Transition: the Movement of Understanding from Process Studies to Ocean Forecast Models

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Abstract. The old ideas about basic research being a clear and immediate progenitor of practical technological applications have been replaced with modern concepts of research as an ongoing enterprise somewhat uncoupled in objective and culture from technology and engineering. Consequently, old ideas in the Department of Defense (DoD) about the impact of research on practical "warfighting" applications —and therefore how one manages the total enterprise from research through to applications—are being revised. The old ideas are encapsulated in the term "transition." This 'Aha Huliko'a concluded with discussion of essentially the same fundamental problem: how does one take the understanding that evolves from research on the physics of mixing and boundary layers in the ocean, and distill and embed that understanding into practical parameterizations for numerical models? Lessons learned from DoD are translated to this current scenario, and suggestions offered to help the process along.

Introduction

In the Department of Defense (DoD), the term that describes the movement of an idea from basic research, into applied research, into engineering development, and finally into application in some military system, is "transition." There is widespread agreement among defense researchers and program managers that transition is exceedingly difficult, usually extremely slow, remarkably erratic, and yet essential; it is one of the major reasons for DoD to sponsor a billion dollars a year in basic research. In the final analysis, the basic research enterprise in DoD needs to show that it has had an impact on the technology in use by the military services; otherwise, the health of defense basic research is endangered[1].

This 'Aha Huliko'a was very much about basic process studies in ocean mixing and boundary layers, how those fundamental ideas might affect the interior ocean, and how the physics and ideas and implications could be embedded into numerical models for use in ocean forecasting, be it on short or climate time scales.

The remarkable thing in the 'Aha, especially during the open discussions of the last day's sessions, was the similarity in objectives, process (or lack thereof), obstacles and frustrations to DoD's long history of attempts to transition technology from research to operations.

In the remainder of this sociological and political-science essay (this is clearly not a scientific paper!), I will discuss some aspects of DoD transition and lessons learned about how it works or doesn't work, draw some specific analogies to the 'Aha issues, and close with some suggestions for how the ocean science community might make better use of its research results in affecting what is done with them in applied or practical situations, like numerical forecasting of the state of the ocean, or climate change. A bit of a theme herein is the oft-quoted line about, "One person's signal is another person's noise." It may be an exciting research result to the basic researcher, but it is probably just a number to someone who is going to use it. I have discussed some of this material in another paper (*Briscoe*, 2003).

[1] Basic research has other impacts on defense applications, notably the production of smart people and new knowledge, both of which are demonstrably important to DoD. However, both have typically long time-scales until their impact is felt, and the impact tends to be diffusive and pervasive rather than easily traceable back to some smart PI and a wise program manager. Thus, the relentless attempt to find new and improved technology that is clearly related to some relatively recent basic research result, especially one funded by DoD.

The myth of transition

*Vannevar Bush (*1945), in his manifesto, "Science—The Endless Frontier," argued that basic research was the progenitor of new technology, hence the nation should get ready for the next great war or the next great economic revolution by heavy investments in basic research so as to ensure the availability of the needed technology. At that time, in that climate (after all, it was widely accepted that the physics leading to the atomic bomb had ended the war in the Pacific), there was no argument. The Office of Naval Research arose from that time and that argument, and half-a-decade later the National Science Foundation was born, nurtured by its first director (from ONR!).

In the early 1960's, President John Kennedy's Secretary of Defense, Robert MacNamara, tried to get his hands around the enormous bureaucracy of DoD by instituting a budgeting and budget-tracking process that lives today. DoD's fiscal system has various funding activities and levels. In the category of Research, Development, Test, and Evaluation ("RDT&E"), the breakdown is into Budget Activity 6.1 (basic research), 6.2 (was called exploratory development, is now called applied research), 6.3 (advanced technology development), 6.4 (advanced component development and prototypes), 6.5 (system development and demonstration), 6.6 (management support), and 6.7 (operational system development). Congress appropriates money into each of these lines, and it is illegal to move money between them (without permission from Congress) or to spend money from one funding level on an effort that is clearly in another category. DoD cannot spend basic research money to purchase equipment for the fleet, for example.

So a concept (linear, sequential, methodical transition of research ideas to useful technology) that had no demonstrable basis (other than trying to get Federal funds into basic research at the end of World War II) is now hard-wired into DoD process, and has been there so long that it is deemed improper—or even suspect —to question it. See *Gryskiewicz and Hills* (1992) for related issues.

