Nothing so Practical as a Good Theory: Reflections on 30 Years of Putting Innovative Ideas into Action[1]

Robert S. Kaplan

Let me start my narrative in May 1980. At that time, I was 40 years old, married to Ellen, a wonderful woman, and with two young daughters. I had become a tenured full professor at Carnegie Mellon business school in my fifth year on the faculty, which I had joined after receiving a Ph. D. in operations research from Cornell University in 1968. In May 1980, I was in my third year as Dean of the Carnegie Mellon business school, which was ranked in the top-10 in the US and considered one of the most innovative at the time. I had already published 45 articles in top-tier academic accounting and management science journals.

In summary, my personal and professional lives were good, and would become even better once I finished my term as dean and could return to being a full professor, writing more academic articles, teaching MBA and executive students, and supervising doctoral students. About this time, in my role as Dean, I asked a senior vice- president of a local company, Westinghouse, to join the school's Board of Advisers. He accepted, though on a conditional basis. He asked me to come to his office at Westinghouse headquarters in downtown Pittsburgh to talk about his concerns. In advance of the meeting, he even sent me a reading assignment, Hayes and Abernathy, "Managing our way to economic decline", Harvard Business Review, which he insisted I read before coming down to speak with him. Remember, I was a top academic at the time, which meant that I read academic journals, nothing as crass or practical as the Harvard Business Review.

At the meeting, the executive told me that business schools were behind the time. They were missing important phenomena that he was seeing in his business. He had been going to Japan for 20 years, the first 17 years as a teacher, but the past 3 years as a student. The Japanese companies were teaching him about Total Quality Management and Just-in-Time inventory policies, which he proceeded to explain to me. Now my Ph. D. was in operations research, and my thesis studied how to optimize inventory levels with random arrival times and uncertain demand. As I was now being told, Japanese had decided not to "optimize" inventory levels; they were devising ways to eliminate the need for inventory altogether. This was the early days of just-in-time (JIT) production systems. He told me that US companies were starting to catch on to Total Quality Management (TQM) and JIT, but business schools were completely unaware and clueless about these innovations and their implications for business.

I am, at heart, an empiricist, so I decided to check out this incredible claim. I launched an executive program on TQM at CMU, but had to hire faculty from HBS to teach in it. I sat in on the program and attempted to learn from it. I challenged the executives who were attending by claiming "economists teach us about U-shaped cost curves; doing too much of anything - reducing defects, eliminating inventory - runs into diminishing returnswhere the additional improvements (in better quality and lower inventory) incurs disproportionately more costs (i.e., marginal costs exceed marginal benefits)". One executive replied, "I wish you were the CEO of one of my competitors; we would destroy you through better quality and lower costs." With this and other observational evidence, I concluded that the executives were correct - business knowledge had become 3-5 years ahead of academic knowledge and the gap widened each year. How could this be?

If true, these innovations fundamentally changed the way we should be teaching cost and management accounting; basically we were using cost accounting concepts that were fine as of 75 years ago when they had been developed in the scientific management movement for the mass production of standard products with high labor content. But the new Japanese management approach had caused the cost accounting,

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as we were teaching and doing research on in business schools, to be obsolete.

At this point, I did what any good academic would do: I wrote a paper for my academic colleagues, "Measuring Manufacturing Performance: A New Challenge for Management Accounting Research" and submitted it to the leading academic journal in my field, The Accounting Review. The first review I got back from a referee stated, "This paper is the worst I have ever read or reviewed; if the journal's editor ever sends me another paper like this, I will resign from the journal's editorial board and never review another paper for it again." Fortunately, the editor, knowing who I was, trusted me enough to override the negative review and publish it. Four years later, it won my professional association's "Outstanding Contributions to Accounting Literature Award," the most distinguished prize of the American Accounting Association.

I also decided to get out of the dean's job as quickly as I could so I could start to work on the management accounting implications of the new production paradigm. I joined the faculty of the Harvard Business School in 1984 to pursue my new research agenda.

