

Allan Steinhardt

Making New Stuff Work

“The empirical method has not been tried and found wanting. Rather, most ignore it and do not want it found.” *Anonymous*

I came to engineering late. I was an undergraduate mathematics major who became enamored with system theory and hence leaped from mathematics to engineering. I became an engineer because I was intrigued by the use of advanced mathematics to solve real problems. I soon learned that while there is much advanced mathematics in theoretical engineering research, this research doesn't always address “real world” problems credibly. This “theory to reality” disconnect is the subject of this article.

The real world disconnect begins in school, but it doesn't end there. We will in the discussions below explore the pitfalls in academia, industry, and government encountered in bringing basic research to a viable end product. I will also describe ways to overcome these pitfalls.

The glory of engineering is making stuff work. The glory of engineering research is making new stuff work. This makes engineering research fundamentally different from physics or mathematics. Medical schools revel comfortably in the glory of being a physician (vice biologist) and law schools revel in the dignity of law (vice philosophy). However, engineering educators sometimes thwart this professional pride by attempting to treat engineering research as a muted form of physics or mathematics. Why not educate engineers to be

proud of their unique dignity, which is taking material from the mines of applied science (notably mathematics and physics) and refining them into items of practical beauty? I do not intend here to take on, at least directly, the large topic of institutional reform but will speak rather at a more personal “grass roots” level. Specifically I will address how one can invest in a research career that furnishes salient solutions to important human problems and how to do this *despite* the plethora of institutional minefields that lie in wait in colleges, companies, and the public sector. (My own informal survey suggests there is rather widespread belief that well above an order of magnitude improvement in research productivity is achievable without extant obstacles. I am not sure I would enjoy a world that changed that fast!)

Research by its nature involves false starts and dead ends. That is unavoidable. I focus here on obstacles that greatly compound and amplify the inherent inefficiencies in research and mitigate against those who seek to provide solutions that directly and tangibly help humankind. I offer my observations as a passionate student of human enterprises, drawn from my experiences as an academic, a government employee, and industrial sponsor/advisor over more than two decades, all in the signal processing area. I will draw examples of the-

ory-to-reality disconnects from my own work, thereby hopefully offending only myself. If what I describe matches your own experience and you find this discourse fruitful, entertaining, or both, great! If not, stay tuned for a better read in these spaces.

Reflections on Academia

Academia is chartered to think up “new” things, with “making” and “working” being secondary. As such, emphasis is lavished on a vetting process to ensure novelty, via peer review. This has become an apparatus that drives all aspects of academic research, “vetted novelty” eclipsing all other factors. This is not without its downside, because it causes one to ignore the user community, and it tends to deemphasize attention to empirical data analysis. Examples of each are revealed in the following personal anecdotes.

When I began graduate school, the topic of “adaptive robust control” was just becoming fashionable. As I was informed, academics perceived that systems engineers were not adopting theories and algorithms that the academics published in their research literature. Worse yet, academics did not seem to seek out why this was happening. Instead, the response to the transition crisis was to create new mathematical abstracts essentially in a vacuum. Energy that might have been spent understanding practitioners' dis-

satisfaction with extant theories was spent creating new ones: notably eschewing the Euclidian norm for Banach space reformulation, L_p spaces, and the like. This was a shock to me, as I would have expected, being a newcomer to engineering, that control theorists and signal processors would want to *talk* to systems engineers and *learn* about their problems. But this was not to be! I was soon caught up in this exercise and found it great fun, even generating a Ph.D. out of it! Looking back more than two decades later, I strive mightily to see how any of our theoretical function space reformulation efforts helped “make new stuff work” in any profound, auditable manner.

