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Review of the *Journal of Guidance, Control, and Dynamics*

THE AIAA Publications Committee periodically reviews the quality of each of the journals and each book series. Last year Professor Arthur Bryson chaired the committee that reviewed the *JGCD*. Other committee members were Itzhack Bar-Itzhack, Anthony Calise, Dallas Denery, Robert Stengel, Kevin Wise, and Paul Zarchan. The overall conclusion of the committee was that "The *JGCD* is being well run and is the leading journal in aerospace guidance and control." This is a gratifying result that is due to current and previous authors, reviewers, members of the editorial staff, and many other people who have contributed over the last 25 years. Thank you to all who have helped to bring the *JGCD* to its high standing in the community. Also, thanks to the committee members for volunteering for the task.

Several of the committees' specific suggestions, such as moving to a Web-based manuscript submission and tracking system, are being implemented for all the journals. The committee also suggested that more papers be published that deal with computer engineering issues that impact guidance and control, air traffic control technology, history of key technology papers, and papers that cover sensors, actuators, and research instrumentation in a systems context. Authors—we can only publish what you submit! This is your "Call for Papers."

Some of the committee members felt the *JGCD* was becoming too mathematical, but others strongly disagreed. The ongoing, 25-year-old debate about trying to publish good applications papers was also raised. These comments led to further discussions among the editorial staff. A perspective on the value of papers we publish is offered in the following guest editorial by Associate Editor Steve Osder and in the follow-up comments by Associate Editor Richard Colgren and former Associate Editor Gary Hartmann. Other Associate Editors also responded to Steve's editorial, but space limits the publication of their comments. Readers are welcome to share their thoughts with any member of the editorial staff listed inside the front cover. We want your feedback; this is your journal.

G. T. Schmidt
Editor-in-Chief

Can We Improve the Value of Papers We Publish?

Stephen S. Osder, Associate Editor

When this journal was founded in the late 1970s, Don Fraser, the original Editor-in-Chief, tried to emphasize application orientation in our papers in the hope that the journal would provide useful support to the design community. When he met with the Associate Editors, he would always solicit new ideas for increasing the application content of journal articles. Despite that effort and the desires of succeeding Editors-in-Chief, the number of articles that represented the real state-of-the-art of guidance and control applications continued to diminish. As an Associate Editor since the first issue of this journal, I have observed the continuing decline of truly useful state-of-the-art content in the papers we publish. One of the consequences of this situation is the dearth of good practical references to support the motivation for many of the papers that are submitted for publication. A paper's contribution is often described as a major breakthrough, based on the citation of prior Journal literature, when in reality, the problem being solved is either trivial or not even an issue for state-of-the-art designs. The difficulty lies in the absence of adequate literature citations that the author could have used to obtain a proper perspective of the real problems. Application papers that this journal continues to solicit could have contributed to such a perspective, especially if we had succeeded in achieving Don Fraser's application orientation goals. In this editorial, I sound off about some specific examples of "non-problems" that are the subject of many of the papers that I continue to encounter as a journal Associate Editor.

These comments have been circulated to the other Associate Editors, and their responses have been both interesting and encouraging. Two of these responses accompany this editorial. The Colgren comments explain reasons why we encounter difficulty in obtaining truly good application papers. The Hartmann comments note that there are some excellent publications that give a fine perspective of the true state-of-the-art, including papers published in this journal. Indeed, I participated with Carl Droste in his presentation that covered real-world experiences in the F-16 Fly-by-Wire system. I covered the history of redundancy management applications, noting the discrepancy between the theoretical approaches covered in the recent literature and the real problems that designers must confront. About three years ago, this journal published a paper in which I extracted some of my material on this subject. I tried to emphasize that fault detection theory is important and useful, but it must be applied with knowledge that the measurements used to identify the plant model could also fail unless sufficient hardware redundancy is used. I continue to receive papers that completely ignore this possibility. My criticism is not with the theoretical papers on subjects mentioned in the following comments, but with their perspective of the real-world constraints that dominate the utility of this theory. I am certain that much of this outstanding theoretical work would actually be enhanced if it were presented with a better understanding of the contemporary state-of-the art in the related technologies.

