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AUTHOR INFORMATION

Nourdine Aliane (nourdine.aliane@uem.es) obtained the electrical engineering degree in 1990 from the École Nationale Polytechnique d'Alger and the Ph.D. in physics in 1995 from the Universidad Complutense de Madrid. He is currently a professor of control engineering and robotics at the Universidad Europea de Madrid, Spain. His interests include control systems, robotics, and education.

Advice to Young Researchers

MALCOLM D. SHUSTER

A wise man can learn from anyone,
even from a fool, but a fool can
learn from no one, not even
from a wise man.
—Jewish proverb

It is easier to give advice than to
bear one's own problems.
—Euripides (480–406 BCE)

A new researcher in any discipline is often at sea without a compass. He may have demonstrated tremendous skill already as a graduate student in the ability to achieve new results in research, but is unaware of the ethical requirements of research or of many activities that can improve the quality of his work and protect that work to a degree from serious errors. I am sorry to say that too many supposedly seasoned researchers are unaware of these errors or choose to ignore them. For the engineer there is the additional requirement that his research should have some connection to the real world, particularly on how one finds research problems in it. The present essay is not intended as a survivor's manual. It has loftier goals for those young creative researchers who, to steal a phrase from Faulkner [1], will not only survive but prevail, whether in industry, government, or academia. The present article is adapted from part of an earlier article [2].

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A BASIC RULE

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. But genius is not enough. 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.

RESEARCH

Be focused! 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 proven 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 may take a long time to develop.

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 (*mea culpa*), 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.

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. (My most important research has often been research and development.) These contacts may also become a good source of research funding, summer salaries, and jobs for your students.

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

A wise man can learn from anyone. Listen carefully to others, especially, to the questions of people struggling to understand your work. Sometimes their difficulties stem from situations not considered in your 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.

It is better to be right than "practical." Wrong work is not "practical," just because you can describe it simply without equations. Rigorous 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 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 really useful or increase our understanding or pose a new problem as well. In a word, it must be truly worthwhile. Regrettably, valueless research sometimes (often?) 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 the student is at least as smart and as 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 thing. 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. Shakespeare's Polonius in *Hamlet* 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 to develop your own ideas. Usually, the idea comes first. 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, even our own.

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. Computers and simulation can only be part of research. As Picasso once said, "Computers are useless. They can only give you answers." (Los ordenatores son inútiles. Sólo pueden darte respuestas.) That statement is not altogether true for us, but there is much truth in it nonetheless.

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, for example, do your estimate errors decrease inversely as the square root of N, as N, the number of measurements, becomes large? Are two-thirds of the estimation errors smaller than the theoretical one sigma in magnitude?

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.

Carpe diem! Seize the day, said the Roman poet Horace (Quintus Horatius Flaccus, 65–8 BCE). Do not procrastinate in your work. Above all, do not procrastinate when it comes to working independently. When one arrives at a faculty as a new assistant professor or even post-doctoral fellow, it is natural to want to be helpful to your new colleagues, or to become a secondary contributor to their work, which you may rightly regard as better than anything that you can do. Your rôle, however, is not to be effectively another graduate student for senior faculty, even if you have much to learn from them. Participate in their work, make yourself useful. It will not hurt your chances for tenure that some highly respected member of the faculty acknowledges a debt to you. But you must also give your department a real reason to give you tenure. You must take the initiative in doing independent research. Don't procrastinate. You can find a million excuses to put this off. Remember the Nike commercial and *just do it!* If you are working in industry, and your boss asks you to do something, but you think you have a better idea, do first what he asks—you don't want to get a reputation of being uncooperative, which could get you fired or at least a poor raise—and then work out your own approach as well, but only if it can be done quickly. And don't show off afterward.

Keep it simple. Not all research is equal, and not all of it is valuable. The best research discovers simple things, which are often the hardest to find. It is easy simply to extend an existing piece of work to treat a small variation of a model or of a set of assumptions, or which consists mostly of simulations. Such work can be valuable, but the simple things are often the most important. Above all, avoid making a career out of publishing endless variations of your dissertation or some other research work. Your dissertation may contain unmined gold, but recognize when the vein has run out. Don't expend enormous effort just to develop a new methodology which is five percent better than an existing one. No one will care except you.

Be useful. Not every piece of work we do can be a breakthrough, but every piece of work should be useful. If not of immediate practical use, it should improve our understanding or provide an intermediate step to something useful and not simply demonstrate the brilliance of the researcher. Sometimes, we discover something new and end up changing a research area. But most research is not pathbreaking. Sometimes, a piece of research simply carries out a more basic and thorough analysis of previous work, not necessarily your own. Sometimes, one publishes afterthoughts about special cases. Sometimes, one publishes a survey or a tutorial. My most cited article isn't research at all but a survey paper, which presented a great mass of material (two hundred years worth, in fact) from a unified point of view with consistent notation and conventions. When I submitted it, I told my friends that I had just submitted my most unoriginal paper, and which, I said, would become my most cited paper. I was right. The most important quality of good research is that it helps others rather than just satisfying one's own intellectual gratification.

