Personal Observations on Interdisciplinarity
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Introduction

This somewhat extended abstract is being composed after having benefited from attending the I^3R Meeting and having enjoyed the wide variety of perspectives and experiences communicated by so many knowledgeable participants. You might think that would make this an easier task, but the opposite is true. My "assignment" was to report on how I^3 relates to professional societies and journals I have known, a not unreasonable request given past associations with the MRS and its journal, JMR, particularly in their more formative years. Some recollections and some comments on current postures of MRS and JMR will be found in the section following. To be sure, there are manifold anecdotes one might relate about overcoming (or not) barriers raised by disciplinary preconception and much revered institutional norms. But to what end? On recalling my own involvements and on trying to discern the common elements in all I heard at our Meeting, I conclude that lessons learned from such accounts are, at the detail level, too situation-specific to be generally useful while at the same time being easily generalized to a few tenets that most of us by now find obvious in principle but that provide no actionable roadmap for implementing I^3 in a specific new arena. How can that be?

Other contributors are submitting the I^3R challenge to scholarly analysis and reporting on significant impediments and enviable achievements. In such a context, my observations at best may seem overly simplistic and could easily come across as a glib trivialization of the whole endeavor. Nevertheless, I cannot resist noting that the common themes permeating our entire discussion reduce to a few fundamental aspects of human nature well known to us all and ubiquitous not merely in our universe of science, technology and research, but in broader society. I feel it is important not to lose sight of this as one examines I^3 problems and solutions, for it is often the larger context that rises to thwart the best of local intentions.

Therefore, let's say what goes without saying. We are a risk averse species. This translates into resistance to change and thus to institutional inertia. When taken in concert with our subjective propensity to categorize and the objective need at any given stage of development to parse complex systems into manageable subunits, it becomes clear why rigid taxonomies are the rule. Be they definitions of departments on campus, political labels, or finding program classifications.

It is fair to say that in the examples presented at this gathering, the origin of hurdles confronting introduction of an I^3 approach in extant systems is the inevitable rigidity of existing structures. Similarly, successes seem to arise when one or more of a few criteria are met. Based on enlightened self-interest, the principal movers find the risk-reward calculus of success compelling enough to pay the price of breaking with tradition. This is often facilitated when the entrepreneurial venture does not risk the mainstay of the
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principals' vocations. Also, barriers are lowered considerably when the new $I^3$ enterprise is "green field," i.e., not making a frontal assault on a preexisting structure. Otherwise, the organizations involved must offer some open avenues to change or at least to circumvent boundaries. Having the right-sized, well-positioned resources is certainly also necessary.

Of course, it would be naïve to underestimate the importance of the details underlying any and all $I^3$ successes, but these must be devised in the milieu of the particular people and institutions involved. I believe no generic protocols for barrier reduction or for incentives that balance risk will fit differing circumstances. Attempts to map one successful formula onto a new situation are likely to disappoint. Likewise, general programs to promote $I^3$ in a top down fashion may raise awareness and enthusiasm, but are always tested at the bench — just as in technology transfer, $I^3R$ is a contact sport. Although not sufficient, it is nevertheless still clearly a necessary prerequisite to create and sustain genuine and practical inducements that mitigate the risk attendant to disturbing the status quo and blunt institutional disincentives.

MRS and JMR

There is no doubt that the Materials Research Society at its founding and to the present succeeds in crossing disciplinary boundaries with impunity. Success is defined here as producing programming across the science and engineering disciplines that is very well received by "customers" from all these disciplines. The last phrase is emphasized because attempts to mimic the MRS formula over the years have done less well when measured against the interdisciplinarity of the audience as opposed to that of the program per se.

How do the above generalizations apply to the nucleation and growth of MRS? The recognition that materials research in the real world was by its nature interdisciplinary (a premise already accepted in practice in industrial labs and in principle in some forward thinking enclaves in (D)ARPA and NSF) and that existing society forums were not adequate moved the MRS founders to offer a forum focused on topic regardless of discipline. The reward would simply be a more conducive forum for their own, their students', and their colleagues' research reports. The risk (i.e., penalty for failure) of this ancillary activity was virtually nonexistent and the required resources minimal. The MRS affinity group did not forsake their personal single discipline societies, so very little territorial back pressure on growth from traditional organizations was engendered. This is an excellent example of how, in the absence of personal and institutional barriers, a good idea with devoted backers will prosper and grow to a size sufficient to then survive being noticed by more traditional players.

MRS has so far successfully resisted attempts to "divisionalize" its programming and continues to rely heavily for its $I^3$ personality on a healthy high-turnover-cadre of enthusiastic (low-risk/high-reward) volunteers. Many new generation researchers consider the materials research field and the MRS, respectively, as their primary discipline and society. After a quarter century, today's challenge for MRS is continued vigilance against the threat of the creeping rigidity that often accompanies an organization's transition from an entrepreneurial to a bureaucratic stage.
The Journal of Materials Research (JMR) is a somewhat different story. MRS launched JMR in January 1986 after performing for several years all the due diligence studies one would normally expect for a new publication. Suffice it to note that the mechanics of business plan preparation were straightforward. These were pursued while MRS was still in a highly innovative entrepreneurial phase, actually coinciding with the establishment of its first stand-alone headquarters office and modest full-time staff. One admittedly self-serving rationale for the young MRS to launch a journal was the idea that a society is not really a society until it has its own journal. Materials research at the time was being touted in many quarters as the emerging field, a real growth area. More mature discipline-oriented organizations were therefore anxious to collaborate (i.e., collegially joint venture) with MRS. The American Institute of Physics was thus involved from the outset as JMR’s first publisher. So design, production, marketing, fulfillment, i.e., all the practical production considerations met no more challenges than any new publication might expect. There were few if any internal or external resisters of change to contend with. Where then did the I³R character of JMR enter the picture?