I joined HBS because I had started to realize that often (not always) the most innovative ideas and thinking arise first in business, not in a university or business school. Business faces continual competitive challenges, and somewhere, within that big sector, there are brilliant ideas and innovations. I wanted to get to a business school that would put me close to business and learn how to identify and access the most innovative businesses in the world. Over the next 25 years, I completely redefined how I would conduct research as an academic in a professional school:

1. Identify a major problem that pervades business that is currently inadequately addressed by contemporary business school teaching and research (Why work on an unimportant problem whose solution no one really cares about? And why work on problems that many academics are already pursuing - the "red ocean" - rather than the "blue ocean" where no other academics are working on?).
2. Identify a plausible solution in business that seems to address this problem- if you want to find out where the herd will be going, locate the lead steer, don't randomly choose an animal in the middle of the pack.
3. Study the solution, write and teach it.
4. Return to industry to put the idea back into practice

Let's see how this worked out in four different applications:

1. Costing

The first project I performed upon coming to HBS was a field study - go to US companies that were the most innovative in applying TQM, JIT and CIM (computer-integrated (flexible) manufacturing - efficient production in batch sizes of 1). I hoped to learn what new cost accounting procedures they had implemented to reflect these production innovations and write this in a paper for a Manufacturing Colloquium, part of HBS 75th anniversary year. I came back depressed and discouragedfrom multiple trips to the companies I had visited. The innovative companies were using the same cost accounting as if they were still mass producing standard products for inventory, rather than for individual customer orders. I observed a complete disconnect between manufacturing and costing. I told the colloquium organizers that my paper idea had blown up. But then I decided, like Sherlock Holmes, that the most interesting aspect of my initial field research was like the Sherlock Holmes case about the Dog that did not Bark in the night. The interesting phenomenon was the resistance of cost accounting to change. That became the topic of my paper and soon the foundation for my first HBR article on "Yesterday's Accounting Undermines Production," which won one of two McKinsey Awards that year as a best paper.

Based on that paper, I co-authored my first trade book, *Relevance Lost*. This year (2012) we celebrate the 25th anniversary year of its publication. In 2007 my academic colleagues in the American Accounting Association recognized this book with the Seminal Contribution to Accounting Literature Award, an award made every 3-5 years, for the most influential paper or book, published at least 20 years earlier.

So my journey had been launched with step 1: identification of a systematic gap in management practice along two dimensions, obsolete and distorted costing, and reliance on financial control systems that ignored improvements in quality, inventory reductions and flexible manufacturing for customization.

Step 2 now required that I participate in solving these huge measurement gaps. Fortunately, after publication of the initial HBR article, and teaching about the measurement gap in several executive programs, senior managers in several companies approached me with their attempted solutions to the problems that I had been writing and teaching about. Now that I was at HBS, I could study their innovations with case studies (done with Robin Cooper) and teach about them in a new second year MBA elective that Robin Cooper and I designed and taught. The case studies at various manufacturing plants of Scovill Corporation, John Deere, Hewlett Packard, Siemens (Germany), and a Swedish wire producer (Kanthal) led to the development of what we now call Activity-Based Costing.

As these innovative companies had independently begun to implement a more accurate way of assigning their overhead to individual products, Cooper and I developed the theory behind why ABC worked better than traditional overhead costing systems which relied on percentage markups over direct labor hours or dollars. Our theory showed that the demand for much indirect and support resources arose from transactions - such as setups, purchase orders, shipments - that were unrelated to volume of items ordered, produced or shipped. Other demands came from product variety - design and development of individual products. So these costs could not possibly be assigned to the volume or quantity of product produced; they were attributable to product variety, not quantity.

Robin Cooper and I wrote articles about this costing innovation, and then were asked by companies to assist them in implementing the idea. We formed a little company that did training, software and consulting for these companies - General Motors, Chrysler, Perkin Elmer, Northern Telecom, Hughes Aircraft etc. We learned how to implement ABC in these companies, and that led to a new round of case studies and HBR articles on both theory and implementation. We had become, in less than 10 years, the world's experts and thought leaders on costing.

