Stefan Thomke

Why Business Experimentation Matters to Industrial Innovation[1]

At the heart of every company's ability to innovate lies a process of experimentation that enables the organization to create and refine its products and services. In fact, no product can be a product without it first having been an idea subsequently shaped through experimentation. Today, a major innovation project involves literally thousands of experiments, all with the same objective: to learn, through rounds of organized testing, whether the product concept or proposed technical solution holds promise for addressing a need or problem. The information derived from each round is then incorporated into the next set of experiments, until the final product ultimately results. In short, innovations do not arrive fully-fledged but are nurtured—through an experimentation process that takes place in laboratories and development organizations. In this brief article, we will examine the role and importance of business experimentation to industrial management and emphasize the need for senior management's involvement.

1 The material in this chapter comes in part from Stefan Thomke, Experimentation Matters, HBS Press, 2003. It has also been published (in German) in Innovationen - Verspechen an die Zukunft, edited by Thomas Ganswindt. Hamburg: Hoffmann und Campe, 2004.

A Challenge in Industrial Management

All industrial organizations have an - explicit or implicit - experimentation process, but few senior managers organize that process to invite innovation. In fact, the book In Search of Excellence noted years ago:

"The most important and visible outcropping of the action bias in the excellent companies is their willingness to try things out, to experiment. There is absolutely no magic in the experiment. It is simply a tiny completed action, a manageable test that helps you learn something, just as in high-school chemistry. But our experience has been that most big institutions have forgotten how to test and learn. They seem to prefer analysis and debate to trying something out, and they are paralyzed by fear of failure, however, small."[1]

This holds especially in the development of new products and services, where no idea can become a product without having been shaped, to one degree or another, through the process of experimentation. But experimentation has often been expensive in terms of the time involved and the labor expended, even as it has been essential in terms of innovation. What has changed, particularly given new information-based technologies available today, is that it is now possible to perform more experiments in an economically viable way while accelerating the drive towards innovation. Not only can more experiments be run today, the kinds of experiments possible is expanding. Never before has it been so economically feasible to ask "what-if" questions

1 Peters and Waterman (1982). In Search of Excellence. New York: Harper& Row, pages 134-135.

and generate preliminary answers. New technologies such as computer modeling & simulation and rapid prototyping enable organizations to both challenge presumed answers and pose more questions. They amplify how innovators learn from experiments, creating the potential for higher performance and new ways of creating value for firms and their customers. At the same time, many companies that do not fully unlock that potential because how they design, organize, and manage their approach to innovation gets in the way. That is, even deploying new technology for experimentation, these organizations are not organized to capture its potential value – in experimentation, in innovation.

"Experimentation" encompasses success and failure; it is an iterative process of understanding what doesn't work and what does. Both results are equally important for learning, which is the goal of any experiment and of experimentation overall. Thus, a crash test that results in unacceptable safety for drivers, a software user interface that confuses customers, or a drug that is toxic can all be desirable outcomes of an experiment – provided these results are revealed early in an innovation process and can be subsequently reexamined. Because few resources have been committed in these early stages, decision-making is still flexible, and other approaches can be "experimented with" quickly. In a nutshell, experiments that result in failure are not failed experiments – but they frequently are considered that when anything deviating from what was intended is deemed "failure".

Herein lies the managerial dilemma that innovators face. A relentless organizational focus on success makes true experimentation all too rare. Because experiments that reveal what doesn't work are frequently deemed "failures," tests may be delayed, rarely carried out, or simply labeled verification, implying that only finding out what works is the primary goal of an experiment. If there is a problem in the experiment, it will, under this logic, be revealed very late in the game. But when feedback on what does not work comes so late, costs can spiral out of control; worse, opportunities for innovation are lost at that point – reinforcing the emphasis on "getting it right the first time." By contrast, when managers understand that effective experiments are supposed to reveal what does not work early, they realize that the knowledge gained then can benefit the next round of experiments and lead to more innovative ideas and concepts – early "failures" can lead to more powerful successes faster. IDEO, a leading product development firm, calls this "failing often to succeed sooner."

