Innovation in Oceanographic Instrumentation

BY THOMAS B. CURTIN AND EDWARD O. BELCHER

INTRODUCTION

The tools of oceanography include instruments that measure properties of the ocean and models that provide continuous estimates of its state. Major improvements in tool capabilities lead to leaps in understanding, and this increased knowledge has many practical benefits. Advances in tool capabilities are sometimes viewed as an objective of basic research, a viewpoint reflected in the basic research funding category of "science and technology" (S&T).

The complexities of and incubation times for advancing instrumentation are often not fully appreciated, resulting in unrealistic expectations and discontinuous support. Greater understanding of the process of innovative instrument development can contribute to sustaining it. Innovation can be incremental or radical depending on performance gains (Utterback, 1994), stimulated or suppressed depending on institutional factors (Van de Ven, 1989; Office of Technology Assessment, 1995), and sustaining or disruptive depending on value propositions (Christensen, 1997). For example, going from a Nansen to a Niskin bottle was an incremental innovation, whereas going from bottle casts to CTD profiles was a radical innovation. Moored current meters incrementally advanced from film recording of gauges, to mechanically digitized signals on reel-to-reel tape, to solid-state analog, to digital conversion and memory. Radical innovation of current-field measurement came with the acoustic Doppler current profiler.

In large organizations, stimulated innovation often occurs in research departments, particularly when the projects have champions: "the new idea either finds a champion or dies" (Schon, 1963). In other parts of the same organization, innovation may be suppressed by the costs associated with re-integrating a system and minimal perceived competition. The incubation time of the computer mouse from inception to wide use was 30 years. In oceanographic observation, where synoptic coverage is an objective, a sustaining innovation would be a sampling platform with improved propulsion that doubles its speed. A disruptive innovation would be a new platform with much *slower* speed, but with much longer duration and a low enough cost to be deployed in great numbers. Here, we will focus on radical, stimulated, disruptive innovation that involves both science and engineering.

To motivate continued investment in basic research, the histories of many radical innovations, ranging from the transistor to radar to the Internet, have been documented (Bacher, 1959; Hetrick, 1959; Becker, 1980; Hove and Gowen, 1979; Allison, 1985; Abbate, 2000. The Defense Acquisition History Team at the US Army Center of Military History is also preparing a document on this subject.). These cases clearly demonstrate that "rapid" innovation in