One example of the pursuit of elegant solutions for which no real practical purpose can be found is in the field of missile guidance. Specifically, "proportional navigation" defines a general class of pursuit strategies that could provide the basis for target intercepts, when properly adapted to accommodate realistic initial conditions, maneuvering limits, sensor dynamics, and unique seeker properties. However, proportional navigation has always had a fascination for the theoretical community because, with appropriate simplifications and coordinate frame creativity, the resultant missile trajectory and miss distance parameters can be solved without recourse to numerical simulation techniques. Although this theory could provide good, top-level insights into the kinematics of the pursuit problem, authors tended to deluge the journal with paper submissions where the new contributions involved variations in coordinate frame formulations that would give no new insights into the problem. These variations could address different initial conditions that may have caused difficulty with other coordinate frame approaches, but they never did recognize how real-world designs had already solved the particular problems being considered. One author that I recall rejected a particular approach as representing an inadequate design because it did not permit a "closed form solution." In an era when simulation techniques permit very detailed representation of the entire problem, including the usual nonlinear properties and the higher order dynamics, designers of real systems are always aware that closed form solutions giving the appropriate design are never available, and they certainly are not needed.

My specialty for many years has been in flight control design. About 15 years ago, when working for a helicopter company, I received paper submittals on "robust" flight control designs for a specific helicopter with which I was very familiar. The designs were based on linear perturbation models at a particular flight condition. I therefore took the designs found in those papers and tested them against the most complete, nonlinear simulation of that aircraft. Those designs fell out of the sky when subjected to the simplest maneuvers or coefficient uncertainties. This motivated me to write a paper that tried to illustrate how a contemporary helicopter fly-by-wire system is designed and tested for robustness. In that paper, I demonstrated why two of those published papers did not work and noted several areas that were neglected, including the absence of consideration for the trim conditions that permit linear perturbation

models to be extracted from a trim state. In the interim years, I have continued to handle helicopter flight control papers for this journal and have observed that the most cited papers in this field are the ones that described the designs that "fell out of the sky."

I mentioned the neglect of the trim conditions and implied that the linearization process between trim states might involve difficulties. Actually, if the design is done properly, these difficulties are alleviated, but many of the authors of flight control papers have begun to appreciate that the trim state could be an issue. I recently received a paper where the authors showed how their flight control system solved for the trim condition. The approach involved variants of the inverse solution of the aircraft equations. That is, given the requirement that attitude rates and linear accelerations are essentially zero during trim flight at a given speed, the authors solve for the longitudinal cyclic, lateral cyclic, collective, and tail rotor required to meet that condition. This is an interesting problem, and there may be reasons why one would like to compute this solution, but it is not relevant to a flight control system design. If the authors could have found a reference to how contemporary fly-by-wire systems for helicopters are mechanized, they would see that the control laws always inherently generate the trim solution. In effect, any fly-by-wire system that I have designed will inherently find the control position solution, in one or two seconds, regardless of where the control positions are placed initially.

Another area where the literature base has not given authors a proper perspective of the real state-of-the-art involves aircraft actuators and the consequences of their failures. A typical Introduction section will proclaim that existing design methods have not solved this problem adequately, and that the breakthroughs to be described in the following paper will contribute immensely to potential flight safety. Hydraulic actuators have been part of the primary flight controls of civil and military aircraft since the 1950s and earlier. Do the authors of these papers realize that every time they fly on a commercial aircraft, the redundant actuator systems, with their inherent fault detection, isolation and reconfiguration already provide the safety that their research intends to offer. These authors have obtained their understanding of the problem from similar research that we have published, but the dearth of real-world application papers contributes to this faulty perspective.

Is there a solution to this problem? Perhaps the researchers do not consider it to be a problem, but in my experience most researchers in academia would like nothing better than to orient their work to solving real-world problems. Unfortunately, the literature base is not an adequate guide for improving the relevance of flight controls research, for example. A simple but difficult solution would be to require that authors review the actual state-of-the-art before proclaiming their breakthroughs. If the existing journal literature does not provide an adequate picture, they need to obtain personal contacts with designers or organizations that could give them a clear description of how contemporary flight control systems perform the tasks of aircraft stabilization and control. Meanwhile, this journal needs to continue the pursuit of good application papers and, in the process, become more creative in motivating the real designers to write such papers.

