CHAPTER **FIVE**

Give Responsibility for Disruptive Technologies to Organizations Whose Customers Need Them

Most executives would like to believe that they're in charge of their organizations, that they make the crucial decisions and that when they decide that something should be done everyone snaps to and executes. This chapter expands on the view already introduced: that in practice, it is a company's *customers* who effectively control what it can and cannot do. As we have seen in the disk drive industry, companies were willing to bet enormous amounts on technologically risky projects when it was clear that their customers needed the resulting products.

But they were unable to muster the wherewithal to execute much simpler disruptive projects if existing, profitable customers didn't need the products.

This observation supports a somewhat controversial theory called *resource dependence*, propounded by a minority of management scholars, [1] which posits that companies' freedom of action is limited to satisfying the needs of those entities outside the firm (customers and investors, primarily) that give it the resources it needs to survive. Drawing heavily upon concepts from biological evolution, resource dependence theorists assert that organizations will survive and prosper only if their staffs and systems serve the needs of customers and investors by providing them with the products, services, and profit they require. Organizations that do not will ultimately die off, starved of the revenues they need to survive.

[2] Hence, through this survival-of-the-fittest mechanism, those firms that rise to prominence in their industries generally will be those whose people and processes are most keenly tuned to giving their customers what they want. The controversy with this theory arises when its proponents conclude that managers are *powerless* to change the courses of their firms against the dictates of their customers. Even if a manager has a bold vision to take her or his company in a very different direction, the power of the customer-focused people and processes in any company well-adapted to survival in its competitive environment will reject the manager's attempts to change direction. Therefore, because they

provide the resources upon which the firm is dependent, it is the customers, rather than the managers, who really determine what a firm will do. It is forces outside the organization, rather than the managers within it, that dictate the company's

course. Resource dependence theorists conclude that the real role of managers in companies whose people and systems are well-adapted to survival is, therefore, only a symbolic one.

For those of us who have managed companies, consulted for management, or taught future managers, this is a most disquieting thought. We are there to manage, to make a difference, to formulate and implement strategy, to accelerate growth and improve profits. Resource dependence violates our very reason for being. Nonetheless, the findings reported in this book provide rather stunning support for the theory of resource dependence—especially for the notion that the customer-focused resource allocation and decision-making processes of successful companies are far more powerful in directing investments than are executives' decisions.

Clearly, customers wield enormous power in directing a firm's investments. What, then, should managers do when faced with a disruptive technology that the company's customers explicitly do not want? One option is to convince everyone in the firm that the company should pursue it anyway, that it has long-term strategic importance despite rejection by the customers who pay the bills and despite lower profitability than the upmarket alternatives. The other option would be to create an independent organization and embed it among emerging customers that *do* need the technology. Which works best?

Managers who choose the first option essentially are picking a fight with a powerful tendency of organizational nature—that customers, not managers, essentially control the investment patterns of a company. By contrast, managers who choose the second option align themselves with this tendency, harnessing rather than fighting its power. The cases presented in this chapter provide strong evidence that the second option offers far higher probabilities of success than the first.

INNOVATION AND RESOURCE ALLOCATION

The mechanism through which customers control the investments of a firm is the resource allocation process—the process that determines which initiatives get staff and money and which don't. Resource allocation and innovation are two sides of the same coin: Only those new product development projects that do get adequate funding, staffing, and management attention have a chance to succeed; those that are starved of resources will languish. Hence, the patterns of innovation in a company will mirror quite closely the patterns in which resources are allocated.

Good resource allocation processes are designed to weed out proposals that customers don't want. When these decision-making processes work well, if customers don't want a product, it won't get funded; if they do want it, it will. This is how things *must* work in great companies. They *must* invest in things customers want—and the better they become at doing this, the more successful they will be.

As we saw in chapter 4, resource allocation is not simply a matter of top-down decision making followed by implementation. Typically, senior managers are asked to decide whether to fund a project only after many others at lower levels in the organization have already decided which types of project proposals they want to package and send on to senior management for approval and which they don't think are worth the effort. Senior managers typically see only a well-screened subset of the innovative ideas generated. [3]

And even after senior management has endorsed funding for a particular project, it is rarely a "done deal." Many crucial resource allocation decisions are made after project approval—indeed, after product launch—by mid-level managers who set priorities when multiple projects and products compete for the time of the same people, equipment, and vendors. As management scholar Chester Barnard has noted:

> From the point of view of the relative importance of specific decisions, those of executives properly call for first attention. But from the point of view of aggregate importance, it is not decisions of executives, but of *non-executive participants* in organizations which should enlist major interest. [Italics added.] [4]

So how do non-executive participants make *their* resource allocation decisions? They decide which projects they will propose to senior management and which they will give priority to, based upon their understanding of what types of customers and products are most profitable to the company. Tightly coupled with this is their view of how their sponsorship of different proposals will affect their own career trajectories within the company, a view that is formed heavily by their understanding of what customers want and what types of products the company needs to sell more of in order to be more profitable. Individuals’ career trajectories can soar when they sponsor highly profitable innovation programs. It is through these mechanisms of seeking corporate profit and personal success, therefore, that customers exert a profound influence on the process of resource allocation, and hence on the patterns of innovation, in most companies.

SUCCESS IN DISRUPTIVE DISK DRIVE TECHNOLOGY

It is possible to break out of this system of customer control, however. Three cases in the history of the disk drive industry demonstrate how managers can develop strong market positions in a disruptive technology. In two cases, managers harnessed, rather than fought, the forces of resource dependence: They spun out independent companies to commercialize the disruptive technology. In the third, the manager chose to fight these forces, and survived the project, exhausted.

Quantum and Plus Development

As we have seen, Quantum Corporation, a leading maker of 8-inch drives sold in the minicomputer market in the early 1980s, completely missed the advent of 5.25-inch drives: It introduced its first versions nearly four years after those drives first appeared in the market. As the 5.25-inch pioneers began to invade the minicomputer market from below, for all the reasons already described, Quantum's sales began to sag.

In 1984 several Quantum employees saw a potential market for a thin 3.5-inch drive plugged into an expansion slot in IBM XT-and AT-class desktop computers—drives that would be sold to personal computer users rather than the OEM minicomputer manufacturers that had accounted for all of Quantum's revenue. They determined to leave Quantum and start a new firm to commercialize their idea.

Rather than let them leave unencumbered, however, Quantum's executives financed and retained 80 percent ownership of this spinoff venture, called Plus Development Corporation, and set the company up in different facilities. It was a completely self-sufficient organization, with its own executive staff and all of the functional capabilities required in an independent company. Plus was extremely successful. It designed and marketed its drives but had them manufactured under contract by Matsushita Kotobuki Electronics (MKE) in Japan.

As sales of Quantum's line of 8-inch drives began to evaporate in the mid-1980s, they were offset by Plus's growing "Hardcard" revenues. By 1987, sales of Quantum's 8-and 5.25-inch products had largely disappeared. Quantum then purchased the remaining 20 percent of Plus, essentially closed down the old corporation, and installed Plus's executives in Quantum's most senior positions. They then reconfigured Plus's 3.5-inch products to appeal to OEM desktop computer makers, such as Apple, just as the capacity vector for 3.5-inch drives was invading the desktop market, as shown in the disk drive trajectory map in Figure 1.7. Quantum, thus reconstituted as a 3.5-inch drive maker, has aggressively adopted sustaining component technology innovations, moving upmarket toward engineering workstations, and has also successfully negotiated the sustaining architectural innovation into 2.5-inch drives. By 1994 the new Quantum had become the largest unit-volume producer of disk drives in the world. [5]

Control Data in Oklahoma

Control Data Corporation (CDC) effected the same self-reconstitution— once. CDC was the dominant manufacturer of 14-inch drives sold into the OEM market between 1965 and 1982; its market share fluctuated between 55 and 62 percent. When the 8-inch architecture emerged in the late 1970s, however, CDC missed it—by three years. The company never captured more than a fraction of the 8-inch market, and those 8-inch drives that it did sell were sold almost exclusively to defend its established customer base of mainframe computer manufacturers. The reason was resources and managerial emphasis: Engineers and marketers at the company's principal Minneapolis facility kept getting pulled off the 8-inch program to resolve problems in the launch of next-generation 14-inch products for CDC's mainstream customers.

CDC launched its first 5.25-inch model two years after Seagate's pioneering product appeared in 1980. This time, however, CDC located its 5.25-inch effort in Oklahoma City. This was done, according to one manager, "not to escape CDC's Minneapolis engineering culture, but to isolate the [5.25-inch product] group from the company's mainstream customers." Although it was late in the market and never regained its former dominant position, CDC's foray into 5.25-inch drives was profitable, and at times the firm commanded a 20 percent share of higher-capacity 5.25-inch drives.