Experimentation And New Knowledge

The pursuit of knowledge is the rationale behind experimentation, and all experiments yield information that comes from understanding what does, and does not, work. For centuries, researchers have relied on systematic experimentation, guided by their insight and intuition, as an instrumental source of new information and the advancement of knowledge. Famous experiments have been conducted to characterize naturally occurring processes, to decide among rival scientific hypotheses about matter, to find hidden mechanisms of known effects, to simulate what is difficult or impossible to research: in short, to establish scientific laws inductively. Some of the most famous series of experiments have led to scientific breakthroughs or radically new innovations from which we still benefit today.

Louis Pasteur's discovery of artificial vaccines is one example. Pasteur had been struggling for years to understand the course of disease, in this case cholera, and ran extensive experiments to accumulate a knowledge base to help him make sense of what experiments in his laboratory were yielding. In 1879, he returned from a summer vacation not realizing that chicken broth cultures, part of one ongoing experiment, had become infected. He thus injected his hens with the infected culture and followed that with injections of fresh, virulent microbes. What he discovered in this process was that the mild disease the infected cultures gave rise to forestalled the deadly form from occurring. Pasteur was able to compare the results of previous experiments with recent ones and thereby draw accurate conclusions based on the knowledge accumulated over the course of all these experiments [2].

Nearly a century later, the discovery of 3M's Post-It adhesive demonstrates the role of experimentation in the discovery of both technical solutions and new market needs. The story began in 1964, when 3M chemist Spencer Silver started a series of experiments aimed at developing polymer-based adhesives[3]. As Silver recalled:

"The key to the Post-It adhesive was doing the experiment. If I had sat down and factored it out beforehand, and thought about it, I wouldn't have done the experiment. If I had limited my thinking only to what the literature said, I would have stopped. The literature was full of examples that said that you can't do this. [4]"

Although Silver's discovery of a new polymer with adhesive properties departed from predictions of current theories about polymers, it would take 3M at least another five years before a market was determined for the new adhesive. Silver kept trying to sell his glue to other departments at 3M, but they were focused on finding a stronger glue that formed an unbreakable bond, not a weaker glue that only supported a piece of paper. Market tests with different concepts (such as a sticky bulletin board) were telling 3M that the Post-it concept was hopeless – until Silver met Arthur Fry. Fry, a chemist and choir director, observed that members of his choir would frequently drop bookmarks when switching between songs. "Gee," wondered Fry, "if I had a little adhesive on these bookmarks, that would be just the ticket." This "Eureka moment" launched a series of experiments with the new polymer adhesive that broadened its applicability and ultimately led to a paper product

2 Hare (1981). Great Scientific Experiments. Oxford: Phaidon Press, page 106.

3 The following account is based on Nayak and Ketteringham (1997). "3M's Post-it Notes: A Managed or Accidental Innovation?" In R. Katz, The Human Side of Managing Technological Innovation. New York: Oxford University Press.

4 Nayak and Ketteringham (1997), page 368

that could be attached and removed, without damaging the original surface. In other words, repeated experimentation was instrumental in finding the now obvious solution, once the "Eureka moment" occurred.

While such "Eureka moments" make for memorable history, they do not give a complete account of the various experimentation strategies, technologies, processes, and history that lead to scientific or innovative breakthroughs. After all, such moments are usually the result of many failed experiments and accumulated learning that prepare the experimenter to take advantage of the unexpected. "Chance", noted Louis Pasteur," favors only the prepared mind."[5] Consider what the authors of a careful study of Thomas Alva Edison's invention of the electric light bulb concluded:

"This invention [the electric light], like most inventions, was the accomplishment of men guided largely by their common sense and their past experience, taking advantage of whatever knowledge and news should come their way, willing to try many things that didn't work, but knowing just how to learn from failures to build up gradually the base of facts, observations, and insights that allow the occasional lucky guess – some would call it inspiration – to effect success."[6]

When firms aim for breakthrough innovations, however, senior management cannot rely on luck or even lucky guesses alone; experimentation must be organized and managed as an explicit part of a strategy for pursuing innovation itself. At the same time, the serendipitous may be more likely when an effective experimentation strategy is in place and new experimentation technologies are integrated into it. The serendipitous is also more likely when experimenters are clear that understanding what does not work is as important to learning as knowing what does.