Consider now the effects of discounting empirical verification. While I was in graduate school, “super resolution” became very popular. Many hundreds of papers were published, with very little attention to real data. Now the theoretical claims of super resolution have failed to materialize in many practical applications. One source of this shortfall, but not the only one, is channel mismatch, which is the decoherence across channels due to the convolution of the sensor’s Green’s function with the transducer input signal. This and other profoundly deleterious effects on the feasibility of realizing super resolution have forced much theory to be recast in light of the corrosive effects of real-world modeling mismatch. (Some readers may be among the mavericks who sought out data or focused on mismatch early on. You know who you are! My comments regard trends and institutional structures, not individuals.)

What were we theorists thinking? Would substantive dialogue with practical control engineers have stifled progress in robust control? Would engaging with real data have slowed down super-resolution advances? I think not. Instead I submit the insular nature of academia hinders progress.

This is not to say that academia doesn’t produce outstanding results. It just means that it does so *despite* overspecialization and insulation, not because of it. In this regard, engineering is vastly different from quantum physics. One can spend one’s time fruitfully on being an expert solely on, say, vector Bosons. That is because the glory of fundamental physics is narrowly focused insight on new knowledge. However, if one’s goal is to dedicate one’s life to advancing engineering, a discipline whose goal is to make things work, a narrow focus might produce little that is practical and beneficial, as I now hope to establish. (Some readers may object that it’s someone else’s job to “find the right problem formulation” and the researcher’s to solve it. But that’s the whole point of this article—there are no “they,” no one is tasked with problem formulation beyond the research community. Government and industry are customers, not generators of problem statements at the abstract mathematics level.)

I call the urge to become increasingly specialized “speciation” from its biological origins. (In biology, speciation is when a species, through mutation, creates offspring that no longer reproduce or socialize with their parent species. The analogy with scientific and engineering communities is, I trust, obvious.) Speciation is best observed in the urge to create ever more specialized journals and ever more myopically focused conferences. I submit that speciation actually hinders progress if unchecked. To this end I unveil for the first time in written public form “Steinhardt’s asymptotic law of speciation”:

$$\lim P(\text{salience}) = 0$$

$$\text{Speciation} \rightarrow \infty$$

This law states that, as disciplines in engineering become more and more specialized and insular, the probabil-

ity that a salient solution will arrive from research activities becomes vanishingly small. (Salience here means that the essence of a practical need has been captured in the mathematical formula of a problem to such an extent that a practical solution is offered once the theoretical problem is solved.) Eventually it reaches a point where it proceeds in ever-diminishing concentric circles until at last it descends into a inner-focused obfuscation from which no viable solutions ever again emerge. A corollary to this asymptotic law is Steinhardt’s speciation bifurcation point, the point of speciation beyond which the rate of progress is reduced, not advanced by, further subspecialization. I will leave it to the architects of journals, conference, and government agencies to ponder whether or not we have exceeded the bifurcation point.

Let me comment on the downside of excess speciation, again from a personal perspective. I have spent much time on eigen-space decompositions in space-time adaptive processing. One can see this as a fairly advanced form of speciation as seen from the following genealogy:

Linear System Theory:

Adaptive systems

Least squares estimation

Adaptive beamforming

Sample matrix inversion

Space time processing

Beam space techniques

Eigen methods

Now suppose we “eigen-engineers” decide to create our own journal and workshop (we are dangerously close to this by the way!). This would insulate us from considering alternatives or understanding how our eigen schemes differ from alternatives. I know of no fielded system that performs adaptive beam forming using eigen schemes, nor do I know of embedded applications where such an approach would

yield superior solutions to practical problems. (Other beam space schemes always seem to win if they use enough prior physical insight.) By defining a community as “eigen advocates,” one cannot trade such schemes against other approaches without threatening the identity of the speciated group, and perhaps even threatening research funding (perhaps even inviting career erosion). It is hard to argue such a societal construct optimizes unbiased, intellectual inquiry. (Speciation, despite its foibles, is outstanding for formal vetting of novelty! Hence a balance must be achieved. My point is a balance is needed, and at present there seems to be no end in sight for how deeply we approach the asymptotic limit.)