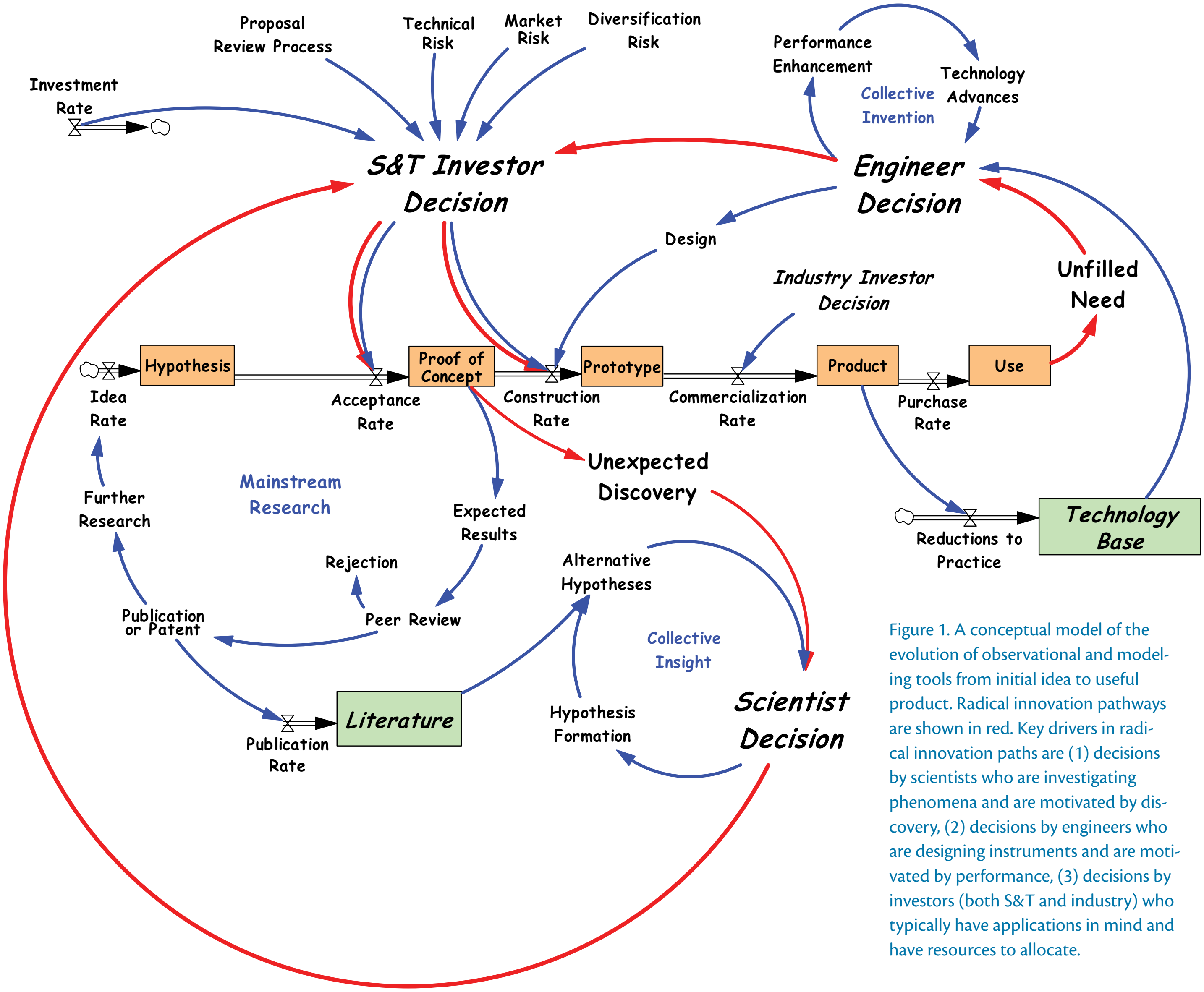

Figure 1. A conceptual model of the evolution of observational and modeling tools from initial idea to useful product. Radical innovation pathways are shown in red. Key drivers in radical innovation paths are (1) decisions by scientists who are investigating phenomena and are motivated by discovery, (2) decisions by engineers who are designing instruments and are motivated by performance, (3) decisions by investors (both S&T and industry) who typically have applications in mind and have resources to allocate.

science and technology is the product of a series of key insights, key decisions, and persistent effort. A simplified, conceptual model of the evolution of observational and modeling tools from initial understanding to useful products can be constructed as a sequential process with multiple feedbacks (Figure 1). The spark for radical innovation is often a scientist working at the limits of understanding making an unexpected discovery that requires better tools to investigate further, or an engineer working with state-of-the-art technology who has an idea for a performance jump with a number of potential applications.

Formulation and testing of hypotheses, and designing and testing prototypes both require financial resources. Decisions by all involved to seek and provide those resources will determine the fate of an embryonic innovation.

The probabilities of the scientist and engineer acquiring the needed resources are increased if a common investor can be found who has an in-depth

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understanding of the technical achievement and a broad awareness of its potential impact. Such understanding and awareness is generated by a feedback process involving education, communication, and brainstorming. The new paradigm of government investment in basic research established shortly after World War II was founded on investors (program managers) with scientific understanding (credentials) making decisions in this context (Bush, 1945).

The latencies and interactions in the feedback pathways, modulated by large-scale technological advances and serendipitous discoveries, determine the time scale of the radical innovation process. A discontinuity in investor priorities (e.g., a change in program managers) can cause a significant delay. Because decisions are required to drive the process, the expertise, intuition, and personalities of key people play a critical role. Successful innovation often depends on people's abilities to manage risk. Technology risk is managed by proceeding incrementally, testing frequently in situ, and maintaining constructive feedback. Market risk is managed by engaging potential users early in the engineering development loop. Single-point failure risk (in both technical approach and financial resources) is managed by diversifying the effort and maintaining some degree of competition. A few representative cases will serve to illustrate the complex web of interactions underlying specific instrument developments.

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TEMPERATURE-SALINITY PROFILING (FIGURE 2)

From the time of the 1872–1876 *Challenger* Expedition through the early years of World War II, temperature and salinity profiles were collected using reversing thermometers and water samples from Nansen bottles lowered to preselected depths on a wire. This process was laborious, particularly in bad weather, and could take several hours, but the resulting data, plotted in a T-S diagram, radically changed our understanding of the ocean by revealing a structure of interleaved, large-scale water masses with different sources, volumes, and mixing rates.

Temperature profiling advanced significantly through the invention of the mechanical bathythermograph (MBT) by Athelstan Spilhaus at the Woods Hole Oceanographic Institution (WHOI) based on a device called an oceanograph originally configured by Carl-Gustav Rossby of the Massachusetts Institute of Technology (MIT). The MBT used temperature- and pressure-sensitive elements to continuously plot a profile on a smoked glass slide. The MBT was improved at WHOI by Al Vine, who replaced the bimetallic temperature sensor with a bourdon tube sensor. The MBT had the advantage of being deployable while underway, although the speed and maneuverability of the ship were limiting factors. The MBT was in service in the US Navy until the early 1960s, when increasing threat from nuclear submarines made acquisition of more accurate, real-time temperature profiles a priority. Sippican, Bisset-Berman, and General Motors responded to a Navy solicitation with prototype devices that were tested off San Diego. Sippican won with its expendable bathythermograph (XBT). Key to Sippican's design was a very fine, two-conductor insulated copper wire suitable for "one shot use." The Navy ordered an initial production quantity for $9.79 each, and in 1968 negotiated a multiyear contract for about one million XBTs at the same price.