If we attempt to add up all the significant experiments that have been carried out since the Greeks began systematic scientific studies around 400 BCE up until the 19th century, we can probably say that the number is in the millions. If we then include experiments initiated in industrial R&D laboratories since the 19th century, the number perhaps reaches several hundred million. That number, in turn, will be dwarfed by the billions or trillions of experiments we will run with computers, combinatorial technologies and other methods in the coming decade alone, fundamentally challenging how innovation will happen. The sheer quantity of inexpensive experimentation possible with these new technologies, along with the knowledge gained from them, will make the "lucky guess" much more likely as long as companies are willing to fundamentally rethink how they research and develop new products and create value for their customers.

Learning from Success and Failure

All business experimentation—whether conducted in Ancient Greece, in Edison's laboratory, or in the presence of simulation or other sophisticated technology today– generates knowledge. That knowledge, however, comes as much from failure as it does from success. Innovators learn from failure: again, understanding what doesn't work is as important as understanding what does. The next round of experimentation should benefit equally from either result. Further, knowledge of either failure or success itself can be stockpiled, providing a resource that, if not applicable to one set of experiments, can be used for subsequent inquiries.

For example, IDEO Product Development, a leading design firm, maintains a "Tech Box" for stockpiling experiments from finished and on-going projects. This giant "shoebox" for cataloging and electronically documenting materials, objects and interesting gadgets is used to inspire innovators in new development projects. A curator organizes and manages the content of the Tech Box and duplicates its contents for other IDEO offices – and occasionally to other companies – throughout the world. Designers and engineers can rummage through the box and play with an assortment of switches, buttons, and odd materials that were all part of successful or failed experiments. The Tech Box underscores the fact that one can never fully anticipate what tools and materials would be required in an experimental project that involves great novelty. Edison learned this lesson early in his career and later tried to have everything at hand in his West Orange laboratory. Thus, when Edison noted that "the most important part of an experimental laboratory is a big scrap heap," he leveraged a well-stocked storeroom and a collection of apparatus, equipment and materials that came from previous experiments. The larger the scrap heap, the wider the search space for Edison and his experimenters and the more likely it was that somewhere in this pile, the solution would be found.

Similarly, pharmaceutical companies stockpile very small quantities of discrete chemical compounds in "chemical libraries," which are used in the search for new drugs. Many of these compounds were generated in prior drug development projects and showed therapeutic promise in complex experiments involving either simple screening procedures or living organisms. Consisting of several hundred thousand compounds and information on their specific properties, these libraries are used to find leads in new drug projects where access to chemical diversity has been an important competitive advantage. Such libraries and the associated information on how and where to use them represent a long history of investments into scientific research, experimentation and strategic commitment; the Economist has referred to them as one of pharmaceutical companies' "most carefully guarded assets".[7]

The fact is, when pharmaceutical companies such as Eli Lilly launch new drugs or automotive firms like BMW introduce new cars, these products are the result of as many failed experiments as successful ones. An innovation process, overall, should assure the gradual accumulation of new knowledge that will guide the path of development itself. This new knowledge, however, is at least partially based on "accumu-

5 Quoted from Hare (1981), page 106.
6 Quoted from Friedel and Israel (1987). Edison's Electrical Light: Biography of An Invention. New Brunswick, NJ: Rutgers University Press, page xiii.

7 Quoted from the Economist (1998), "A Survey of the Pharmaceutical Industry," 21 February, pages 9-10.

lated failure" that has been carefully understood.