So what can be done? This will be largely saved for the end, where I address the issue of making new things work across institutional lines. Suffice to say for now that any graduate student or junior professor can greatly enhance the odds his/her lifetime work has lasting relevance by talking to users and being ruthlessly objective in assessing the merit of competitive technologies. This takes integrity and courage, not merely intellect! But the payoff is worth it.

A final important note. Much salutory, valuable work in academia lies in the area of performance analysis and theoretical underpinnings. Such work can have enormous salience, provided the assumptions underlying the analysis reflect real-world realities. This is because complex systems almost always are poorly understood, and so, insight can be a substantial value added in reducing risk, cost, development time, or all three.

Reflections on Industry

Unlike academia, industry is focused on “making” things (profit comes from selling stuff you make, after all), novelty and functionality being sec-

ondary. Industry, it can be argued, has the most to lose, as well as gain, by new technology. This creates great irony; survival in industry hinges on deciding if proposed technical novelty is friend or foe. This decision rarely evolves dispassionately, and even rarer by technologists themselves. An outstanding example is the satellite versus terrestrial cellular telecommunications trade. Many billions of dollars of space-based infrastructure has been written off in bankruptcy proceedings because terrestrial wireless technology, driven to no small extent by creative signal processing algorithms, eclipsed space-based solutions. Cellular telecom technology has created vast wealth while crippling and destroying jobs and entire mid-range companies. Clearly, in such a high-stakes business, what is best technically may be far less intriguing than mere corporate survival, and, in fact, fears surrounding the latter instinct can, and have, eclipsed sound judgment surrounding the former.

If we should find an institution anywhere focused on optimal solutions, empirically verified, and ruthlessly critiqued it would be industry. My experience, however, is that reality is much more complex. The same forces that gravitate towards speciation in academia are manifest in industry. Fierce loyalty is not found here along intellectual subspecialties, but rather it blossoms in research groups, divisions, or other communal lines. Such loyalty leads to parochialism, which in turn tends to govern the work breakdown in labor and the choice of technical venue at all levels. For instance, it would be very rare indeed for an infrared division to argue that a client's needs are best met with a radar solution. Seldom does industry have the time, or the inclination, to trade technologies across cultural boundaries; consequently, “making stuff that works,” is often the first casualty of trading away trades, so to speak.

Again, in industry, one is very keen on replicating behaviors of apparently successful competitors. This can fog judgment, especially if it is done by corporate leaders. The march of the lemmings, with the pursuing of Enron and WorldCom into the .com sea, is a particularly graphic illustration. Here the conviction was that a new business model, which somehow enabled one to get rich through creative financing, had to be embraced even while it was little understood. (This is an accounting analogy, not a technical one, though the technical duals are hopefully obvious.) The list of technologies that have been pursued beyond what prudence would have allowed is long and distinguished. Engineers are human and struggle for courage just like anyone else. It is just as difficult in industry, as anywhere else, to ask probing questions, questions that address the future of entire industries, fortunes, careers, friendships, and business partnerships. Making new stuff work is a mental challenge. It is also dangerous and destabilizing for corporations and institutions, and it is hopelessly naive to imagine that novelty will be warmly embraced, even by potential benefactors.

Reflections on Government

OK. I admit. Government can be very funny. For an excellent read I suggest *Below the Beltway* by Dave Barry. I will avoid a general discussion of governmental foibles and focus on research funding and oversight.

If academia focuses on “new” and industry on “making things,” then government is focused on making stuff “work.” Think about it. Government has deep pockets and enjoys a great deal of visibility, or notoriety, in a free society. It is far more likely that government will overspend, or delay, than that it will field a device too early, if at all. So what obstacles are produced?