At about that same time, oceanographers began to use XBTs from research vessels, and a new approach to measuring salinity using electrical conductivity was developed to enable simultaneous profiling of this important variable. Initially, toroidal inductive cells were used by Bisset-Berman. To better define and control the sample volume, Neil Brown developed a four-electrode cell that was more stable and precise. Because electrical conductivity is sensitive to the complete ionic content in the sampled volume in contrast to the more selective chlorinity-based chemical method previously used to analyze water samples, a new equation of state for seawater was developed to provide cross-calibration with the established Seawater

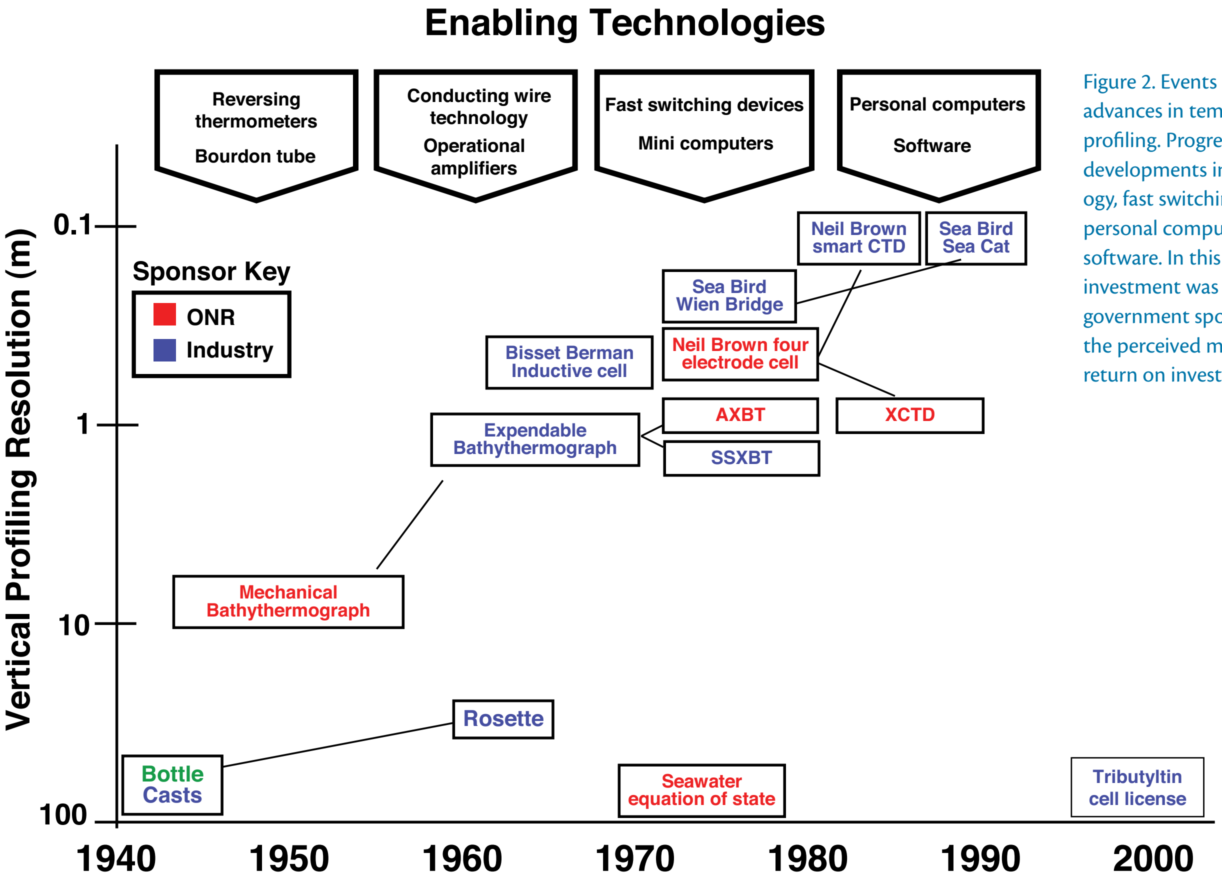

Figure 2. Events contributing to advances in temperature-salinity profiling. Progress hinged upon developments in wire technology, fast switching devices, and personal computer hardware and software. In this case, industry investment was on a par with government sponsorship because the perceived market justified the return on investment.

Standard produced in Copenhagen (Millero et al., 1980). Throughout the 1970s, much of the effort in temperature and salinity profiling was focused on adjusting and conditioning probe responses, including compensating for differing thermal and electrical response times that caused spiking. By the end of the 1970s, CTDs lowered on a conductor cable were in common use, profiling to full ocean depths, although data processing remained tedious.

The availability and increasing computational power of the personal computer in the 1980s significantly advanced CTD capabilities by enabling much more of the signal processing to be done in software rather than hardware. Sea-Bird capitalized on this technology and developed a new generation of CTD that was more robust, portable, and user-friendly. The 20-year revolutionary development of a precise, reliable CTD then transitioned to a more evolutionary trajectory as software tools matured and microprocessors and digital storage continued to follow Moore's Law (Brock, 2006).

