me very happy, however unworthy I may feel of all the fuss and attention. In the last words of the Roman emperor Augustus (63 B.C.E.–14 C.E.):

ACTA EST FABULA, PLAUDITE[55]

Acknowledgment

Harold Black, Jim Blucker, Russell Carpenter, Paul Cefola, Yang Cheng, Don Chu, John Crassidis, Paul Davenport, Natan Eismont, Bob Fischell, Norman Fitz-Coy, Jim Flynn, Luc Fraiture, Mike Griffin, Bob Groves, Bob Jacobs, John Junkins, Jeremy Kasdin, Jean Kechichian, Jim Kirkpatrick, Gene Lefferts, Peter Likins, Landis Markley, Ljudmila Meister, Daniele Mortari, Greg Natanson, Michael Ovchinnikov, Milt Phenneger, Mike Plett, Noel Polk, Dan Scheeres, Emil Schiesser, Wei Shyy, Andy Sinclair, Greg Staley, Joel Storch, Conrad Sturch, Sergei Tanygin, Barry Tossman, Grace Wahba, Roger Werking, and Jim Wertz all helped in the preparation of this article. None of these esteemed colleagues is the source of any opinion expressed in this work, and not all agree with them. Any errors or misstatements in this article are the sole fault of the author.

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[53] I have fought the good fight.
[54] This exhortation is especially appropriate for us, because it is attributed to the great Greek astronomer and philosopher Thales of Miletus (624–547 B.C.E.). It was chiseled over the gate to the oracle's compound at Delphi, where the greats of ancient times came for wisdom.
[55] The story is done, applaud.

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