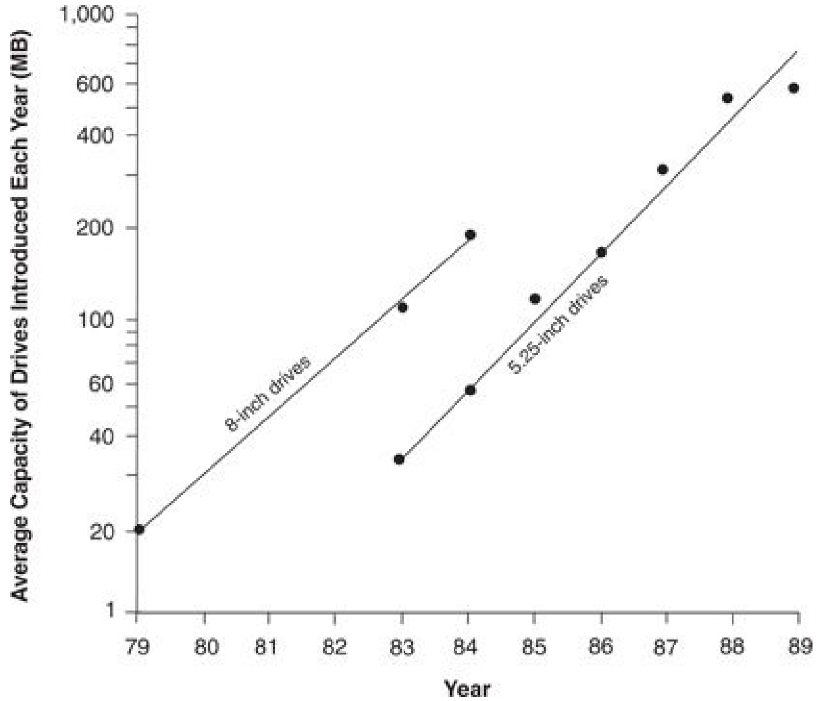
Micropolis: Transition by Managerial Force

Micropolis Corporation, an early disk drive leader founded in 1978 to make 8-inch drives, was the only other industry player to successfully make the transition to a disruptive platform. It did not use the spin-out strategy that had worked for Quantum and Control Data, however, choosing instead to manage the change from within the mainstream company. But even this exception supports the rule that customers exert exceptionally powerful influence over the investments that firms can undertake successfully.

Micropolis began to change in 1982, when founder and CEO Stuart Mabon intuitively perceived the trajectories of market demand and technology supply mapped in Figure 1.7 and decided that the firm should become primarily a maker of 5.25-inch drives. While initially hoping to keep adequate resources focused on developing its next generation of 8-inch drives so that Micropolis could straddle both markets, [6] he assigned the company's premier engineers to the 5.25-inch program. Mabon recalls that it took "100 percent of my time and energy for eighteen months" to keep adequate resources focused on the 5.25-inch program, because the organization's own mechanisms allocated resources to where the customers were—8-inch drives.

By 1984, Micropolis had failed to keep pace with competition in the minicomputer market for disk drives and withdrew its remaining 8-inch models. With Herculean effort, however, it did succeed in its 5.25-inch programs. Figure 5.1 shows why this struggle occurred: In making the transition, Micropolis assumed a position on a very different technological trajectory. It had to walk away from every one of its major customers and replace the lost revenues with sales of the new product line to an entirely different group of desktop computer makers. Mabon remembers the experience as the most exhausting of his life.

Figure 5.1 Technology Transition and Market Position at Micropolis Corporation



Source: Data are from various issues of *Disk/Trend Report.*

Micropolis finally introduced a 3.5-inch product in 1993. That was the point at which the product had progressed to pack more than 1 gigabyte in the 3.5-inch platform. At that level, Micropolis could sell the 3.5-inch drive to its existing customers.

DISRUPTIVE TECHNOLOGIES AND THE THEORY OF RESOURCE DEPENDENCE

The struggles recounted earlier of Seagate Technology's attempts to sell 3.5-inch drives and of Bucyrus Erie's failed attempt to sell its early Hydrohoe only to its mainstream customers illustrate how the theory of resource dependence can be applied to cases of disruptive technologies. In both instances, Seagate and Bucyrus were among the first in their industries to develop these disruptive products. But despite senior managers' decisions to introduce them, the impetus or organizational energy required to launch the products aggressively into the appropriate value networks simply did not coalesce—until customers needed them.

Should we then accept the corollary stipulated by resource-dependence theorists that managers are merely powerless individuals? Hardly. In the Introduction, exploring the image of how people learned to fly, I noted that all attempts had ended in failure as long as they consisted of fighting fundamental laws of nature. But once laws such as gravity, Bernoulli's principle, and the notions of lift, drag and resistance began to be understood, and flying machines were designed that accounted for or harnessed those laws, people flew quite successfully. By analogy, this is what Quantum and Control Data did. By embedding independent organizations within an entirely different value network, where they were dependent upon the appropriate set of customers for survival, those managers harnessed the powerful forces of resource dependence. The CEO of Micro-polis fought them, but he won a rare and costly victory.

Disruptive technologies have had deadly impact in many industries besides disk drives, mechanical excavators, and steel. [7] The following pages summarize the effect of disruptive technologies in three other industries— computers, retailing, and printers—to highlight how the only companies in those industries that established strong market positions in the disruptive technologies were those which, like Quantum and Control Data, harnessed rather than fought the forces of resource dependence.

DEC, IBM, AND THE PERSONAL COMPUTER

Quite naturally, the computer industry and the disk drive industry have parallel histories, because value networks of the latter are embedded in those of the former. In fact, if the axes and intersecting trajectories depicted on the disk drive trajectory map in Figure 1.7 were relabeled with computer-relevant terms, it would summarize equally well the failure of leading computer industry firms. IBM, the industry's first leader, sold its mainframe computers to the central accounting and data processing departments of large organizations. The emergence of the minicomputer represented a disruptive technology to IBM and its competitors. Their customers had no use for it; it promised lower, not higher, margins; and the market initially was significantly smaller. As a result, the makers of mainframes ignored the minicomputer for years, allowing a set of entrants—Digital Equipment, Data General, Prime, Wang, and Nixdorf—to create and dominate that market. IBM ultimately introduced its own line of minicomputers, but it did so primarily as a defensive measure, when the capabilities of minicomputers had advanced to the point that they were performance-competitive with the computing needs of some of IBM's customers.

Similarly, none of the makers of minicomputers became a significant factor in the desktop personal computer market, because to them the desktop computer was a disruptive technology. The PC market was created by another set of entrants, including Apple, Commodore, Tandy, and IBM. The minicomputer makers were exceptionally prosperous and highly regarded by investors, the business press, and students of good manage-ment—until the late 1980s, when the technological trajectory of the desktop computer intersected with the performance demanded by those who had previously bought minicomputers. The missile-like attack of the desktop computer from below severely wounded every minicomputer maker. Several of them failed. None established a viable position in the desktop personal computer value network.

A similar sequence of events characterized the emergence of the portable computer, where the market was created and dominated by a set of entrants like Toshiba, Sharp, and Zenith. Apple and IBM, the leading desktop makers, did not introduce portable models until the portables' performance trajectory intersected with the computing needs of their customers.

Probably none of these firms has been so deeply wounded by disruptive

Probably none of these firms has been so deeply wounded by disruptive technology as Digital Equipment. DEC fell from fortune to folly in just a few years, as stand-alone workstations and networked desktop computers obviated most customers' needs for minicomputers almost overnight.

DEC didn't stumble for lack of trying, of course. Four times between 1983 and 1995 it introduced lines of personal computers targeted at consumers, products that were technologically much simpler than DEC's minicomputers. But four times it failed to build businesses in this value network that were perceived within the company as profitable. Four times it withdrew from the personal computer market. Why? DEC launched all four forays from within the mainstream company. [8] For all of the reasons so far recounted, even though executive-level decisions lay behind the move into the PC business, those who made the day-to-day resource allocation decisions in the company never saw the sense in investing the necessary money, time, and energy in low-margin products that their customers didn't want. Higher-performance initiatives that promised up-scale margins, such as DEC's super-fast Alpha microprocessor and its adventure into mainframe computers, captured the resources instead.

In trying to enter the desktop personal computing business from within its mainstream organization, DEC was forced to straddle the two different cost structures intrinsic to two different value networks. It simply couldn't hack away enough overhead cost to be competitive in low-end personal computers because it needed those costs to remain competitive in its higher-performance products.

Yet IBM's success in the first five years of the personal computing industry stands in stark contrast to the failure of the other leading mainframe and minicomputer makers to catch the disruptive desktop computing wave. How did IBM do it? It created an autonomous organization in Florida, far away from its New York state headquarters, that was free to procure components from any source, to sell through its own channels, and to forge a cost structure appropriate to the technological and competitive requirements of the personal computing market. The organization was free to succeed along metrics of success that were relevant to the personal computing market. In fact, some have argued that IBM's subsequent decision to link its personal computer division much more closely to its mainstream organization was an important factor in IBM's difficulties in maintaining its profitability and market share in the personal computer industry. It seems to be very difficult to manage the peaceful, unambiguous coexistence of two cost structures, and two models for how to make money, within a single company.