ACOUSTIC COMMUNICATION (FIGURE 3)

Underwater voice communication systems using amplitude modulation techniques first appeared in Navy systems shortly after World War II. The AN/UQC underwater telephone employed single sideband modulation and operated over direct-path ranges to 8,000 yards. In the early 1960s, the UQC was replaced by the WQC-2 with a low-frequency band for greater operating range. At about the same time, researchers sponsored by the Office of Naval Research (ONR) conducted acoustic telemetry experiments using simple code-modulation techniques. In the decades following, engineers capitalized on advances in low-power, digital signal-processing hardware developed for other purposes. Commercial, inexpensive transistor chips in the 1970s led to digital, integrated circuits and to more sophisticated modulation schemes employing frequency-shift-keying and error-correction coding. Industry used this technology to develop underwater acoustic releases, and the Navy decided to use it in prototype digital acoustic communication systems.

Application-specific integrated circuits in the 1980s included low-cost, Fast Fourier Transform (FFT) signal processors that enabled acoustic telemetry systems with frequency-division-multiple-access (FDMA) modulation. Industry employed FDMA/FFT technology in the design of deep-water acoustic transponders used for precise undersea navigation and oil-well valve control. These applications demand high reliability but low data rate. Concurrently, researchers at MIT developed a higher-data-rate, digital acoustic modem designed to overcome multipath interference and reverberation. A throughput breakthrough occurred in the 1990s with the invention of very-high-data-rate, phase-coherent modulation schemes employing joint, adaptive equalization and synchronization to minimize intersymbol interference caused by multipath propagation. This approach resulted from the collaboration of researchers from MIT, Northeastern University, and WHOI. Progress was not always smooth. Current capability was enabled by a series of investor decisions made by a diverse and transient set of people from various agencies.

LENS-BASED SONAR (FIGURE 4)

The Applied Physics Laboratory, University of Washington (APL-UW) began investigating a spherical, liquid-lens sonar in the 1960s (Belcher, 1996). In 1992, the Naval Explosive Ordnance Disposal Technology Division (NEODTD) wanted to develop a technology for explosive ordinance disposal (EOD) divers to identify a mine in turbid water without tactile examination. Conventional sonars did not have the resolution required to pick out distinguishing features. Medical ultrasound systems operating in the 3–10 MHz range had the necessary resolution, but their range and field-of-view were