The reason why experiments inevitably fail as part of product development effort has to do with the uncertain nature of the innovation process itself. When teams undertake the development of products or services – particularly novel or complex ones, they rarely know in advance whether a particular concept will work as intended. That means they have to find ways of rapidly discarding dysfunctional ideas while retaining others that show promise. At the same time, the "dysfunctional ideas" themselves have generated knowledge and should, as such, be captured. Edison understood this very well when he noted that "Just because something doesn't do what you planned it to do doesn't mean it's useless. Reverses should be an incentive to great accomplishment. Results? Why, man, I have gotten lots of results! If I find 10,000 ways something won't work, I haven't failed. I am not discouraged, because every wrong attempt discarded is just one more step forward"[8]. A century later, academic research on R&D organizations showed these insights to be more relevant than ever: project teams spent an average of 77% of their time on experimentation and related analysis activities to resolve uncertainty[9].

Not all uncertainty is alike, however (see Table 1). Technical uncertainty arises from the exploration of solutions (e.g., materials) that have not been used before, or have not been combined in "this" way before, or miniaturized in such a way before. As such, it often relates to product functionality and can be managed through rigorous prototype testing throughout development. Production uncertainty exists when we do not know if a technical solution that works well in prototypes can also be produced cost-effectively. What may work in small quantities may not be feasible when production ramps up: the entire manufacturing process itself may need to be revised. At every stage of R&D, technical and production uncertainty exists and needs to be managed, in part through a systematic process of experimentation.

Type of Uncertainty	Questions that Experimentation Addresses
Technical	Does the product/service ("it") work?
Production	Can it be produced or delivered cost-effectively and at high quality
Customers Needs	Does it address and satisfy customer needs?
Market	Does the market size justify the resource investment?

Table 1. Experimentation and uncertainty

Beyond technical and production uncertainty, rapidly changing customer demands create need uncertainty, another critical reason for rigorous experimentation. Customers are rarely able to fully specify all of their needs because they either face uncertainty themselves or cannot articulate their needs on products that do not yet exist. If they have neither seen nor used such a product before, they themselves will have to experiment before arriving at a recommendation. Finally, when innovations are "disruptive," as research has shown, market uncertainty can be so significant that firms are reluctant to allocate sufficient resources to the development of products for those markets[10]. In such cases, the composition and needs of new markets evolve themselves, and are either difficult to assess or change so quickly that they can catch good management by surprise. To successfully harness the opportunities of disruptive change, successful managers rely in part on experimentation[11]. According to the research, such managers "planned to fail early and inexpensively in the search for the market for a disruptive technology. They found that their markets generally coalesced through an iterative process of trial, learning, and trial again."[12] An effective experimentation strategy addresses innovation opportunities in all four areas: technical, production, need and market uncertainty. My research has shown that such a strategy encompasses the following principles[13]:

- Organize for rapid experimentation.
- Implement "front-loaded" processes that identify potential problems before resources are committed and design decisions locked in.
- Experiment and test frequently but do not overload an organization.
- Integrate new technologies into the current innovation system.
- Fail early and often but avoid wasteful "mistakes" that produce no useful information and are therefore without value.
- Manage projects as experiments

Conclusion

I frequently ask management audiences to list all the business experiments that they are aware of in their companies. After all, in the absence of similar experiences or good predictability of outcomes in complex business settings, true experimentation is the only way to manage uncertainty and identify promising innovation in the future. Projects that become experiments after they are finished or during late stages don't count because they usually provide few opportunities to learn. However, projects that are designed, funded and managed as experiments (i.e., maximize learning from success and failure) do matter and should be an integral part of a firm's innovation strategy.

I have found that few managers can prepare such a list or present a portfolio of business experiments – even though they know that experimentation is the lifeblood of new products, services and business opportunities. Thomas Edison could not have said it more clear-

8 Quoted from www.thomasedison.com/edquote.htm

9 Allen (1977). Managing the Flow of Technology. Cambridge, MIT Press, chapter 4.

10 The work on disruptive technology and its role in why firms fail is discussed in Christensen (1997). The Innovator's Dilemma: When New Technologies Cause Great Firms to Fail. Boston: Harvard Business School Press.