Those still influenced by a single-discipline mindset saw the great success of MRS meetings and their rapid growth and from these somewhat superficially concluded that its journal would automatically enjoy the same success. Of course a meeting and a journal are different things. MRS’s own community survey hinted strongly that presenters at MRS meetings would first choose to publish a finished archival research paper in their own favorite single-topic or single-discipline journal. And, typical MRS meeting attendees would first invest their valuable time reading those same more narrowly focused journals. The dominant reasons, i.e., that authors want to target their specific audience and that readers want to efficiently find and digest new developments within their own limited interests, were not the only reasons. The imprimatur imputed to the chosen periodical anoints its pages with a credibility and apparent significance that no new upstart journal could offer.

MRS was looking at several years of red ink that might never be recouped, but to its credit and after much sole searching, the ideal that JMR should reflect the panorama of materials research, even well beyond that common at MRS meetings, prevailed over the quite legitimate fears of the “focus or fail” proponents. JMR is now a fiscally sound, widely cited, well respected journal, able to maintain high standards of paper acceptance. True, some partial focusing strategies along the way to “capture” topics under-represented elsewhere have been employed. It is also true that, whereas JMR has become the primary outlet for some, it will likely never be that for the majority of narrow topics and disciplines that fit under the materials field umbrella. I regard the initial decision to remain broad and MRS’s continued comfort with JMR striving to be the best reflection of the field, even while being number two for narrower purposes, to be a victory for the I³R ethos.

A Conclusion of Sorts

Perhaps it is clear that I am not a fan of specially propounded programs to effect I³R. These I feel are all too often only of two types: (1) ineffective because words are not supported by
real resources and other necessary organizational follow-through, or (2) over-defined such that these programs themselves evolve along their own rigid lines allowing at most one generation of real interdisciplinarity. Common underlying aspects of \( \text{IP}^3 \) successes always seem to include a bit of chaos, lack of definition, and an unfettered settling-in process for collaborators that allows only the favorable and productive connections to form “organically.” This is tantamount to suggesting that non-structured formats, deconstructed taxonomies, multiply degenerate ground states, and continually moving research targets should be the rule.

A small dose of realism, of course, tells us that this highly creative and disorganized process must somehow couple to the larger very structured world of the ultimate customer. Therein, I suggest, lies the key clue to ultimate success. I believe pockets of \( \text{IP}^3 \) “Utopian” anarchy can actually be sustained, even within and between our structured institutions when enlightened intermediation is present. It’s clear from the presentations at this gathering that many here have played just this critical role in their own stories – that is they manage the interface so as simultaneously to protect innovation from undue premature interference, to provide an overall sense of direction without directives, and to satisfy the sources of resources that their long term goals are being served. A very tall order and probably the reason that we here hope to discover some best practices at meetings such as this.

**Appendix - Leftovers**

A plethora of disjoint issues concerning \( \text{IP}^3 \) and its context remains on my figurative notepad. These could be characterized as assorted symptoms for which I don’t pretend to have the cure nor even always have a clear idea of the root causes. Some were topics of at least brief discussion at our Meeting while others were overlooked. They are enumerated below as mercifully brief, albeit occasionally irreverent, snippets simply to encourage their further consideration in other future forums.

The “applied” versus the “basic” character of R&D within and between institutions are often are made rivals rather than partners. Basic folks resist being mere job shops while applied folks resist funding sand-box or blue-sky research.

As an editor, I decry the rapid disappearance of industrial researchers whose managers fail to reward, or even tolerate, time spent on scholarly works.

Ubiquitous misunderstanding of the multiprogram DOE national laboratories (conspicuously under-represented at our Meeting, as it happens) leads to their being simultaneously praised for addressing large complex multidisciplinary projects and chided by blue ribbon committees for not having been more narrowly focused on missions so that they might shed some of the very breadth on which their utility rests.

Workers in disparate fields with very different institutional goals speak different languages. Jargon is readily overcome but drastic differences in unspoken context will scuttle a collaboration if no one notices that participants are “talking past each other.”
Equitable assignment of the rewards of $1^3R$ can be another serious impediment. How personal credit is shared within teams and attributed to individuals in their home institutions will determine how collaborators interact. Intellectual property rights, a more concrete reward allocation issue, can scuttle collaborations when legal conservatism controls what ought to be primarily business decisions. Included here are the fuzzy notions of “pre-competitive” and “generic” research that supposedly avoids IP concerns. Does it then also avoid valuable commercializable discovery?

Interdisciplinarity requires some breadth in the topical expertise of each collaborator, but as with any investigation, forefront research demands deep understanding of the fundamentals and the details of each component task. How does one balance these apparently contradictory traits as reflected in curricula and postgraduate training? Put another way, can a “renaissance researcher” fill both bills?

Materials research tends to be owned or disowned from time to time by the disciplines, depending on whether credit for a materials discovery may be claimed. Typically, explicit acknowledgement of sister disciplines’ role is conveniently overlooked. Is there a substantive downside for the image of the field in the eyes of both students and funding sources because this otherwise somewhat amusing game?

Do slogans and buzz words such as “coopetition” or “collaboratory” help raise awareness and communicate the $1^3$ idea more accurately and succinctly, or are they mere advertising gimmicks?

Can anyone claim a net efficiency in $1^3R$ over less collaborative algorithms? I.e., is the extra cost in time and effort to needed to overcome traditional resistance to crossing those boundaries, outweighed quantitatively by increased value of results and/or increased productivity of the team?

Even accepting the practical barriers to actually accomplishing an $1^3$-type arrangement, why is it that we must continually re-explain why the concept per se is good? There are many things in life that are difficult to do but are obviously good for us if done. Why is $1^3$ different?

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