The conclusion that a single organization might simply be incapable of competently pursuing disruptive technology, while remaining competitive in mainstream markets, bothers some “can-do” managers—and, in fact, most managers try to do exactly what Micropolis and DEC did: maintain their competitive intensity in the mainstream, while simultaneously trying to pursue disruptive technology. The evidence is strong that such efforts rarely succeed; position in one market will suffer unless two separate organizations, embedded within the appropriate value networks, pursue their separate customers.

KRESGE, WOOLWORTH, AND DISCOUNT RETAILING

In few industries has the impact of disruptive technology been felt so pervasively as in retailing, where discounters seized dominance from traditional department and variety stores. The technology of discount retailing was disruptive to traditional operations because the quality of service and selection offered by discounters played havoc with the accustomed metrics of quality retailing. Moreover, the cost structure required to compete profitably in discount retailing was fundamentally different than that which department stores had developed to compete within their value networks.

The first discount store was Korvette's, which began operating a number of outlets in New York in the mid-1950s. Korvette's and its imitators operated at the very low end of retailing's product line, selling nationally known brands of standard hard goods at 20 to 40 percent below department store prices. They focused on products that "sold themselves" because customers already knew how to use them. Relying on national brand image to establish the value and quality of their products, these discounters eliminated the need for knowledgeable salespeople; they also focused on the group of customers least attractive to mainstream retailers: "young wives of blue collar workers with young children." [9] This was counter to the upscale formulas department stores historically had used to define quality retailing and to improve profits.

Discounters didn't accept lower profits than those of traditional retailers, however; they just earned their profits through a different formula. In the simplest terms, retailers cover their costs through the gross margin, or markup, they charge over the cost of the merchandise they sell. Traditional department stores historically marked merchandise up by 40 percent and turned their inventory over four times in a year—that is, they earned 40 percent on the amount they invested in inventory, four times during the year, for a total return on inventory investment of 160 percent. Variety stores earned somewhat lower profits through a formula similar to that used by the department stores. Discount retailers earned a return on inventory investment similar to that of department stores, but through a different model: low gross margins and high inventory turns. Table 5.1 summarizes the three positions.

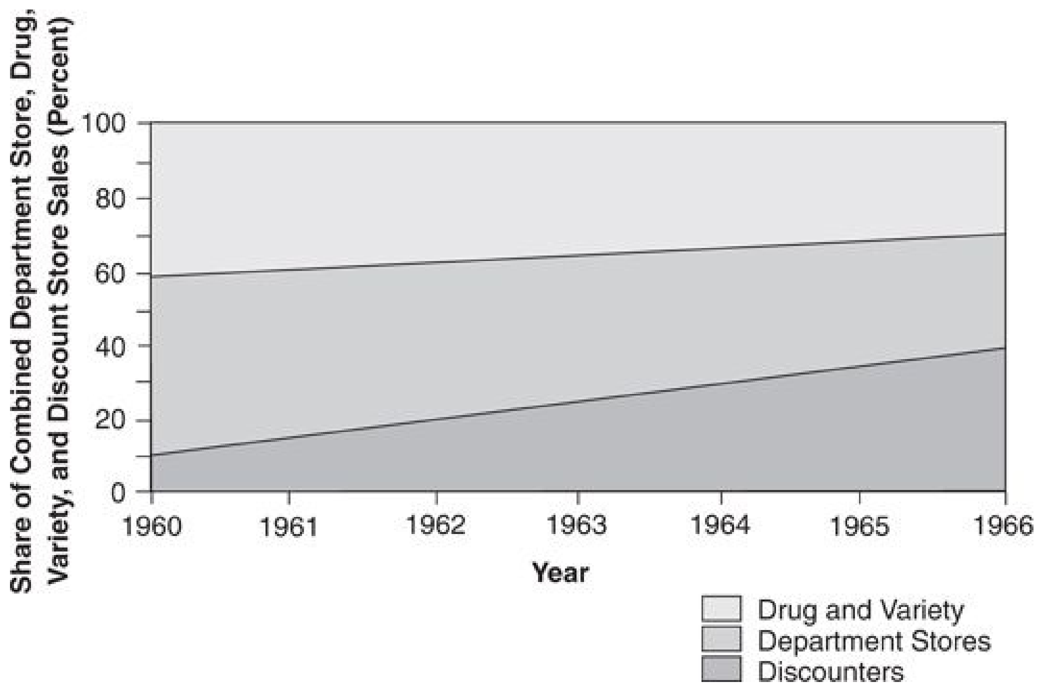
The history of discount retailing vividly recalls the history of minimill steel making. Just like the minimills, discounters took advantage of their cost structure to move upmarket and seize share from competing traditional retailers structure to move upmarket and seize share from competing traditional retailers at a stunning rate: first at the low end, in brand-name hard goods such as hardware, small appliances, and luggage, and later in territory further to the northeast such as home furnishings and clothing. Figure 5.2 illustrates how stunning the discounters' invasion was: Their share of retailing revenues in the categories of goods they sold rose from 10 percent in 1960 to nearly 40 percent a scant six years later.

Table 5.1 Different Pathways to Profits

Retailer Type	Company Example	Typical Gross Margins	Typical Inventory Turns	Return on Inventory Investment*
Department stores	R. H. Macy	40%	4x	160%
Variety stores	F. W. Woolworth	36%	4x	144%
Discount retailers	Kmart	20%	8x	160%

* Calculated as Margins x Turns, in other words, the total of the margins earned through successive turnovers each year. *Source:* Annual corporate reports of many companies in each category for various years.

Figure 5.2 Gains in Discount Retailers' Market Share, 1960–1966



Source: Data are from various issues of *Discount Merchandiser*.

Just as in disk drives and excavators, a few of the leading traditional retailers —notably S. S. Kresge, F. W. Woolworth, and Dayton Hudson— saw the disruptive approach coming and invested early. None of the other major retail chains, including Sears, Montgomery Ward, J. C. Penney, and R. H. Macy, made a significant attempt to create a business in discount retailing. Kresge (with its Kmart chain) and Dayton Hudson (with the Target chain) succeeded. [10] They both created focused discount retailing organizations that were independent from their traditional business. They recognized and harnessed the forces of resource dependence. By contrast, Woolworth failed in its venture (Woolco), trying to launch it from within the F. W. Woolworth variety store company. A detailed comparison of the approaches of Kresge and Woolworth, which started from very similar positions, lends additional insight into why establishing independent organizations to pursue disruptive technology seems to be a necessary condition for success.

S. S. Kresge, then the world's second largest variety store chain, began studying discount retailing in 1957, while discounting was still in its infancy. By 1961, both Kresge and its rival F. W. Woolworth (the world's largest variety store operator) had announced initiatives to enter discount retailing. Both firms opened stores in 1962, within three months of each other. The performance of the Woolco and Kmart ventures they launched, however, subsequently differed dramatically. A decade later, Kmart's sales approached $3.5 billion while Woolco's sales were languishing unprofitably at $0.9 billion. [11]

In making its commitment to discount retailing, Kresge decided to exit the variety store business entirely: In 1959 it hired a new CEO, Harry Cunningham, whose sole mission was to convert Kresge into a discounting powerhouse. Cunningham, in turn, brought in an entirely new management team, so that by 1961 there "was not a single operating vice president, regional manager, assistant regional manager, or regional merchandise manager who was not new on the job." [12] In 1961 Cunningham stopped opening any new variety stores, embarking instead on a program of closing about 10 percent of Kresge's existing variety operations each year. This represented a wholesale refocusing of the company on discount retailing.

Woolworth, on the other hand, attempted to support a program of sustaining improvements in technology, capacity, and facilities in its core variety store businesses while simultaneously investing in disruptive discounting. The managers charged with improving the performance of Woolworth's variety stores were also charged with building "the largest chain of discount houses in America." CEO Robert Kirkwood asserted that Woolco "would not conflict with the company's plans for growth and expansion in the regular variety store operations," and that no existing stores would be converted to a discount format. [13] Indeed, as discount retailing hit its most frenzied expansion phase in the 1960s, Woolworth was opening new variety stores at the pace it had set in the 1950s.

Unfortunately (but predictably), Woolworth proved unable to sustain within a single organization the two different cultures, and two different models of how to make a profit, that were required to be successful in variety and discount retailing. By 1967 it had dropped the term "discount" from all Woolco advertising, adopting the term "promotional department store" instead. Although initially Woolworth had set up a separate administrative staff for its Woolco operation, by 1971 more rational, cost-conscious heads had prevailed.