But we still had to make one further theoretical advance - that actually came from a series of debates with Eli Goldratt, author of *The Goal and advocate for Theory of Constraints*. Goldratt, a Ph. D. in theoretical physics, was a very smart fellow who argued that "operating expenses" were fixed and "the goal" for production was to maximize throughput processed by the company's bottleneck resource; i.e., eliminate all unused capacity in the bottleneck resource, as I would now explain it. In order to respond to Goldratt's claim that operating expenses were fixed, I had to develop the theory about how to make spending variable, especially as the demand for the resource decreased. This led to making the distinction between the cost of supplying resources (what gets measured by your financial system) and the cost of using resources (the role for ABC). The key insight was measuring the cost of unused capacity as shown in the following equation:

Cost of supplying resources = Cost of using resources + Cost of unused capacity

This simple but powerful explanation provided the foundational "theory" for ABC. As you reduce the quantity of demands on the organization's capacity-supplying resources - through process improvements and lean initiatives, by increasing batch sizes, reducing product variety, etc - you create unused capacity in these resources, which managers can then redeploy or eliminate. So my response to Goldratt became, "ain't no fixed costs, only inattentive managers." This theoretical breakthrough provided the solid foundation for all ABC work.

Cooper and I then proceeded to apply our insights to multiple companies and continued to write cases, teach them in MBA and executive programs, consult with companies, speak at conferences, collaborate with ABC software companies, and codify our knowledge in textbooks (*Design of Cost Management Systems*) and trade books (*Cost & Effect*). At some point, the ABC movement became self-sustaining without much additional work from us.

2. Performance Measurement/Strategy Execution

The development of the BSC went exactly the same way. From my book, *Relevance Lost*, I knew that reliance on financial performance measurement and control systems was inadequate. One company, Analog Devices, approached me to help them with their costing of silicon wafers and chips. I wrote a case about their situation, which included a "corporate scorecard" for quality. The corporate scorecard supplemented financial measures with measures of customer quality, manufacturing quality, and employee "quality" - turnover and absenteeism.

About that time in 1990, I agreed to serve as an academic consultant to a project of the Nolan, Norton, a leading IT strategy consulting company, on "Measuring Performance in the Organization of the Future." The CEO, Dave Norton, launched this project because he believed,"Current approaches to performance measurement are based on an obsolete organizational Methods and are interfering with companies' ability to move into the future."

I presented much of my recent work to the 12 companies participating in the project, which included AMD, Apple Computer, Cigna Insurance, DuPont, GE, HP and Shell. Of all the material, they focused on the new Analog Devices case as the most promising opportunity for improving performance measurement. Norton and I re-formulated the Analog Devices "quality scorecard" into a scorecard based on strategy, which we labeled a "Balanced Scorecard." We wrote an article, published in HBR in Jan 1992, and the rest, as we say is history. The "theory" underlying the Balanced Scorecard was that financial measurements were inadequate to measure a company's progress in creating long-term and sustainable value. The source of such sustainable value, in 1990 and subsequently, would come from a company's intangible assets, which included the following:

- Customer loyalty and willingness to recommend
- Innovative product and service pipeline
- High quality and responsive operating processes
- Employee capabilities and motivation
- Data bases and IT systems
- Culture, alignment

At the time, the book values of a company's physical and financial assets were only 20-30% of its market value. For example, today the Indian IT services company Infosys has only $6 billion in assets on its balance sheet but its market value is $35 billion.

Norton and I believed in the scientific principle, "If you can't measure it, you can't manage it." So we proposed a comprehensive structure for measurement that illustrated how improvements in a company's intangible assets drove future financial performance.