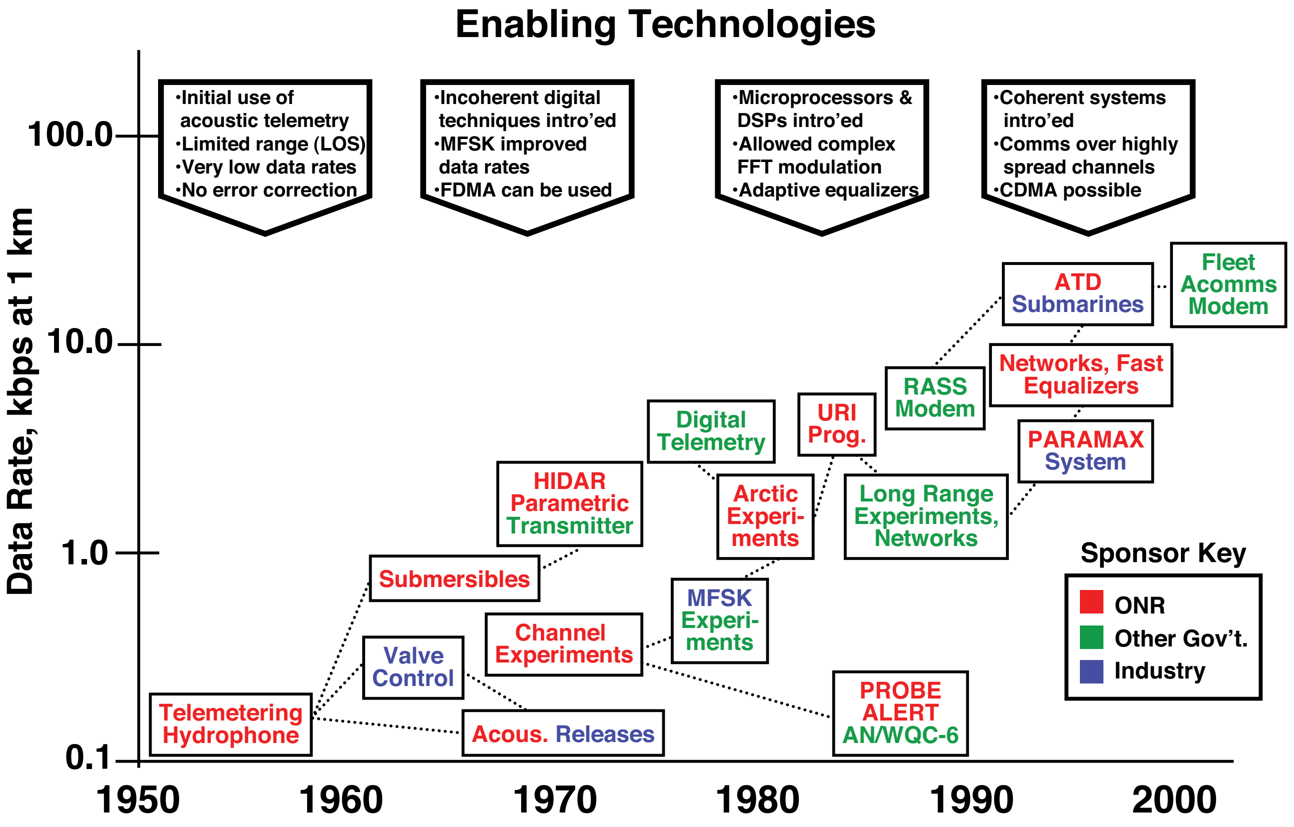

Figure 3. Events contributing to advances in underwater acoustic communication. Advances were built upon industry developments in low-power, digital signal processing hardware developed for other purposes. Progress was sustained by a series of investor decisions that shifted among agencies (Defense Advanced Research Projects Agency, National Science Foundation, Office of Naval Research) as available resources and priorities changed over time (Curtin and Benson, 1999).

limited to 10 cm. NEODTD needed a sonar that bridged the gap between conventional sonars and medical ultrasound. Simulations and verifications in test tanks indicated that a sonar operated at 3 MHz with 0.25-degree beams would provide the needed resolution. Digital and analog beam formers for these specifications were too large and required too much power for the desired diver-held sonar. The Naval Surface Warfare Center in Panama City (NSWC-PC) had experience in thin-lens designs that had advantages over the liquid lenses. In 1992, NEODTC brought the APL-UW and NSWC-PC teams together to design a lens-based sonar that operated at 3 MHz.

The NEODTD design goal was a two-dimensional, circular array of 128 by 128 beams. The processing required a custom chip; Q-Dot in Colorado and a Lockheed Martin Division in Massachusetts were funded to carry out the development. The Defense Advanced Research Projects Agency (DARPA) also provided funding. APL-UW believed a rectangular aperture and a one-dimensional array could also provide sufficient resolution to identify a mine, greatly reducing the processing demands. In 1993, NSWC-PC sponsored an experiment with rectangular lenses at 750 kHz with 0.5 by 14 degree beams ensonifying the seafloor. Excellent, detailed images of objects were generated. These results led to funding by the Office of Special Technology (OST) from 1994 to 1997 to develop a diver-held sonar using rectangular lenses and a one-dimensional array at 750 kHz. Three prototypes were delivered, decreasing in weight from 85 to 34 lbs (in air) while increasing in function and image clarity (Kamgar-Parsi et al., 1998).

In 1994, NSWC-Carderock sponsored the development of a very-high-resolution, 3-Mhz, fixed-focus scanning sonar using planar-concave lens elements, and in 1996 sponsored a one-dimensional, forward-looking sonar (3 MHz, 128 beams, 0.25 degree beam