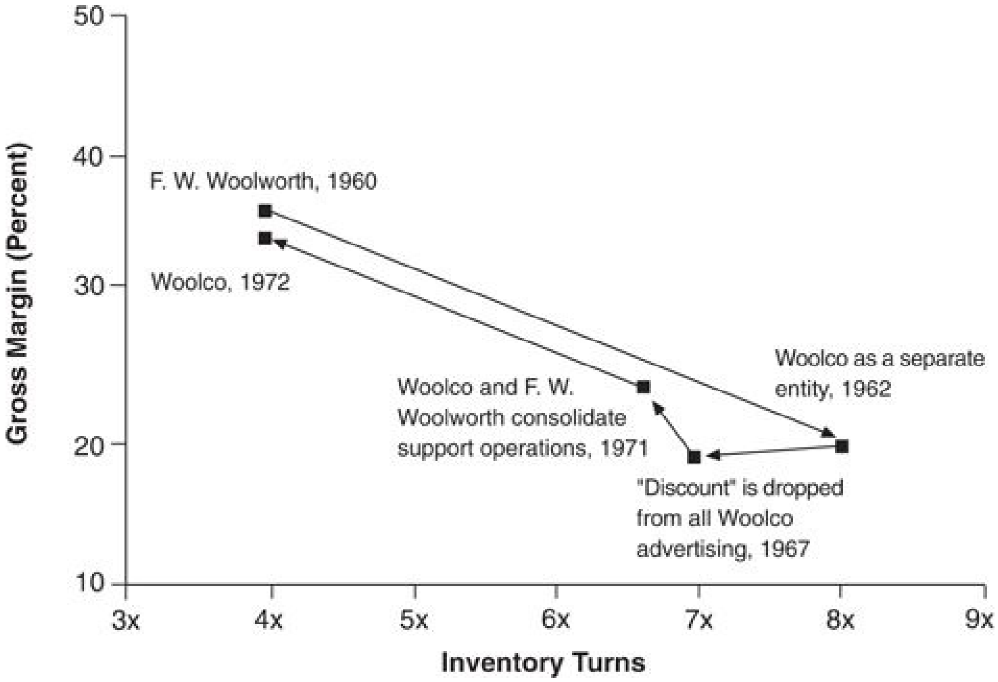
> In a move designed to increase sales per square foot in both Woolco and Woolworth divisions, the two subsidiaries have been consolidated operationally on a regional basis. Company officials say the consolidation

> —which involves buying offices, distribution facilities and management personnel at the regional level—will help both to develop better merchandise and more efficient stores. Woolco will gain the benefits of Woolworth's buying resources, distribution facilities and additional expertise in developing specialty departments. In return, Woolworth will gain Woolco's knowhow in locating, designing, promoting and operating large stores over 100,000 sq. ft. [14]

What was the impact of this cost-saving consolidation? It provided more evidence that two models for how to make money cannot peacefully coexist within a single organization. Within a year of this consolidation, Woolco had increased its markups such that its gross margins were the highest in the discount industry—about 33 percent. In the process, its inventory turns fell from the 7x it originally had achieved to 4x. The formula for profit that had long sustained F. W. Woolworth (35 percent margins for four inventory turns or 140 percent return on inventory investment) was ultimately demanded of Woolco as well. (See Figure 5.3.) Woolco was no longer a discounter—in name or in fact. Not surprisingly, Woolworth's venture into discount retailing failed: It closed its last Woolco store in 1982.

Woolworth's organizational strategy for succeeding in disruptive discount retailing was the same as Digital Equipment's strategy for launching its personal computer business. Both founded new ventures within the mainstream organization that had to earn money by mainstream rules, and neither could achieve the cost structure and profit model required to succeed in the mainstream value network.

Figure 5.3 Impact of the Integration of Woolco, and F. W. Woolworth on the Way



Source: Data are from various annual reports of F. W. Woolworth Company and from various issues of *Discount Merchandiser*.

SURVIVAL BY SUICIDE: HEWLETT-PACKARD'S LASER JET AND INK-JET PRINTERS

Hewlett-Packard's experience in the personal computer printer business illustrates how a company's pursuit of a disruptive technology by spinning out an independent organization might entail, in the end, killing another of its business units.

Hewlett-Packard's storied success in manufacturing printers for personal computers becomes even more remarkable when one considers its management of the emergence of bubble-jet or ink-jet technology. Beginning in the mid-1980s, HP began building a huge and successful business around laser jet printing technology. The laser jet was a discontinuous improvement over dot-matrix printing, the previously dominant personal computer printing technology, and HP built a commanding market lead.

When an alternative way of translating digital signals into images on paper (ink-jet technology) first appeared, there were vigorous debates about whether laser jet or ink jet would emerge as the dominant design in personal printing. Experts lined up on both sides of the question, offering HP extensive advice on which technology would ultimately become the printer of choice on the world's desktops. [15]

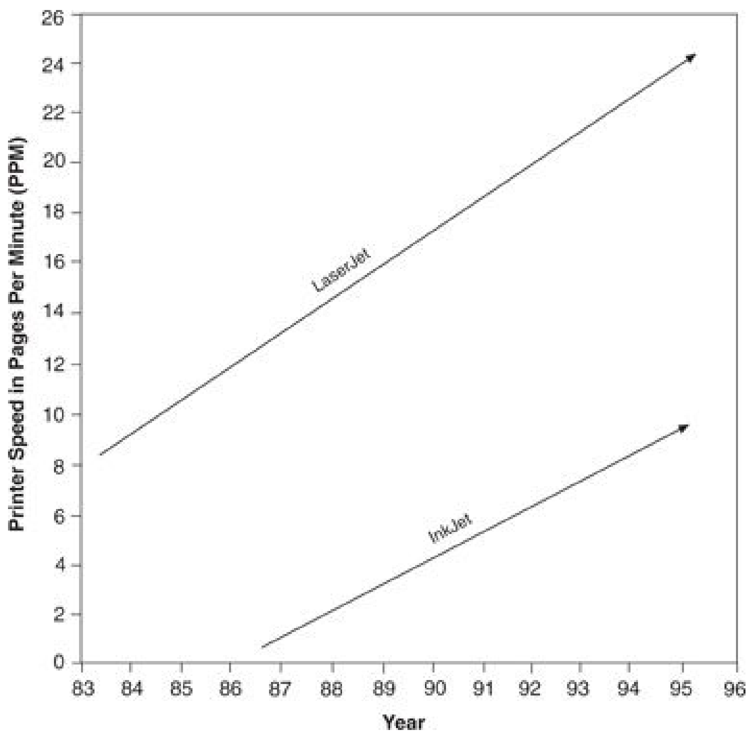
Although it was never framed as such in the debates of the time, inkjet printing was a disruptive technology. It was slower than the laser jet, its resolution was worse, and its cost per printed page was higher. But the printer itself was smaller and potentially much less expensive than the laser jet. At these lower prices, it promised lower gross margin dollars per unit than the laser jet. Thus, the ink-jet printer was a classic disruptive product, relative to the laser jet business.

Rather than place its bet exclusively with one or the other, and rather than attempt to commercialize the disruptive ink-jet from within the existing printer division in Boise, Idaho, HP created a completely autonomous organizational unit, located in Vancouver, Washington, with responsibility for making the ink-jet printer a success. It then let the two businesses compete against each other. Each has behaved classically. As shown in Figure 5.4, the laser jet division has moved sharply upmarket, in a strategy reminiscent of 14-inch drives, mainframe computers, and integrated steel mills. HP's laser jet printers can print at high speeds with exceptional resolution; handle hundreds of fonts and complicated graphics; print on two sides of the page; and serve multiple users on a network. They have also gotten larger physically.

The ink-jet printer isn't as good as the laser jet and may never be. But the critical question is whether the ink jet could ever be as good a printer as the personal desktop computing *market* demands. The answer appears to be yes. The resolution and speed of ink-jet printers, while still inferior to those of laser jets, are now clearly good enough for many students, professionals, and other un-networked users of desktop computers.

HP's ink-jet printer business is now capturing many of those who would formerly have been laser jet users. Ultimately, the number of users at the highest-performance end of the market, toward which the laser jet division is headed, will probably become small. One of HP's businesses may, in the end, have killed another. But had HP not set up its ink-jet business as a separate organization, the ink-jet technology would probably have languished within the mainstream laser jet business, leaving one of the other companies now actively competing in the ink-jet printer business, such as Canon, as a serious threat to HP's printer business. And by staying in the laser business, as well, HP has joined IBM's mainframe business and the integrated steel companies in making a *lot* of money while executing an upmarket retreat. [16]

Figure 5.4 Speed Improvement in InkJet and LaserJet Printers



Source: Hewlett-Packard product brochures, various years.

NOTES

1. The theory of resource dependence has been most thoroughly argued by Jeffrey Pfeffer and Gerald R. Salancik in *The External Control of Organizations: A Resource Dependence Perspective* (New York: Harper & Row, 1978).
2. This implies that, in managing business under both normal conditions and conditions of assault by a disruptive technology, the choice of which customers the firm will serve has enormous strategic consequences.
3. Joseph L. Bower, in *Managing the Resource Allocation Process* (Homewood, IL: Richard D. Irwin, 1972), presents an elegant and compelling picture of the resource allocation process.
4. Chester Barnard, *The Functions of the Executive* (Cambridge, MA: Harvard University Press, 1938), 190–191.
5. Quantum's spin-out of the Hardcard effort and its subsequent strategic reorientation is an example of the processes of strategy change described by Robert Burgelman, in "Intraorganizational Ecology of Strategy-Making and Organizational Adaptation: Theory and Field Research," *Organization Science* (2), 1991, 239–262, as essentially a process of natural selection through which suboptimal strategic initiatives lose out to optimal ones in the internal competition for corporate resources.
6. The failure of Micropolis to maintain simultaneous competitive commitments to both its established technology and the new 5.25-inch technology is consistent with the technological histories recounted by James Utterback, in *Mastering the Dynamics of Innovation* (Boston: Harvard Business School Press, 1994). Utterback found that firms that attempted to develop radically new technology almost always tried to maintain simultaneous commitment to the old and that they almost always failed.
7. A set of industries in which disruptive technologies are believed to have played a role in toppling leading firms is presented by Richard S. Rosenbloom and Clayton M. Christensen in "Technological Discontinuities, Organizational Capabilities, and Strategic Commitments," *Industrial and Corporate Change* (3), 1994, 655–685.
8. In the 1990s, DEC finally set up a Personal Computer Division in its attempt to build a significant personal computer business. It was not as autonomous

from DEC's mainstream business; however, the Quantum and Control Data spin-outs were. Although DEC set up specific performance metrics for the PC division, it was still held, *de facto*, to corporate standards for gross margins and revenue growth.