Government folks involved in overseeing research and acquisition generally differ from their narrow specialist brethren in academia. Further they eschew the parochial loyalties that thrive in industry, since they tend to travel widely and rotate from position to position, at least in the defense arena. Nevertheless government throws up its own roadblocks to progress. In some sense, government creates and sustains impedimenta begotten in industry and academia. The governmental quirk lies not in speciation (universities) or self-sustainment (corporations) but rather in the unfortunate proclivity to map the world into technical readiness levels. Vast armies of bureaucrats (yours truly is among their ranks) are arrayed to assess and fund technology at all levels of maturity. Most readers are probably aware of the “six point” scale, 6.1, 6.2, etc. (6.1 denotes basic research, 6.2 advanced development, 6.3 engineering prototype, etc.) This tends to create the impression that basic research, 6.1, which funds most of the work published by the IEEE Signal Processing Society, can be assessed in isolation or with a symbolic gesture toward salience. (Such as asking proposal writers for a small paragraph on how their work will change the world. These paragraphs are often discounted. Rarely is any empirical vetting, vice mere peer review, mandated.)

Surely unproven speculative technology cannot be forced too early to “prove its way.” However by creating “technical readiness” stove pipes, the government isolates various communities in an artificial and counterproductive fashion. This insulates researchers from the needs and constraints of practitioners and conceals emerging technical opportunities from potential users. Members of the signal processing community can do a great service by engaging their public sector sponsors to break down transition barriers and build relationships between members of technical com-

munities that span the technical maturation spectrum.

As an anecdotal example of how this breakdown mitigates against salience, consider funding on automatic object recognition. Vast funds have been devoted to exploring how one can improve automated recognition. However it would be nearly unheard of for government to ask the signal processing community to engage in a serious trade analysis, using advanced performance bounds, to assess how, and if, fundamental device research could provide value added for automated recognition, or what device requirements are needed to obtain a particular level of classification error. What is a better investment strategy, more resolution? More signal to noise ratio? Multiple bands? Serious discussion of such things is rare; instead the sensor community speciates, and the algorithm community speciates, and the users are left adrift as what to buy, when to buy it, how well it might work, and what the best investment plan might be.

Conclusions

So we have seen the rough breakdown, making (industry), new (academia), things work (government). No rational observer could argue that this triad is configured in anything roughly approaching a globally optimal solution to provide solutions to human needs!

Let us again contrast engineering research to its pure science counterpart. As an example of the latter a Boson researcher does not necessarily need to know much about other related topics like gravitons. Further the “Bosonist,” if successful, may receive a Nobel Prize. Unlike his engineering brethren, however, he is not likely to develop, in any immediate sense, a disruptive technology that will impact the very survival of his own career or institution. In contrast research engineers, by definition, are

engaged in transformational concepts, which by their very nature threaten the institutions that birthed them. As an example, examine the impact of optical fibers and wireless technology on telecommunications. These technologies paved the way for low entry competition for local carriers and rendered obsolete massive investments in low bandwidth copper infrastructure. Such research can never remain insular since its goal is making things work, not “mere” insight. We all know the joke about the engineer soon to be beheaded who proudly reveals an engineering flaw in the guillotine design. Like any effective parody it reveals a truism: institutions seek preservation of self and status quo and treat ambivalently those whose task it is to make new (read unsettling, even if salutary and beneficial) things work. Engineers tend to be blissfully unaware of this fact, and to some degree prefer crisp technical issues to messy sociological ones (even to their own peril).

As a modest aid I offer suggestions to those who seek to promulgate salient ideas in the service of humankind. (Advancing your ideas is not to be confused with advancing your career! For the latter please consult your neighborhood career counselor.) I would welcome e-mail from any readers proffering further insight or criticism (asteinhardt@darpa.mil).

▲ *Be willing to talk to other specialists.* This can take numerous routes. Perhaps take a sabbatical doing something completely different. Perhaps a term at DARPA? Or NSF? Perhaps a year in industry? Or perhaps attend a workshop or conference in a totally foreign field. Less invasive, but lots of fun, is to spend a day doing nearly random searches on Google™ in areas completely apart from signal processing but which might use signal processing. Finally, at the next SP conference be willing to meet new faces. People often cluster in their own comfortable niche of old friends

at conferences. This is a symptom of “pre-speciation,” and while it’s great to catch up with old friends, making new ones is a good idea, too.