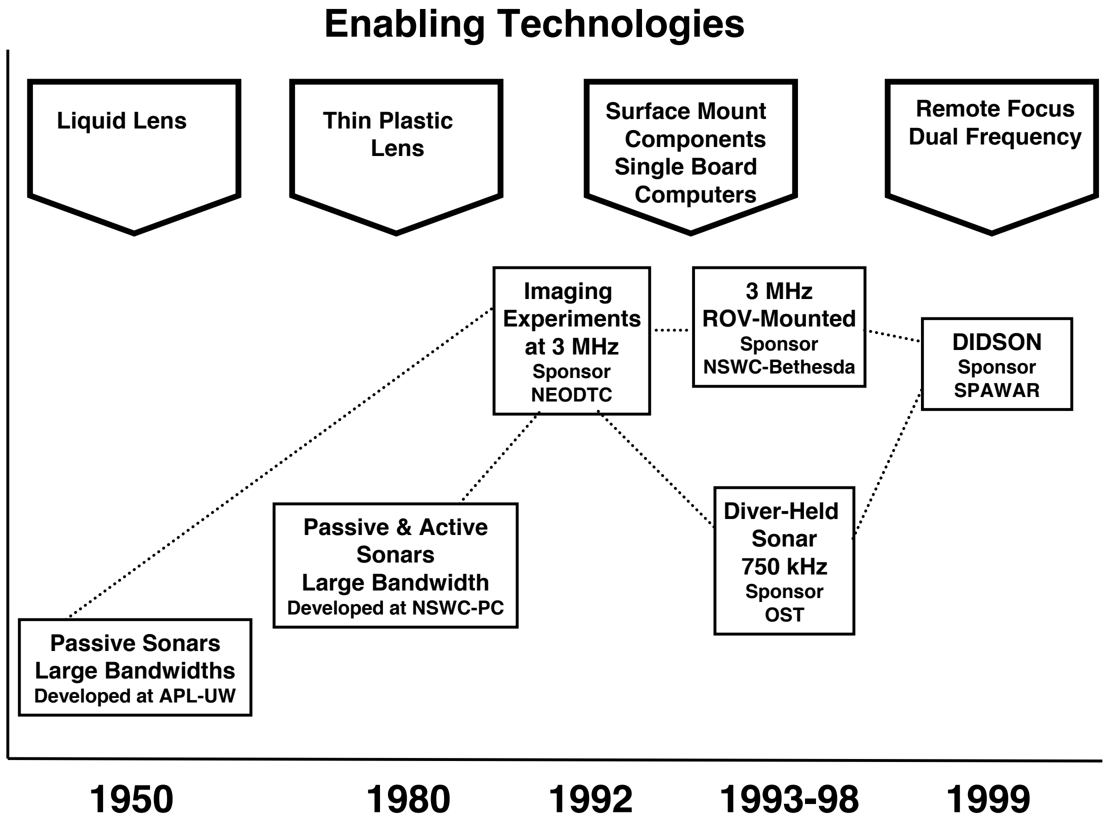

Figure 4. Events contributing to advances in high-resolution, lens-based sonar. Progress hinged upon developments in materials, surface-mount components, and digital signal processing chips. In this case, the discontinuities in sponsorship were bridged by in-house laboratory investment.

width). Both systems were mounted on remotely operated vehicles for examining and cleaning ship hulls in turbid water. The APL-UW group still remembers the phone call from Dana Lynn exclaiming that not only could they see individual barnacles but also peeling paint. No other sonar had that definition.

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A live video-stream allowed real-time feedback for guiding the remotely operated vehicle under the hull. The system's 3-m range was a significant limitation. In 1997, NSWC-Crane sponsored the development of two 750-kHz, lens-based sonars to monitor underwater tests at the Glendora Test Facility in Indiana. Also in 1997, OST and NEODTD sponsored the development of a 2 MHz lens-based sonar with a mask-mounted display made by NSWC-PC. The objective was to make it small and specialized for hull searches.

Parallel efforts, one driven by resolution (3 MHz) and one driven by range (750 kHz), converged in 1999 when the Space and Naval Warfare Systems Command (SPAWAR) sponsored the development of a dual-frequency, remotely focused, 96-beam sonar to identify divers initially detected by a large, low-frequency harbor surveillance sonar. The remote focus allowed a sharp image of targets from 40- to 1-m ranges. The sonar weight was 17 lbs in air and 1.5 lbs in water. The Dual-frequency Identification Sonar (DIDSON) released in 2000 appealed not only to SPAWAR but also to others. APL-UW started receiving orders—not for further development, but for copies of DIDSON. Early customers included WHOI, the Naval Sea Systems Command, the US Geological Survey, MagnaPatch, Battelle, and the Alaska Department of Fish and Game. When the team received its fifteenth order, the university agreed that DIDSON should be commercialized, and in 2002, Sound Metrics Inc. was formed to produce DIDSON. As of June 2008, over 216 DIDSONs are working in the field, deployed in three sectors: security, fisheries management, and commercial underwater inspection (Belcher, 2006).

OCEAN GLIDERS (FIGURE 5)

The ocean glider as an observation platform was first described by Stommel (1989) and then included in a broader class of autonomous vehicles by Curtin et al. (1993). The first prototypes were developed as part of a 1995 Department of Defense Multidisciplinary University Research Initiative investigating autonomous ocean sampling networks. This initiative was managed as a partnership among scientists, engineers, observers, and modelers. Objectives included improved spatial gradient estimation given sparse sampling and improved spatial field predictability by assimilating targeted observations (adaptive sampling) into models. Synoptic, spatial sampling and targeted observations required multiple, mobile, controllable platforms that would be affordable. Three parallel efforts produced complementary vehicles in the appropriate cost-size class: Slocum (Webb Research), Spray (Scripps), and Seaglider (University of Washington). After several years of testing, milestone deployments that demonstrated endurances of months occurred in the late 1990s in the Labrador Sea, Gulf of Alaska, off New Jersey, and in Monterey Bay (Curtin and Bellingham, 2001). These first-generation gliders were designed primarily for vertical profiling, and were not optimized for most efficient flight. In 2003, a comprehensive study of glider performance and trade-offs was completed by a group of engineers involved in the initial designs (Jenkins et al., 2003). This study led to the design and fabrication of a larger, faster, and more efficient glider in the form of a flying wing, which continues to be tested in 2008 (D'Spain et al., 2007).

In the early 2000s, first-generation research gliders were also deployed successfully by the Naval Oceanographic Office in several US Navy fleet exercises. Their demonstrated capabilities to measure the sound-speed structure in the upper ocean, even in harsh conditions, eventually led to a 2008 Navy acquisition program. Stimulated by a perceived growing market, the three first-generation gliders are now commercial products of acquiring corporations: Spray from Bluefin Robotics (Battelle),