9. "Harvard Study on Discount Shoppers," *Discount Merchandiser*, September, 1963, 71.

0. When this book was being written, Kmart was a crippled company, having been beaten in a game of strategy and operational excellence by WalMart. Nonetheless, during the preceding two decades, Kmart had been a highly successful retailer, creating extraordinary value for Kresge shareholders. Kmart's present competitive struggles are unrelated to Kresge's strategy in meeting the original disruptive threat of discounting.

1. A detailed contrast between the Woolworth and Kresge approaches to discount retailing can be found in the Harvard Business School teaching case. "The Discount Retailing Revolution in America," No. 695-081.

2. See Robert Drew-Bear, "S. S. Kresge's Kmarts," *Mass Merchandising: Revolution and Evolution* (New York: Fairchild Publications, 1970), 218.

3. F. W. Woolworth Company Annual Report, 1981, p. 8.

4. "Woolco Gets Lion's Share of New Space," *Chain Store Age*, November, 1972, E27. This was an extraordinarily elegant, rational argument for the consolidation, clearly crafted by a corporate spin-doctor extraordinaire. Never mind that no Woolworth stores approached 100,000 square feet in size!

5. See, for example, "The Desktop Printer Industry in 1990," Harvard Business School, Case No. 9-390-173.

6. Business historian Richard Tedlow noted that the same dilemma had confronted A&P's executives as they deliberated whether to adopt the disruptive supermarket retailing format:

> The supermarket entrepreneurs competed against A&P not by doing better what A&P was the best company in the world at doing, but by doing something that A&P did not want to do at all. The greatest entrepreneurial failure in this story is Kroger. This company was second in the market, and one of its own employees (who left to found the world's first supermarket) knew how to make it first. Kroger executives did not listen. Perhaps it was lack of imagination or perhaps, like the executives at A&P, those at Kroger also had too much invested in the standard way of doing business. If the executives at A&P endorsed the supermarket revolution, they were ruining their own distribution system. That is why they sat by

paralyzed, unable to act until it was almost too late. In the end, A&P had little choice. The company could ruin its own system, or see others do it.

See Richard Tedlow, *New and Improved: The Story of Mass Marketing in America* (Boston: Harvard Business School Press, 1996).

CHAPTER **SIX**

Match the Size of the Organization to the Size of the Market

Managers who confront disruptive technological change must be leaders, not followers, in commercializing disruptive technologies.

Doing so requires implanting the projects that are to develop such technologies in commercial organizations that match in size the market they are to address. These assertions are based on two key findings of this study: that leadership is more crucial in coping with disruptive technologies than with sustaining ones, and that small, emerging markets cannot solve the near-term growth and profit requirements of large companies.

The evidence from the disk drive industry shows that creating new markets is significantly *less* risky and *more* rewarding than entering established markets against entrenched competition. But as companies become larger and more successful, it becomes even more difficult to enter emerging markets early enough. Because growing companies need to add increasingly large chunks of new revenue each year just to maintain their desired rate of growth, it becomes less and less possible that small markets can be viable as vehicles through which to find these chunks of revenue. As we shall see, the most straightforward way of confronting this difficulty is to implant projects aimed at commercializing disruptive technologies in organizations small enough to get excited about small-market opportunities, and to do so on a regular basis even while the mainstream company is growing.

ARE THE PIONEERS *REALLY* THE ONES WITH ARROWS IN THEIR BACKS?

A crucial strategic decision in the management of innovation is whether it is important to be a leader or acceptable to be a follower. Volumes have been written on first-mover advantages, and an offsetting amount on the wisdom of waiting until the innovation's major risks have been resolved by the pioneering firms. "You can always tell who the pioneers were," an old management adage goes. "They're the ones with the arrows in their backs." As with most disagreements in management theory, neither position is always right. Indeed, some findings from the study of the disk drive industry give some insight into when leadership is critical and when followership makes better sense.

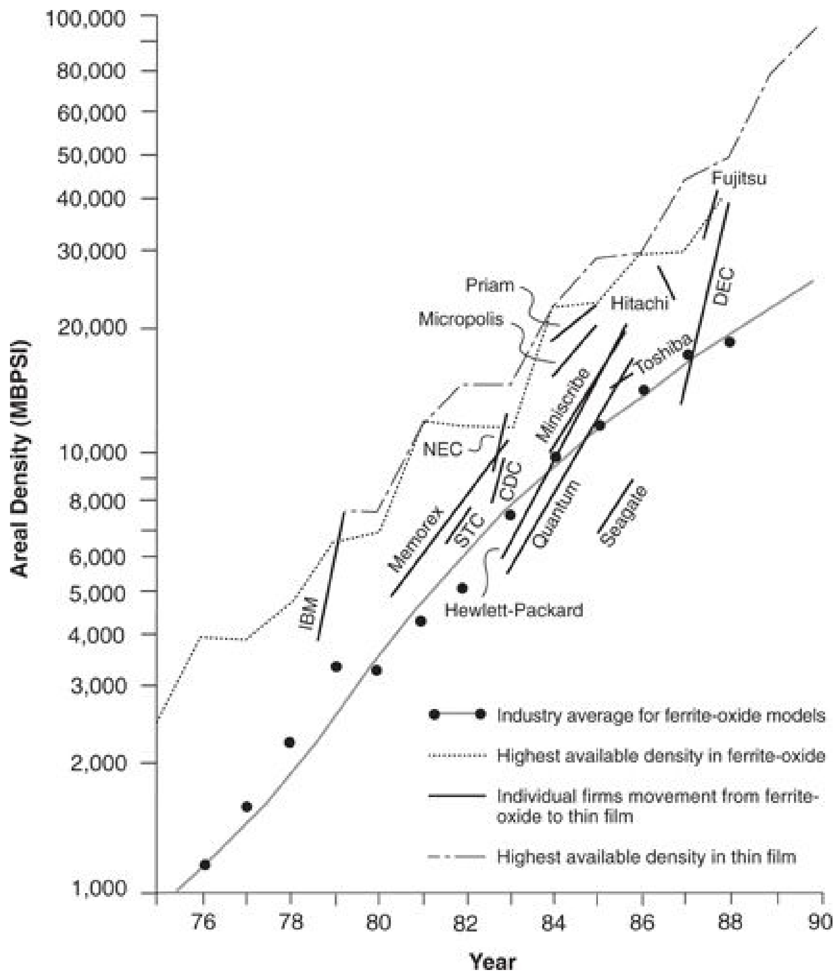
Leadership in Sustaining Technologies May Not Be Essential

One of the watershed technologies affecting the pace at which disk drive makers have increased the recording density of their drives was the thin-film read/write head. We saw in chapter 1 that despite the radically different, competence-destroying character of the technology, the $100 million and five-to-fifteen year expense of developing it, the firms that led in this technology were the leading, established disk drive manufacturers.

Because of the risk involved in the technology's development and its potential importance to the industry, the trade press began speculating in the late 1970s about which competitor would lead with thin-film heads. How far might conventional ferrite head technology be pushed? Would any drive makers get squeezed out of the industry race because they placed a late or wrong bet on the new head technology? Yet, it turned out, whether a firm led or followed in this innovation did *not* make a substantial difference in its competitive position. This is illustrated in Figures 6.1 and 6.2.

Figure 6.1 shows when each of the leading firms introduced its first model employing thin-film head technology. The vertical axis measures the recording density of the drive. The bottom end of the line for each firm denotes the maximum recording density it had achieved before it introduced a model with a thin-film head. The top end of each line indicates the density of the first model each company introduced with a thin-film head. Notice the wide disparity in the points at which the firms felt it was important to introduce the new technology. IBM led the industry, introducing its new head when it had achieved 3 megabits (Mb) per square inch. Memorex and Storage Technology similarly took a leadership posture with respect to this technology. At the other end, Fujitsu and Hitachi pushed the performance of conventional ferrite heads nearly ten times beyond the point where IBM first introduced the technology, choosing to be followers, rather than leaders, in thin-film technology.