Research ideas sometimes come from the strangest places. I have found that long-distance driving, walks in the mall, music, novels, poetry, and cooking sometimes stimulate research ideas. The list is endless. Read my earlier article on "The Arts and Engineering" in *IEEE Control Systems Magazine* [3].

SIMULATION IN RESEARCH

Simulation is a valuable tool. Simulation is valuable as a partial verification of your work, since simulation failure indicates that something is wrong somewhere. It is valuable also for illustrating 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 insight or intuition, which come from physical or mathematical understanding. However, it is often the pathway to insight and intuition.

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

Simulation is not a proof. I see too much shoddy work, sometimes even in the journals, "proved" by simulations. The worst sort of article, in my opinion, is the kind in which the writer proposes arbitrarily several different solutions to a problem, none of which is obviously correct mathematically, and decides which one is best by simulation tests. Just because your residual errors 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 the simulations correctly, although the model that was 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.

PUBLISHING

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.

Use clear and systematic notation! Do not introduce new notation just to be different. Using 0 for an index of an array 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 the symbol for every column or row matrix. This occurs less frequently in the IEEE Controls arena than in other disciplines, such as Aerospace Engineering, where I work mostly.

Do not build permanent monuments to bad work! Conferences are a good place to present 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.

The world will remember only your archival publications. With rare 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. (Nonetheless, my first significant researches in any new area 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 πολὺ* (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 overly scrupulous. Always check for earlier work before you publish a result. Given the bibliographic resources provided by the Internet, especially (in 2008) the AIAA, IEEE, Google, and Google Scholar Web sites, 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 20 years ago, I become suspicious that the research is old and obsolete or that a superior similar article by someone else has already appeared.

Pride goeth before a fall. (approx. 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 [4] 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* [5] 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 the 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. The reviewer will probably not be the only reader to miss your point. Detailed reviews are worth gold, even if they are negative.

TEACHING

Good teaching in Engineering is Research. In the words of the philosopher Columbanus Sutor: *Docendo discimus*, by teaching we learn, and learning is certainly part of research. 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. If you acquire a reputation as a great teacher, you will also make valuable friends among both students and faculty. One of my most valuable journal articles came from trying to motivate an algorithm for course presentation.

THE DARK SIDE OF RESEARCH

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 youth. 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), developing software, 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.

LAST WORDS

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 more than 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, which were mostly development or management, 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

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2008

UKACC CONTROL CONFERENCE (CONTROL 2008)
2–4 September, Manchester, U.K.
General Chair: Hong Wang
Program Chairs: John O. Gray, Guoping Liu
http://www.control2008.org/index.php

▲ **MULTICONFERENCE ON SYSTEMS AND CONTROL**
3–5 September, San Antonio, Texas, USA
General Chair: Oscar R. Gonzales
Program Chairs: Gary Balas (CCA), Marco Lovera (CACSD), Kevin L. Moore (ISIC)
http://conferenze.dei.polimi.it/msc08/

▲ **CONFERENCE ON CONTROL APPLICATIONS**
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▲ **INTERNATIONAL SYMPOSIUM ON INTELLIGENT CONTROL**
3–5 September, San Antonio, Texas, USA
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● **INTERNATIONAL CONFERENCE ON CONTROL, AUTOMATION, AND SYSTEMS 2008 (ICCAS 2008)**
10–14 October, Seoul, Korea
General Chair: Sung Kwun Kim
Program Chair: Doo Yong Lee
http://2008.iccas.org/

2008 DYNAMIC SYSTEMS AND CONTROL CONFERENCE (DSCC 2008)
20–22 October, Ann Arbor, Michigan, USA
General Chair: Galip Ulsoy
Program Chair: Eduardo Misawa
http://www.dsc-conference.org/

▲ **47TH IEEE CONFERENCE ON DECISION AND CONTROL**
9–11 December, Cancun, Mexico
General Chair: Chaouki Abdallah
Program Chair: Thomas Parisini
http://control.disp.uniroma2.it/CDC08/

● **10TH INTERNATIONAL CONFERENCE ON CONTROL, AUTOMATION, ROBOTICS AND VISION (ICARV 2008)**
17–20 December, Hanoi, Vietnam
General Chair: Y.C. Soh
Program Chair: C. Wen
http://www.icarcv.org/2008/

2009

▲ **2009 AMERICAN CONTROL CONFERENCE**
10–12 June, St. Louis, Missouri, USA
General Chair: K. Hoo

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FOCUS ON EDUCATION *(continued from page 117)*

research—but, it seems, research was unavoidable; it's my nature. May research bring you these same joys.

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AUTHOR INFORMATION

Malcolm Shuster (mdshuster@ comcast.net.) is director of Research for Acme Space Company. After a decade of research in theoretical Nuclear Physics, he wanted to do something different and spent the next 30 years mostly in the aerospace industry, where he quickly found himself doing research again. More extensive author information can be found in a previous article in the *IEEE Control Systems Magazine* (August 2008). He can be contacted at Acme Space Company, 13017 Wisteria Drive, Box 328, Germantown, MD 20874; http//home.comcast.net/~mdshuster.