Hindsight and TRACES

In the 1960's DoD decided to find out more about transition, so looked at 20 major weapons systems and asked "Where did all this stuff come from?" This was Project Hindsight (*Kostoff*, 1997), which concluded after looking back 15 years that the origin of most of the stuff (materials, electronics, algorithms, etc) could not be found, but in any case was not clearly related to investments (especially from DoD) in basic research. NSF then ran its own study, called TRACES, by looking back, picking some emerging research results, and asking what happened to them over time. They concluded that things like magnetic ferrite memories had, indeed, come from basic research investments. Both studies were well-intentioned, and both were flawed in methodology and conclusions. Nevertheless, the only real adjustment to the concept of transition was that it was hard to track and took a long time.

Modern concepts

Donald Stokes (1997) revisited the subject some years later and concluded that the impact of science on technology is quite slow, quite diffusive, and rarely traceable. However, he also concluded, the impact of technology on science was immediate, obvious, and could be readily identified. Just imagine that new tool that allows you to measure something you could not previously measure; new ideas and multiple papers come out right away.

Stokes also concluded that the prime objective of basic research is not technology, but new knowledge. And, as a corollary, he concluded that the prime objective of technology/engineering/applied studies is improved technology. Thus, science lives in its own world and improves itself; and technology improves itself. The commingling is minimal, and the short-term impacts are one-sided with technology mostly affecting science, but not vice-versa. On the long term, of course, the science enterprise produces smarter people and much new knowledge, hence the slow, "diffusive" impact of science on technology.

Hand in hand with the flawed concept of sequential, linear transition and of separate funding levels is the idea that each level can function alone, in isolation, and when it is finished with its work, hand off its results to the next level. So the basic researchers do their work, and publish, but do not work with the applied researchers, and so on. The language used even reflects this culture: "hand-off," and "throw [the result] over the transom."

Each funding level operated under its own ontology. Basic research was mostly done in academia; applied research in the DoD laboratories; and higher category work in industry. If a basic researcher were asked, "What are you doing to help transition?" the answer was usually, "That's not my job. I just do the basic research." But each level had this only-unto-itself attitude, so there was little transfer of knowledge or needs throughout the system.

The program mangers at each applied level received no awards for taking things from the lower levels, only for pushing them to the higher levels. So a 6.2 program manager—dealing primarily with early technology issues—could comfortably ignore what was going on in 6.1 so long as he managed to get 6.3 to pick up a few of his results. But 6.1 program managers did not have the same pressures; their culture was to get a result into publication, and then they were done.

I've painted a very bleak picture here, so as to make a point very clear: the fact that some very good basic research and improved understanding was going on had not much impact on technology, DoD applications, or war-fighting capabilities. At the very least, it had not had time to permeate; at worst, it would not permeate.

Vertical integration

In the early 1990's ONR began running an experiment in "vertical integration," namely the blending of 6.1, 6.2, and 6.3 funds under a single program manager in an attempt to remove some of the perceived barriers to transition. The first big experiment along this line was the Navy Ocean Modeling and Prediction program, or NOMP as it was known. The program manager was Bob Peloquin. He managed money from ONR, from the Office of Naval Technology (6.2), and from the Office of the Oceanographer of the Navy (6.3). A lesson-learned from this activity was that vertical integration made it easier for the needs and requirements of the more applied programs to be made known to the basic researchers, but it was not any easier to get them to work on those problems! Folks involved joked that, "If I think it up and work on it, it's basic research; if you think it up and tell me to do it, then it's applied research, even if it is the same piece of work."

Today, much of Navy science and technology funding is vertically integrated. ONR now controls all the 6.1, 6.2 and 6.3 funding in Navy. The other services are not so vertically integrated; they still live very much in the world of separate funding levels and separate work. Navy program managers tend to blend the basic and applied work across the funding boundaries; basic and applied are hard to separate in practice, and it is useful to let the same people do both the basic thinking and try out the applications.