Figure 6.1 Points at Which Thin-Film Technology Was Adopted by Leading Manufacturers, Relative to the Capabilities of Ferrite/Oxide Technology at the Time of the Switch



Source: Data are from various issues of *Disk/Trend Report.*

What benefit, if any, did leadership in this technology give to the pioneers? There is no evidence that the leaders gained any significant competitive advantage over the followers; none of the firms that pioneered thin-film technology gained significant market share on that account. In addition, pioneering firms appear not to have developed any sort of learning advantage enabling them to leverage their early lead to attain higher levels of density than did followers. Evidence of this is displayed in Figure 6.2. The horizontal axis shows the order in which the firms adopted thin-film heads. Hence, IBM was the first, Memorex, the second, and Fujitsu the fifteenth. The vertical axis gives the rank ordering of the recording density of the most advanced model marketed by each firm in 1989. If the early adopters of thin-film heads enjoyed some sort of experience-based advantage over the late adopters, then we would expect the points in the chart to slope generally from the upper left toward the lower right. The chart shows instead that there is no relationship between leadership and followership in thin-film heads and any subsequent technological edge. [1]

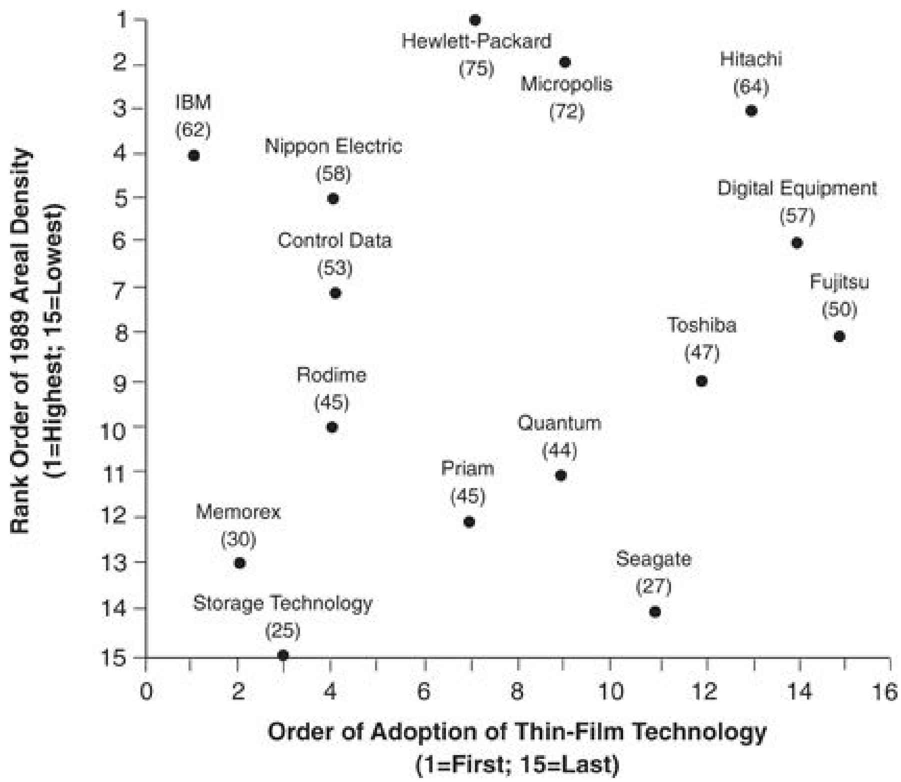
Each of the other sustaining technologies in the industry's history present a similar picture. There is no evidence that any of the leaders in developing and adopting sustaining technologies developed a discernible competitive advantage over the followers. [2]

Leadership in Disruptive Technologies Creates Enormous Value

In contrast to the evidence that leadership in sustaining technologies has historically conferred little advantage on the pioneering disk drive firms, there is strong evidence that leadership in disruptive technology has been *very* important. The companies that entered the new value networks enabled by disruptive generations of disk drives within the first two years after those drives appeared were six times more likely to succeed than those that entered later.

Eighty-three companies entered the U.S. disk drive industry between 1976 and 1993. Thirty-five of these were diversified concerns, such as Memorex, Ampex, 3M, and Xerox, that made other computer peripheral equipment or other magnetic recording products. Forty-eight were independent startup companies, many being financed by venture capital and headed by people who previously had worked for other firms in the industry. These numbers represent the complete census of all firms that ever were incorporated and/or were known to have announced the design of a hard drive, whether or not they actually sold any. It is not a statistical sample of firms that might be biased in favor or against any type of firm.

Figure 6.2 Relationship between Order of Adoption of Thin-Film Technology and Areal Density of Highest-Performance 1989 Model



Source: Clayton M. Christensen, "Exploring the Limits of the Technology S-Curve. Part I: Component Technologies," *Production and Operations Management* 1, no. 4 (Fall 1992): 347. Reprinted by permission.

The entry strategies employed by each of these firms can be characterized along the two axes in Table 6.1. The vertical axis describes *technology* strategies, with firms at the bottom using only proven technologies in their initial products and those at the top using one or more new component technologies. [3] The horizontal axis charts *market* strategies, with firms at the left having entered already established value networks and those at the right having entered emerging value networks. [4] Another way to characterize this matrix is to note that companies that were agressive at entry in developing and adopting sustaining innovations appear in the two top boxes, left and right, while companies that led at entry in creating new value networks appear in the two right-hand boxes, top and bottom. The companies in the right boxes include *all* companies that attempted to create new value networks, even those networks that did not materialize into substantial markets (such as removable hard drives).

Table 6.1 Disk Drive Companies Achieving $100 Million in Annual Revenues in at Least One Year Between 1976 and 1994

Technology Strategy at Entry — New Technology

Type of Firm	S	F	N	T	% Success	Sales ($millions)	Type of Firm	S	F	N	T	% Success	Sales ($millions)
Start-ups	0	7	3	10	0%	$235.3	Start-ups	3	4	1	8	37%	$16,379.3
Related-Technology	0	1	0	1	0%	0.0	Related-Technology	0	0	0	0	—	—
Related-Market	0	3	0	3	0%	1.4	Related Market	0	0	0	0	—	—
Forward Integrators	0	1	0	1	0%	0.0	Forward Integrators	0	0	0	0	—	—
Total	0	12	3	15	0%	$236.7	Total	3	4	1	8	37%	$16,379.3

Technology Strategy at Entry — Proven Technology

Type of Firm	S	F	N	T	% Success	Sales ($millions)	Type of Firm	S	F	N	T	% Success	Sales ($millions)
Start-ups	3	11	4	18	17%	$2,485.7	Start-ups	4	7	2	13	31%	$32,043.7
Related-Technology	0	4	0	4	0%	191.6	Related-Technology	4	2	0	6	67%	11,461.0
Related-Market	0	12	0	12	0%	361.2	Related-Market	1	4	0	5	20%	2,239.0
Forward Integrators	0	3	0	3	0%	17.7	Forward Integrators	0	0	0	0	—	—
Total	3	30	4	37	8%	$3,056.2	Total	9	13	2	24	36%	$45,743.7

Established Market — Emerging Market

Market Strategy at Entry

Statistics for all companies, regardless of technology strategy:

	S	F	N	T	% Success	Sales ($millions)		S	F	N	T	% Success	Sales ($millions)
Start-ups	3	18	7	28	11%	$2,721.0	Start-ups	7	11	3	21	33%	$48,423.0
Related-Technology	0	5	0	5	0%	191.6	Related-Technology	4	2	0	6	67%	11,461.0
Related-Market	0	15	0	15	0%	362.6	Related-Market	1	4	0	5	20%	2,239.0
Forward Integrators	0	4	0	4	0%	17.7	Forward Integrators	0	0	0	0	—	—
Total	3	42	7	52	6%	$3,292.9	Total	12	17	3	32	37%	$62,123.0

Source: Data are from various issues of *Disk/Trend Report.*
Note: S indicates success, F indicates failure, N indicates no, T indicates total.

Each quadrant displays the number of companies that entered using the strategy represented. Under the *S* (for “success”) are the number of firms that successfully generated $100 million in revenues in at least one year, even if the firm subsequently failed; *F* (for “failure”) shows the number of firms that failed ever to reach the $100 million revenue threshold and that have subsequently exited the industry; *N* (for “no”) indicates the number of firms for which there is as yet no verdict because, while still operating in 1994, they had not yet reached $100 million in sales; and *T* (for “total”) lists the total number of firms that entered in each category. [5] The column labeled “% Success” indicates the percentage of the total number of firms that reached $100 million in sales. Finally, beneath the matrix are the sums of the data in the two quadrants above.

The numbers beneath the matrix show that only three of the fifty-one firms (6 percent) that entered established markets ever reached the $100 million revenue benchmark. In contrast, 37 percent of the firms that led in disruptive technological innovation—those entering markets that were less than two years old—surpassed the $100 million level, as shown on the right side of Table 6.1. Whether a firm was a startup or a diversified firm had little impact on its success rate. What mattered appears not to have been its organizational form, but whether it was a leader in introducing disruptive products and creating the markets in which they were sold. [6]

Only 13 percent of the firms that entered attempting to lead in sustaining component technologies (the top half of the matrix) succeeded, while 20 percent of the firms that followed were successful. Clearly, the lower-right quadrant offered the most fertile ground for success.