11 Garvin (2002), "A Note on Corporate Venturing and New Business Creation," Note 302-091, Harvard Business School, argues that new business or ventures can be regarded as experiments where direct contact with the marketplace is essential to exploration and validation, particularly for radically new businesses where the usual sources of knowledge provide only limited insight.

12 Quoted from Christensen (1997), page 99.

13 For more information on these principles, see S. Thomke (2003), Experimentation Matters.

ly: "The real measure of success is the number of experiments that can be crowded into twenty-four hours.[14]"

Author:

Stefan Thomke, an authority on the management of innovation, is the William Barclay Harding Professor of Business Administration at Harvard Business School. He has worked with US, European and Asian firms on product, process, and technology development, organizational design and change, and strategy.
Since joining the Harvard faculty in 1995, Professor Thomke has taught and chaired numerous MBA and executive courses on innovation management, R&D strategy, product & service development, and operations, both at Harvard Business School and in individual company programs in the United States and abroad. He is chair of the Executive Education Program Leading Product Innovation, which helps business leaders in revamping their product development processes for greater competitive advantage, and is faculty chair of HBS executive education in India. Professor Thomke is also on the core faculty of the Advanced Management Program (AMP) where he teaches the course Leading Innovation. Previously, he was faculty chair of the MBA Required Curriculum and faculty co-chair of the doctoral program in Science, Technology and Management (S,T&M).

Stefan Thomke, Ph.D.
William Barclay Harding Professor of Business Administration Harvard Business School, Boston (USA)

Professor Thomke's research and writings have focused primarily on the process, economics, and management of business experimentation in innovation.
He is a widely published author with more than three dozen articles, cases and notes published in books and leading journals such as California Management Review, Harvard Business Review, Journal of Product Innovation Management, Management Science, Organization Science, Research Policy, Sloan Management Review, Strategic Management Journal and Scientific American. He is also author of the books Experimentation Matters: Unlocking the Potential of New Technologies for Innovation (Harvard Business School Press, 2003) and Managing Product and Service Development (McGraw-Hill/Irwin, 2006).

Professor Thomke was born and grew up in Calw, Germany. He holds B.S. and M.S. degrees in Electrical Engineering, a S.M. degree in Operations Research, a S.M. degree in Management from the MIT Sloan School of Management, and a Ph.D. degree in Electrical Engineering and Management from the Massachusetts Institute of Technology (MIT) where he was awarded a Lemelson-MIT doctoral fellowship for invention and innovation research. Prior to joining the Harvard University faculty, he worked in electronics and semiconductor manufacturing and later was with McKinsey & Company in Germany where he served clients in the automotive and energy industries.

14 Quoted from Millard (1990). Edison and the Business of Innovation. John Hopkins University Press, page 40.

Call for Papers

Themenschwerpunkt: Due Diligence

in WINGbusiness 04/2011

Beschreibung

Für die Ausgabe 04/2011 laden wir Autoren ein, wissenschaftliche Artikel (WINGPaper) zum Thema „Due Diligence" einzureichen.
Gerne nehmen wir Arbeiten entgegen, welche den Themenkreis „Due Diligence", unter besonderer Berücksichtigung der theoretischen Grundlagen und Funktionen des „Due Diligence", sowie der praktischen Umsetzung als auch die Erstellung von Reports, vorrangig im Immobiliensektor, zum Inhalt haben.

Hinweise für Autoren:
Vorlagen zur Erstellung eines WING-papers und konkrete Layout-Richtlinien sind als Download unter: http://www.wing-online.at/services/wingbusiness/medienfolder.html oder per E-Mail verfügbar.
Autoren können ihre Beiträge zum Themenschwerpunkt als PDF an office@wing-online.at übermitteln.

Die Ergebnisse des Reviews werden dem Autor innerhalb von 4-8 Wochen nach Einsendung des Artikels zugestellt.

Annahmeschluss: 09.09.2011