▲ *Be willing to be wrong.* Boy is this a tough one! Imagine if the satellite folks would have been willing earlier to assess that terrestrial repeaters might not have been so bad after all! Suppose those of us in signal processing could admit (and I even hesitate to say this publicly) that deterministic side lobe control and fixed filter design and beam formation can sometimes compete with, or dominate, purely adaptive approaches. Imagine a ruthless assessment of the value added of super-resolution verses bandwidth augmentation research. Who is better poised intellectually to do so than the readers of these pages?

▲ *Use empirical testing.* This one harkens back to the quote at the beginning of this article. Empirical verification goes beyond simple MATLAB-generated synthetic data

that conveniently matches all the theoretical model’s behavior. It means immediately aggressively seeking out real data from real users. Empiricism can also impact cultural exchange. For example, one can empirically verify from first-hand dialogue the perspectives of the world at large, sponsors, customers, and competitors.

Empiricism, and the other steps outlined above, takes guts and perseverance. It is not for the faint or weak hearted. But the rewards are staggering, nothing short of changing the world forever. Isn’t that why, in our youth, we chose engineering in the first place?

Acknowledgments

Thanks to David Perrine for helpful comments and Arye Nehorai for inviting this item in the first place. DARPA approved this for public release, distribution unlimited.

Allan O. Steinhardt received the Ph.D.



in 1983 from the University of Colorado, Boulder. He was a member of the technical staff, Lincoln Laboratory, MIT, from 1984 to 1987 and 1993 to

1996. From 1987 to 1993, he was an assistant professor at Cornell University. Since 1996 he has been with the Defense Advanced Research Projects Agency (DARPA), where he is currently a senior scientist, IXO. He has published 120 signal-processing-related journal and conference papers, and he coauthored *Adaptive Radar Detection* with Simon Haykin. At DARPA he started about a dozen new signal/sensor programs valued at nearly US\$200 million. He received the 1986 Paper Award, IEEE Signal Processing Society; the 1990 Best Professor Award, Cornell student IEEE; and the 1998 Outstanding Achievement Award, U.S. Secretary of Defense.

Call for Papers

IEEE Signal Processing Magazine

Special Section on Video and Signal Processing for Surveillance Networks and Services

Emerging surveillance networks and services increasingly rely on state-of-the-art signal processing methodologies found in a variety of disciplines, such as image and video processing, computer vision, communications systems, speech processing, statistical pattern recognition, and machine learning. In surveillance systems of tomorrow, signals generated by multiple sensors need to be processed, transmitted and presented at multiple levels in order to capture the different aspects of the monitored environment. Multiple level representations can be used to exploit perception augmentation for humans interacting with such systems. Situation awareness techniques that utilize multi-sensor inputs can provide enhanced indexing capabilities needed for focusing human attention on information of interest. Intelligent indexing and alerting functionalities rely on the capability of collecting and presenting relevant information in an efficient way on either fixed or mobile multiple control terminals. Multi terminal mobile and cooperative alarm detection in surveillance is another emerging problem where signal/video processing approaches becomes more and more relevant.

The main goal of this special section is to illustrate to the IEEE Signal Processing Magazine readership that video and signal processing methodologies represent a key aspect in the design of efficient and cost effective surveillance networks and services. Prospective contributors are invited to submit high quality tutorial papers that address emerging research issues related to the use of video and signal processing in the context of surveillance networks and services. Contributions offering a technology perspective, focusing on future trends in the area, or discussing business implications of different video and signal processing approaches in this domain, are strongly encouraged. See <http://www.cspl.umd.edu/spm/> for submission instructions and dates White papers are due 1 July 2003.

Guest Editors: K.N. Plataniotis, (University of Toronto, kostas@dsp.utoronto.ca) and C.S. Regazzoni (University of Genova, carlo@dibe.unige.it)