The cumulative sales numbers in the right-most columns in each quadrant show the total, cumulative revenues logged by all firms pursuing each of the strategies; these are summarized below the matrix. The result is quite stunning. The firms that led in launching disruptive products together logged a cumulative total of $62 billion dollars in revenues between 1976 and 1994. [7] Those that followed into the markets later, after those markets had become established, logged only $3.3 billion in total revenue. It is, indeed, an innovator’s dilemma. Firms that sought growth by entering small, emerging markets logged *twenty times* the revenues of the firms pursuing growth in larger markets. The difference in revenues per firm is even more striking: The firms that followed late into the markets enabled by disruptive technology, on the left half of the matrix, generated an average cumulative total of $64.5 million per firm. The *average* company that led in disruptive technology generated $1.9 *billion* in revenues. The firms on the left side seem to have made a sour bargain. They exchanged a *market risk*, the risk that an emerging market for the disruptive technology might not develop after all, for a *competitive risk*, the risk of entering markets against entrenched competition. [8]

COMPANY SIZE AND LEADERSHIP IN DISRUPTIVE TECHNOLOGIES

Despite evidence that leadership in disruptive innovation pays such huge dividends, established firms, as shown in the first four chapters of this book, often fail to take the lead. Customers of established firms can hold the organizations captive, working through rational, well-functioning resource allocation processes to keep them from commercializing disruptive technologies. One cruel *additional* disabling factor that afflicts established firms as they work to maintain their growth rate is that the larger and more successful they become, the more difficult it is to muster the rationale for entering an emerging market in its early stages, when the evidence above shows that entry is so crucial.

Good managers are driven to keep their organizations growing for many reasons. One is that growth rates have a strong effect on share prices. To the extent that a company's stock price represents the discounted present value of some consensus forecast of its future earnings stream, then the *level* of the stock price—whether it goes up or down—is driven by changes in the projected *rate of growth* in earnings. [9] In other words, if a company's current share price is predicated on a consensus growth forecast of 20 percent, and the market's consensus for growth is subsequently revised downward to 15 percent growth, then the company's share price will likely *fall*—even though its revenues and earnings will still be growing at a healthy rate. A strong and increasing stock price, of course, gives a company access to capital on favorable terms; happy investors are a great asset to a company.

Rising share prices make stock option plans an inexpensive way to provide incentive to and to reward valuable employees. When share prices stagnate or fall, options lose their value. In addition, company growth creates room at the top for high-performing employees to expand the scope of their responsibilities. When companies stop growing, they begin losing many of their most promising future leaders, who see less opportunity for advancement.

Finally, there is substantial evidence that growing companies find it much easier to justify investments in new product and process technologies than do companies whose growth has stopped. [10]

Unfortunately, companies that become large and successful find that maintaining growth becomes progressively more difficult. The math is simple: A

$40 million company that needs to grow profitably at 20 percent to sustain its stock price and organizational vitality needs an additional $8 million in revenues the first year, $9.6 million the following year, and so on; a $400 million company with a 20 percent targeted growth rate needs new business worth $80 million in the first year, $96 million in the next, and so on; and a $4 billion company with a 20 percent goal needs to find $800 million, $960 million, and so on, in each successive year.

This problem is particularly vexing for big companies confronting disruptive technologies. Disruptive technologies facilitate the emergence of new markets, and there are no $800 million emerging markets. But it is precisely when emerging markets are small—when they are *least* attractive to large companies in search of big chunks of new revenue—that entry into them is so critical.

How can a manager of a large, successful company deal with these realities of size and growth when confronted by disruptive change? I have observed three approaches in my study of this problem:

1. Try to affect the growth rate of the emerging market, so that it becomes big enough, fast enough, to make a meaningful dent on the trajectory of profit and revenue growth of a large company.
2. Wait until the market has emerged and become better defined, and then enter after it “has become large enough to be interesting.”
3. Place responsibility to commercialize disruptive technologies in organizations small enough that their performance will be meaningfully affected by the revenues, profits, and small orders flowing from the disruptive business in its earliest years.

As the following case studies show, the first two approaches are fraught with problems. The third has its share of drawbacks too, but offers more evidence of promise.

CASE STUDY: PUSHING THE GROWTH RATE OF AN EMERGING MARKET

The history of Apple Computer's early entry into the handheld computer, or personal digital assistant (PDA), market helps to clarify the difficulties confronting large companies in small markets.

Apple Computer introduced its Apple I in 1976. It was at best a preliminary product with limited functionality, and the company sold a total of 200 units at $666 each before withdrawing it from the market. But the Apple I wasn't a financial disaster. Apple had spent modestly on its development, and both Apple and its customers learned a lot about how desktop personal computers might be used. Apple incorporated this learning into its Apple II computer, introduced in 1977, which was highly successful. Apple sold 43,000 Apple II computers in the first two years they were on the market, [11] and the product's success positioned the company as the leader in the personal computer industry. On the basis of the Apple II's success Apple went public in 1980.

A decade after the release of the Apple II, Apple Computer had grown into a $5 billion company, and like all large and successful companies, it found itself having to add large chunks of revenue each year to preserve its equity value and organizational vitality. In the early 1990s, the emerging market for handheld PDAs presented itself as a potential vehicle for achieving that needed growth. In many ways, this opportunity, analogous to that in 1978 when the Apple II computer helped shape its industry, was a great fit for Apple. Apple's distinctive design expertise was in user-friendly products, and user-friendliness and convenience were the basis of the PDA concept.

How did Apple approach this opportunity? Aggressively. It invested scores of millions of dollars to develop its product, dubbed the "Newton." The Newton's features were defined through one of the most thoroughly executed market research efforts in corporate history; focus groups and surveys of every type were used to determine what features consumers would want. The PDA had many of the characteristics of a disruptive computing technology, and recognizing the potential problems, Apple CEO John Sculley made the Newton's development a personal priority, promoting the product widely, and ensuring that the effort got the technical and financial resources it needed.

Apple sold 140,000 Newtons in 1993 and 1994, its first two years on the market. Most observers, of course, viewed the Newton as a big flop. Technically, its handwriting recognition capabilities were disappointing, and its wireless communications technologies had made it expensive. But what was most damning was that while Sculley had publicly positioned the Newton as a key product to sustain the company's growth, its first-year sales amounted to about 1 percent of Apple's revenues. Despite all the effort, the Newton made hardly a dent in Apple's need for new growth.

But was the Newton a failure? The timing of Newton's entry into the handheld market was akin to the timing of the Apple II into the desktop market. It was a market-creating, disruptive product targeted at an undefinable set of users whose needs were unknown to either themselves or Apple. On that basis, Newton's sales should have been a pleasant surprise to Apple's executives: It outsold the Apple II in its first two years by a factor of more than three to one. But while selling 43,000 units was viewed as an IPO-qualifying triumph in the smaller Apple of 1979, selling 140,000 Newtons was viewed as a failure in the giant Apple of 1994.

As chapter 7 will show, disruptive technologies often enable something to be done that previously had been deemed impossible. Because of this, when they initially emerge, neither manufacturers nor customers know how or why the products will be used, and hence do not know what specific features of the product will and will not ultimately be valued. Building such markets entails a process of mutual discovery by customers and manufacturers—and this simply takes time. In Apple's development of the desktop computer, for example, the Apple I failed, the first Apple II was lackluster, and the Apple II[H11501] succeeded. The Apple III was a market failure because of quality problems, and the Lisa was a failure. The first two generations of the Macintosh computer also stumbled. It wasn't until the third iteration of the Macintosh that Apple and its customers finally found "it": the standard for convenient, user-friendly computing to which the rest of the industry ultimately had to conform. [12]

In launching the Newton, however, Apple was desperate to short-circuit this coalescent process for defining the ultimate product and market. It assumed that its customers knew what they wanted and spent very aggressively to find out what this was. (As the next chapter will show, this is impossible.) Then to give customers what they thought they wanted, Apple had to assume the precarious role of a sustaining technology leader in an emerging industry. It spent enormous sums to push mobile data communications and handwriting recognition technologies beyond the state of the art. And finally, it spent aggressively to convince people to buy what it had designed.

Because emerging markets are small by definition, the organizations competing in them must be able to become profitable at small scale. This is crucial because organizations or projects that are perceived as being profitable and successful can continue to attract financial and human resources both from their corporate parents and from capital markets. Initiatives perceived as failures have a difficult time attracting either. Unfortunately, the scale of the investments Apple made in its Newton in order to hasten the emergence of the PDA market made it very difficult to earn an attractive return. Hence, the Newton came to be broadly viewed as a flop.

As with most business disappointments, hindsight reveals the faults in Apple's Newton project. But I believe that the root cause of Apple's struggle was *not* inappropriate management. The executives' actions were a symptom of a deeper problem: Small markets cannot satisfy the near-term growth requirements of big organizations.

CASE STUDY: WAITING UNTIL A MARKET IS LARGE ENOUGH TO BE INTERESTING

A second way that many large companies have responded to the disruptive technology trap is to wait for emerging markets to “get large enough to be interesting” before they enter. Sometimes this works, as IBM’s well-timed 1981 entry into the desktop PC business demonstrated. But it is a seductive logic that can backfire, because the firms creating new markets often forge capabilities that are closely attuned to the requirements of those markets and that later entrants find difficult to replicate. Two examples from the disk drive industry illustrate this problem.