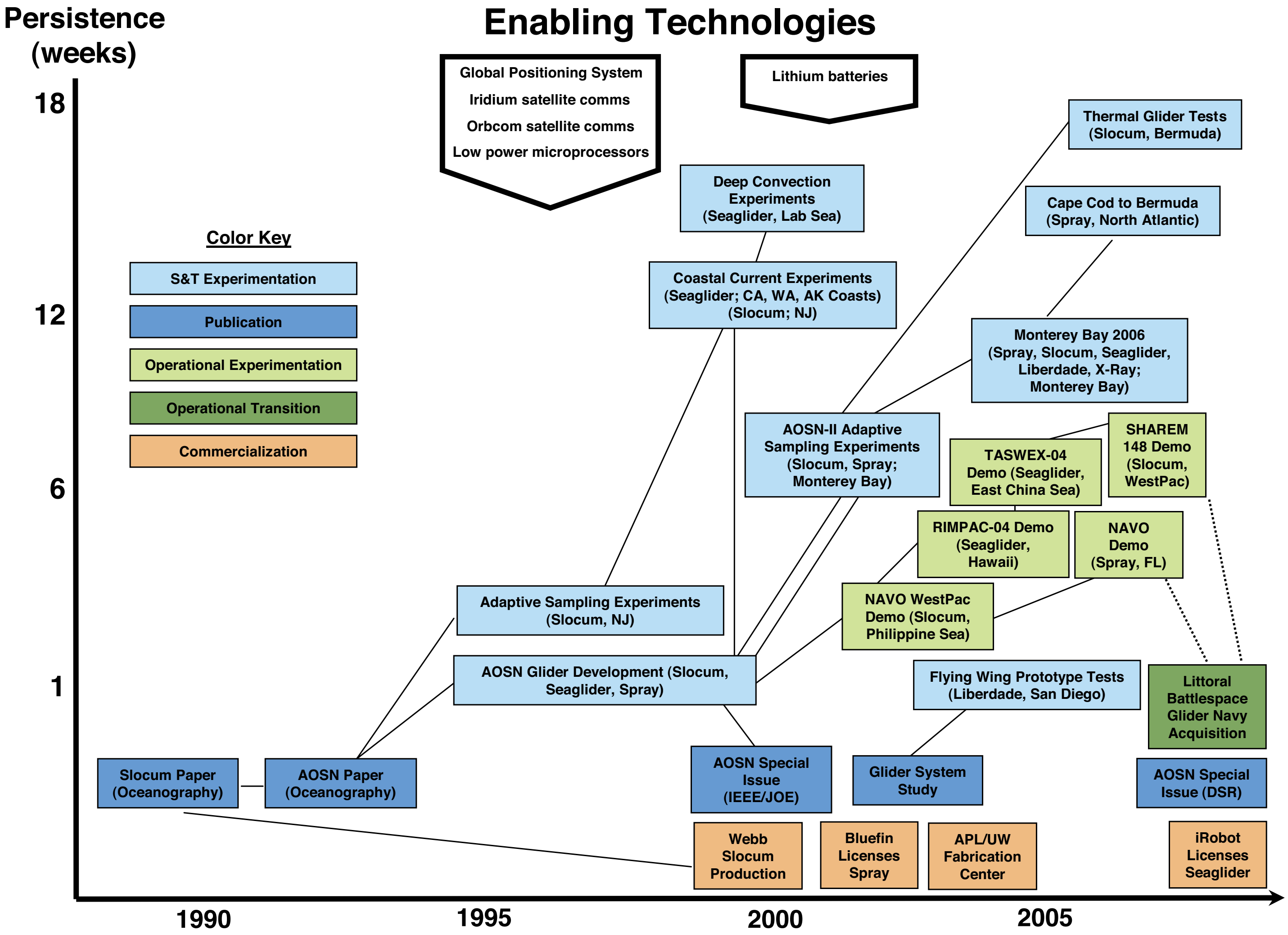

Figure 5. Events contributing to advances in underwater gliders. Progress hinged upon government-funded developments in GPS navigation and industry-funded developments in low-power, satellite communication. Other large-scale enablers include low-power microprocessors and associated sensors and signal processors and lithium-based batteries. In this case, funding was provided primarily by ONR in three parallel initial developments.

Seaglider from iRobot, and Slocum from Teledyne.

Although sometimes viewed as a radical innovation, the ocean glider is an integration of well-developed components (pressure housings, pumps/bladders, sensors, computers, batteries). The disruptive innovation lies in the integration of these components into an affordable, controllable, persistent mobile platform (system). The jump in ocean sampling potential afforded by gliders has been made possible by global satellite navigation and communication systems developed for other purposes, notably GPS and Iridium. This dependency is also a risk, because the oceanographic community has little influence on the future viability of these systems.

The capability offered by gliders is both a technical and conceptual innovation. The goal of achieving high-volume coverage and synoptic sampling was achieved with a design based on many slow, affordable, coordinated vehicles (a practical network). Slow speed provides long endurance. Affordability was viewed as analogous to the transition from mainframe to personal computers, and a strategy to capitalize on trends in low-power microprocessor technology was adopted. Disruptive jumps in capability for mobile vehicles of any type (maritime, air, ground) tend to be driven by radical engine innovation. The buoyancy engines in first-generation ocean gliders are based on mechanical pump technology. The thermal engine currently under development could produce a radical jump in endurance with a design goal of five years (40,000 km range). This thermodynamic engine derives its energy from ocean temperature gradients. A prototype is currently (summer 2008) running a section off Bermuda (Douglas Webb, Webb Research Corp., *pers.comm. 2008*).