The practicality of this idea soon became apparent. Several companies approached us to help them implement the concept, which Dave - a management consultant - was obviously prepared to do. Based on experience with half-dozen companies, we wrote two more HBR articles and our first book, published in 1996, which has now sold about 1 million copies in English and 24 other languages. As Robin Cooper and I had done with ABC, Norton and I worked closely with companies in US and subsequently around the world, to implement our new idea. We also extended the concept out to nonprofits and public sector enterprises. As we did this work, we kept writing articles, cases and books about what we learned from working with all these enterprises and from the executives who spoke at our conferences about their experiences implementing the BSC concept. The big transformation occurred among the first six clients that Dave and I worked with, when we saw how they were using the BSC, not just for performance measurement - our initial objective, but for strategy execution, an entirely new application.

Shifting the focus from performance measurement to strategy execution was the big theoretical breakthrough. In 1990, we had no idea about the problems that almost all companies experienced in their strategy execution. But as Norton and I worked with and listened to the company executives who were implementing our approach, we learned that the most important challenge they currently face was strategy execution. We then adapted our measurement framework to solve that challenge. So just like how much I learned from the senior Westinghouse executive in 1980 about the challenges he was facing, Norton and I learned about

the strategy execution challenges that executives were facing in the 1990s, and we developed a comprehensive approach for solving that challenge.

3. Risk Management

So now let's fast forward the story to 2007 and the onset of the global financial crisis. By September 2008, multiple companies in the US had failed - Bear Stearns, Merrill Lynch, Lehman Brothers, Citigroup (too big to have been allowed to fail), FNMA, Freddie Mac, and also GM and Chrysler. Additional companies failed in the UK, Ireland, Iceland and in Continental Europe as well. All triggered by a small (at the time) decline in US housing prices. How could this happen? How could solid companies allow themselves to become so vulnerable to a modest decline in the price of a single asset class - US residential homes?

Once again, for the third time, I had identified a major failure in companies' management systems - this time their risk management systems. As Yogi Berra once said, "It's déjà vu, all over again." By now, I already knew how to acquire the knowledge that might permit another theoretical breakthrough - find the few enterprises that operated in highly risky environments and study the risk management systems they used to protect themselves from failures. With another HBS colleague, Anette Mikes, we began to write cases on the best risk management systems we could find in diverse enterprises - sending unmanned missions to Mars, a large Canadian utility,and in the two Wall Street banks - Goldman Sachs, JP Morgan Chase, that had survived well the 2007/08 financial crisis. From this research, came an entirely new theoretical framework for thinking about risk - which we wrote about in a June 2012 HBR article, "Managing Risks: A New Framework." We continue to write cases about risk management systems and force ourselves to develop even better conceptual frameworks by teaching a new one - week HBS executive program on risk management that I created. This journey is still in its early days.

4. Health Care Value Framework

And the final evidence of the practicality of good theory is occurring in my other current project, a collaboration with strategy colleague, Michael Porter, on how to measure costs correctly in health care. The sound theoretical foundation of the newest variant of ABC, which I called time-driven ABC, enables us to measure costs of treating patients over a complete cycle of care, a measurement, that when combined with good outcome measures from the clinical treatment (Porter's contribution), should enable us to transform the delivery of health care. In this sector, any new costing system implemented must be able to withstand enormous quantities of skepticism, scrutiny and criticism. I am confident that the theory we have developed over the past 25 years will stand up to these challenges, and provide the basis for information that will enable health care systems around the world to improve the outcomes they deliver to patients while lowering their cost-to-serve.

Summary

All four of these work streams were inspired by problems that any of us could readily identify in practice. The advances, however, required an underlying belief by me that many management and societal problems, at the root, are caused by poor or inadequate measurements. Good measurements, grounded in micro-economic theory, provide a common platform that brings diverse stakeholders and perspectives to agree on the facts. People are entitled to their own opinions but they are not entitled to their own facts. The measurement innovations I have worked on during the past 30+ years strive to provide managers and employees with a "single version of the truth." Empowered by valid data, managers and employees can then work collaboratively to make better decisions, improve processes, create more value for customers, and, thereby, implement their strategies better. Activity-based costing, Balanced Scorecard, strategy maps and risk management are all solidly based in sound economic and organizational theory, and these concepts are proving immensely practical and valuable to enterprises around the world.

Thank you for the opportunity of sharing this 30 year journey with you.

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