Lessons learned

DoD in general, but Navy in particular has learned some lessons about transition, getting useful results from basic research, shortening the time for a new idea to get into practice, etc. The main lessons are these:

- Basic researchers prefer to choose their own problems to work on. Let them. But make sure they are informed of some of the tough obstacles and unanswered questions in the world of applications, and that they have access to problem-solving money as well as to new-idea money.
- "Hand-offs" from basic to applied research are ineffective. "Hand-holding" is a better paradigm, in which researchers from both cultures work together.
- A piece of basic research that just ends up in a journal is mostly lost to further application. Rare exceptions exist, but don't count on them. Basic researchers need to be encouraged to go beyond the publication by giving presentations to applied audiences (for example, an MTS meeting instead of an AGU meeting), by writing "so what" articles in more general publications, and by visiting DoD labs and appropriate industries and just talking about their work.
- Some folks are natural impedance matches between the basic and applied worlds; they communicate across the boundaries and get satisfaction from it. Find these people and nurture them.
- Real problems are usually multi or interdisciplinary. Basic researchers get their kudos from within a monolithic discipline. Breaking down the disciplinary barriers is essential. Some people are natural integrators/synthesizers. Find these people and nurture them.

- Some people view themselves as problem solvers, not as extenders of a body of knowledge. They are willing to learn new things, or to import old things from other fields, in order to make progress on the problem.
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Closing thoughts on the issue of transition: the reason we have a basic research enterprise is to produce new knowledge, smarter people, and to understand things better. This is good. But some fraction of the enterprise must do more; society demands that "useful" things come of the national investment in basic research, and they are unwilling to allow this to happen by slow serendipity. Thus, DoD tries to speed up transition and to have more of the basic research affect the technology and applications. Basic researchers can do several things to help:

- Applaud, rather than denigrate, a researcher who does something "useful" or talks at an applied meeting, or communicates to the public. This is not a demeaning of the purity of science; it is getting more from it than we get by just publishing in a reviewed journal.
- Try finding a tough "real-world" problem and then either collaborating with others to work on it, or learn yourself what you need to know. If the problem is typical, it will be at least multi-disciplinary and have something in it for everyone.
- Ask yourself about your research, "Who would care about this, or about some extension of it?" and then go find that person and tell them about it.

Transition of process study results to forecast models

We were concerned in this 'Aha with how the mixing in the boundary layers communicates to the interior; this is an understanding question. Let's suppose we actually knew the answer to the question; so what? For our understanding to have some impact, it needs to distilled, codified, incorporated into forecast models so that our state estimations and climate forecasts are improved.

In other words, how do we transition our improved understanding into application!

Drawing from the almost 60 years of lessons-learned in DoD[2], we can anticipate the following kinds of things happening.

- A boundary-layer researcher discovers a really solid result on turbulence in a stratified fluid, and consequently on mixing and movement of mixed fluid into the interior ocean. It is a highly geo-specific and time-variable result, but clearly has captured the physics and phenomenology. He/She publishes the result, and receives kudos from his/her colleagues.
- Ocean forecasters continue to use del-fourth or some other pervasive parameterization.
- Program managers get frustrated and demand some forecaster use the new and improved understanding.
- One brave forecaster tries to understand the mixing publication, spends much time trying to distill its essence, and is criticized by his/her colleagues for wasting time on a minor issue compared to resolution, assimilation, or air-ocean coupling.

How do we prevent this scenario, or at least work toward its mitigation?

Firstly, both the process folks and the forecasting folks need to get over it; they need to work together and collaborate on something neither of them knows how to do.

Secondly, the program managers need to foster the collaboration, and not put all the stress on the forecaster.

Thirdly, both communities need to applaud such efforts. This includes promotions and tenure committees, and permitting a paper describing the work to appear in the major journals.

Finally, more meetings like the 'Aha that bring separate communities together need to be encouraged.

[2] Transition has been a DoD issue for a long time, but it is only in the last 15 years, and especially the last 5-10 years, that overt understanding of transition and progress in dealing with it has been made.

Closing comments

Nothing I've said here should be taken as critical of either the basic or the applied research communities. They do marvelous work. My point is they don't do enough to communicate with each other, to help each other, or to make use of the remarkable fruits of the basic research enterprise. The basic researcher cannot stop by saying, "I've published, I'm done." The applied researcher cannot avoid fundamental questions by saying, "I'm only permitted to do an engineering fix; it's not my job to do the research."

Science is hard. It is asking a lot of a single individual to have breadth and depth and do basic and applied work, and communicate to the public. But collaboration is easy, comparatively, and can be a lot of fun.

I fully anticipate that the big breakthroughs in the parameterization of mixing in the ocean will come from strong collaboration between several people who get quite obsessed by the problem and will not let go if it. I wonder who they will be.

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