CONCLUSIONS AND RECOMMENDATIONS

These and other cases indicate that the characteristic time scale for innovative oceanographic instrumentation to go from initial idea to useful product (Figure 1) is 15 to 30 years or more. Such time scales are incompatible with academic career advancement (tenure), and are longer than the typical residence time of investors (program managers). Without a champion creatively providing resources, a radical innovation is much more likely to languish. Even with a champion, the task is challenging. Christensen (1997) captures the essence of this challenge in describing the innovator's dilemma:

> *Managing innovation mirrors the resource allocation process: Innovation proposals that get the funding and manpower they require may succeed; those given lower priority, whether formally or de facto, will starve for lack of resources and have little chance of success. One major reason for the difficulty of managing innovation is the complexity of managing the resource allocation process. A company's executives may seem to make resource allocation decisions, but the implementation of those decisions is in the hands of a staff whose wisdom and intuition have been forged in the company's mainstream value network: They understand what the company should do to improve profitability. Keeping a company successful requires that employees continue to hone and exercise that wisdom and intuition. This means, however, that until other alternatives that appear to be financially more attractive have disappeared or been eliminated, managers will find it extraordinarily difficult to keep resources focused on the pursuit of a disruptive technology.*

Often, for accounting purposes, the evolution from idea to product is viewed as a series of sequential, monotonic steps. In reality, the evolution is likely to be tortuous. An efficient process requires closely coupled decisions of scientists, engineers, and investors in an environment of shared goals and constructive feedback. The process has both stochastic and deterministic components. On time scales shorter than the residence times of the three key participants, stochastic events (technical failures, scheduling delays, funding gaps) can be managed effectively by collaborative, creative decisions, and the process is relatively efficient. Efficiency drops considerably on longer time scales, due to the disruptive effect of personnel changes and a consequent degradation of commitment and corporate memory.

The cases described above illustrate these effects. Innovations in temperature-salinity profiling, acoustic communication, and lens-based sonar evolved over several decades. Their histories lack common sets of scientists, engineers, and investors that span significant time periods. Progress was made in response to arbitrary opportunities often precipitated by geo-political events. In contrast, the time scale of ocean glider innovation was half that of the other cases. A coherent set of scientists, engineers, and investors envisioned the scientific goal, understood the technological potential, and sustained the resources to see it through to its first plateau. It is fortunate that they were all in the right place at the right time.

Besides geo-political events that affect resource priorities, external (to the ocean sciences) technology developments must be superimposed on any of these processes. All innovations in ocean instrumentation have capitalized on external technologies developed for large or lucrative markets (shown along the top of the case-study figures). The applicability of and trends in enabling, external technologies are important factors in the timing and pace of investment.

Another factor that may have contributed to the relative efficiency of ocean glider development is initial publication 20 years ago of *Oceanography* itself. The ocean glider vision was set forth in two papers included in early volumes. Publications in peer-reviewed journals tend to focus on what has been done (observed, modeled, analyzed) with perhaps some suggestions for further research. *Oceanography*'s more flexible approach provided a channel to describe what could be done with gliders. The effect of communicating a compelling vision is hard to quantify, but the essential role of leadership in effecting radical innovation is universally acknowledged.

In addition to visionary leadership, oceanographic innovation would benefit from the following:

1. close coupling of science and engineering
2. a coherent investment strategy based on distributed, coordinated resources
3. effective processes for communication, feedback, and contingency planning
4. incentive to assume responsibility for risky instrumentation development projects without undue career jeopardy

A central goal of a distributed investment strategy is to sustain a critical mass of resources on a development effort to see it through the inevitable years of early failures and multiple field tests necessary to make instruments work robustly in the ocean. Distributed resources reduce the risk of crippling funding cuts in response to setbacks or uncontrollable budgetary events. Distributed resources appropriately integrated can also enable sufficient attention on a project for performers to be productive, especially in these days of ever-more-fractionated support. Substantial individual support can also contribute to institutional patience in the promotion process. Communication, feedback, and a culture of trust are essential. In the face of uncertainty, there is no substitute for good judgment informed by accurate data and guided by experience-based intuition.

In short, a recipe for efficient, radical innovation is to work in collaborative, multidisciplinary teams, be tenacious and focused on long-term objectives while producing short-term successes, and find creative champions among funding agencies and investor organizations. Be mindful of serendipity. Capitalize on opportunities.

ACKNOWLEDGEMENTS

Although insight is characteristically a solitary event, creating an innovative tool always involves many people. Similarly, a paper on innovation rests on the backs of many more people than can be practically acknowledged. Long hours at sea by many dedicated colleagues have produced the observational tools that exist today. Their efforts have been both humbling and inspiring, and are the real source material for what has been outlined here. Studies of entrepreneurial communities conclude that maintaining a culture of innovation is a critical factor. For the past 20 years, The Oceanography Society has nourished the self-reliant, creative culture at the heart of oceanography, and thus has also contributed in many ways to this work. Jim Hannon, Doug Bennett, and Doug Webb shared their long corporate memories. A special thanks to Mel Briscoe for insight and encouragement.

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