Comment by Richard Colgren, Associate Editor

I have been in the industry for 20 years and agree with most of Steve Osder's points. However, I must also point out that we are in a Catch-22 situation. Academic authors need to write papers. Engineers in industry are not encouraged to write papers; actually most often they are directly or indirectly discouraged from doing so. Few, if any, rewards are given in industry for journal publications. The author must get company and/or government permission to publish after various reviews of the written materials. Often no time is provided to write the papers in the first place.

Academic authors need good models to work on. Industry engineers have these models and usually cannot release them, because of government and company access restrictions. Often the problems of industrial interest (even what problems have been solved using proprietary methods) are again controlled by the company and/or the government and not released. Liability concerns also generate additional controls and further restrict the release of information. This problem of access and control is getting worse, not better. The gap between academic research and industrial application appears to grow every year, in large part because of this tight control of information by the government and by industry (often by people who do not understand the engineering problem involved).

I would like to see more applied work done at universities. Wind tunnels and other devices for generating the required data are expensive to own and operate. Constructing these models takes significant time to accomplish. Also, government and industry are not adequately guiding or supporting university research, decreasing the ability of universities to develop these models. The demand to publish so many papers per year causes university authors to use the discussed "textbook" models and "example" applications that too often diverge from reality. This is very much a result of the tenure system emphasizing the number of papers published. Some universities are addressing this to a degree. (The Naval Postgraduate School is a good example of where less of the emphasis is on the number of papers and more is on the use of detailed models and testing.) Meanwhile, government and industry engineers are hindered by the previously discussed restrictions on publication, drying up the other source of papers and directions for research.

As long as industry thinks they can train new graduates "on the job" and the restrictive behavior continues, the problem will get worse. With retirement (and simple aerospace-wide downsizing), the industry has lost many of those doing "on-the-job" training. Those remaining have less time available to mentor. I would argue that several of the well-publicized aerospace failures of the last few years result from downsizing and the loss of experience. We will continue to pay for this in real dollars either over the short term or over the long term. Academic researchers need to spend more time in the laboratory developing models for the literature. This will decrease the dependence on the sometimes overused and overly simplified models discussed in the editorial. Government and industry need to do more to support (through money and people hours) this type of real and releasable research at universities. Government and industry finally need to implement protections only on data of a true national security and competitive nature.

Comment by Gary Hartmann, former Associate Editor

I would agree with some of Steve's comments on "real-world problems" but I also see a few items not discussed that are relevant (in my opinion).

For many "applications papers" it is hard to see what would justify publishing them in a journal such as *JGCD*. Look at flight control or aided navigation—these areas are pretty mature; there are very few new equations or new approaches to discuss. Textbooks have appeared that cover this material. What are the people in industry working on in these areas? In many cases they are working on designing systems that are lower cost or easier to certify, etc.—nothing that we would see as really "novel."

Here's where I disagree with Steve. I would claim that there are lots of "real-world" descriptions of problems (and existing solutions) available to the student or young engineer if he or she were motivated to look. Many of these are not in *JGCD*, but they are in the open literature. Consider the AIAA Guidance, Navigation, and Control conference, DASC, PLANS, etc. These conferences attract many papers describing current programs with a real-world perspective. There are now textbooks covering many of the topics Steve mentioned that were not available in the 70s and 80s. For example, Carl Droste did an AIAA-sponsored case study on the F-16 FBW; it covers the practical matters that Steve mentioned, and in far more detail than a journal paper could! Similar case studies are out on the Boeing 777. Look at Paul Savage's recent books on navigation; there is much information on real-world problems and solutions in this specialized area.

The question that might be asked is "Can you, or should you try to re-position the *JGCD* to get more of the papers Steve would appreciate?" I'm not so sure—it might move the journal too close to a trade magazine.