Priam Corporation, which ascended to leadership of the market for 8-inch drives sold to minicomputer makers after its entry in 1978, had built the capability in that market to develop its drives on a two-year rhythm. This pace of new product introduction was consistent with the rhythm by which its customers, minicomputer makers, introduced their new products into the market.

Seagate’s first 5.25-inch drive, introduced to the emerging desktop market in 1980, was disruptively slow compared to the performance of Priam’s drives in the minicomputer market. But by 1983, Seagate and the other firms that led in implementing the disruptive 5.25-inch technology had developed a *one-year* product introduction rhythm in their market. Because Seagate and Priam achieved similar percentage improvements in speed with each new product generation, Seagate, by introducing new generations on a one-year rhythm, quickly began to converge on Priam’s performance advantage.

Priam introduced its first 5.25-inch drive in 1982. But the rhythm by which it introduced its subsequent 5.25-inch models was the two-year capability it had honed in the minicomputer market—not the one-year cycle required to compete in the desktop marketplace. As a consequence, it was never able to secure a *single* major OEM order from a desktop computer manufacturer: It just couldn’t hit their design windows with its new products. And Seagate, by taking many more steps forward than did Priam, was able to close the performance gap between them. Priam closed its doors in 1990.

The second example occurred in the next disruptive generation. Seagate Technology was the second in the industry to develop a 3.5-inch drive in 1984. Analysts at one point had speculated that Seagate might ship 3.5-inch drives as early as 1985; and indeed, Seagate showed a 10 MB model at the fall 1985 Comdex Show. When Seagate still had not shipped a 3.5-inch drive by late 1986, CEO Al Shugart explained, “So far, there just isn’t a big enough market for it, as yet.” [13] In 1987, when the 3.5-inch market at $1.6 billion had gotten “big enough to be interesting,” Seagate finally launched its offering. By 1991, however, even though Seagate had by then built substantial volume in 3.5-inch drives, it had not yet succeeded in selling a single drive to a maker of portable computers: Its models were all sold into the desktop market, defensively cannibalizing its sales of 5.25-inch drives. Why?

One likely reason for this phenomenon is that Conner Peripherals, which pioneered and maintained the lead in selling 3.5-inch drives to portable computer makers, fundamentally changed the way drive makers had to approach the portables market. As one Conner executive described it,

> From the beginning of the OEM disk drive industry, product development had proceeded in three sequential steps. First you designed the drive; then you made it; and then you sold it. We changed all that. We first *sell* the drives; then we design them; and then we build them. [14]

In other words, Conner set a pattern whereby drives for the portable computer market were custom-designed for major customers. And it refined a set of capabilities in its marketing, engineering, and manufacturing processes that were tailored to that pattern. [15] Said another Conner executive, “Seagate was never able to figure out how to sell drives in the portable market. They just never got it.” [16]

CASE STUDY: GIVING SMALL OPPORTUNITIES TO SMALL ORGANIZATIONS

Every innovation is difficult. That difficulty is compounded immeasurably, however, when a project is embedded in an organization in which most people are continually questioning why the project is being done at all. Projects make sense to people if they address the needs of important customers, if they positively impact the organization’s needs for profit and growth, and if participating in the project enhances the career opportunities of talented employees. When a project doesn’t have these characteristics, its manager spends much time and energy justifying why it merits resources and cannot manage the project as effectively. Frequently in such circumstances, the best people do not want to be associated with the project—and when things get tight, projects viewed as nonessential are the first to be canceled or postponed.

Executives can give an enormous boost to a project’s probability of success, therefore, when they ensure that it is being executed in an environment in which everyone involved views the endeavor as crucial to the organization’s future growth and profitability. Under these conditions, when the inevitable disappointments, unforeseen problems, and schedule slippages occur, the organization will be more likely to find ways to muster whatever is required to solve the problem.

As we have seen, a project to commercialize a disruptive technology in a small, emerging market is very unlikely to be considered essential to success in a large company; small markets don’t solve the growth problems of big companies. Rather than continually working to convince and remind everyone that the small, disruptive technology might *someday* be significant or that it is at least strategically important, large companies should seek to embed the project in an organization that is small enough to be motivated by the opportunity offered by a disruptive technology in its early years. This can be done either by spinning out an independent organization or by acquiring an appropriately small company. Expecting achievement-driven employees in a large organization to devote a critical mass of resources, attention, and energy to a disruptive project targeted at a small and poorly defined market is equivalent to flapping one’s arms in an effort to fly: It denies an important tendency in the way organizations work. [17]

There are many success stories to the credit of this approach. Control Data, for example, which had essentially missed the 8-inch disk drive generation, sent a group to Oklahoma City to commercialize its 5.25-inch drive. In addition to CDC's need to escape the power of its mainstream customers, the firm explicitly wanted to create an organization whose size matched the opportunity. "We needed an organization," reflected one manager, "that could get excited about a $50,000 order. In Minneapolis [which derived nearly $1 billion from the sale of 14-inch drives in the mainframe market] you needed a million-dollar order just to turn anyone's head." CDC's Oklahoma City venture proved to be a significant success.

Another way of matching the size of an organization to the size of the opportunity is to acquire a small company within which to incubate the disruptive technology. This is how Allen Bradley negotiated its very successful disruptive transition from mechanical to electronic motor controls.

For decades the Allen Bradley Company (AB) in Milwaukee has been the undisputed leader in the motor controls industry, making heavy-duty, sophisticated switches that turn large electric motors off and on and protect them from overloads and surges in current. AB's customers were makers of machine tools and cranes as well as contractors who installed fans and pumps for industrial and commercial heating, ventilating, and air conditioning (HVAC) systems. Motor controls were electromechanical devices that operated on the same principle as residential light switches, although on a larger scale. In sophisticated machine tools and HVAC systems, electric motors and their controls were often linked, through systems of electromechanical relay switches, to turn on and off in particular sequences and under particular conditions. Because of the value of the equipment they controlled and the high cost of equipment downtime, controls were required to be rugged, capable of turning on and off millions of times and of withstanding the vibrations and dirt that characterized the environments in which they were used.

In 1968, a startup company, Modicon, began selling electronic programmable motor controls—a disruptive technology from the point of view of mainstream users of electromechanical controls. Texas Instruments (TI) entered the fray shortly thereafter with its own electronic controller. Because early electronic controllers lacked the real and perceived ruggedness and robustness for harsh environments of the hefty AB-type controllers, Modicon and TI were unable to sell their products to mainstream machine tool makers and HVAC contractors. As performance was measured in the mainstream markets, electronic products underperformed conventional controllers, and few mainstream customers needed the programmable flexibility offered by electronic controllers.

As a consequence, Modicon and TI were forced to cultivate an emerging market for programmable controllers: the market for factory automation. Customers in this emerging market were not equipment manufacturers, but equipment *users*, such as Ford and General Motors, who were just beginning their attempt to integrate pieces of automatic manufacturing equipment.

Of the five leading manufacturers of electromechanical motor controls—Allen Bradley, Square D, Cutler Hammer, General Electric, and Westinghouse—only Allen Bradley retained a strong market position as programmable electronic controls improved in ruggedness and began to invade the core motor control markets. Allen Bradley entered the electronic controller market just two years after Modicon and built a market-leading position in the new technology within a few years, even as it kept its strength in its old electromechanical products. It subsequently transformed itself into a major supplier of electronic controllers for factory automation. The other four companies, by contrast, introduced electronic controllers much later and subsequently either exited the controller business or were reduced to weak positions. From a capabilities perspective this is a surprising outcome, because General Electric and Westinghouse had much deeper expertise in microelectronics technologies at that time than did Allen Bradley, which had no institutional experience in the technology.

What did Allen Bradley do differently? In 1969, just one year after Modicon entered the market, AB executives bought a 25 percent interest in Information Instruments, Inc., a fledgling programmable controller startup based in Ann Arbor, Michigan. The following year it purchased outright a nascent division of Bunker Ramo, which was focused on programmable electronic controls and their emerging markets. AB combined these acquisitions into a single unit and maintained it as a business separate from its mainstream electromechanical products operation in Milwaukee. Over time, the electronics products have significantly eaten into the electromechanical controller business, as one AB division attacked the other. [18] By contrast, each of the other four companies tried to manage its electronic controller businesses from within its mainstream electromechanical divisions, whose customers did not initially need or want electronic controls. Each failed to develop a viable position in the new technology.

Johnson & Johnson has with great success followed a strategy similar to Allen Bradley's in dealing with disruptive technologies such as endoscopic surgical equipment and disposable contact lenses. Though its total revenues amount to more than $20 billion, J&J comprises 160 autonomously operating companies, which range from its huge MacNeil and Janssen pharmaceuticals companies to small companies with annual revenues of less than $20 million. Johnson & Johnson’s strategy is to launch products of disruptive technologies through very small companies acquired for that purpose.

SUMMARY

It is not crucial for managers pursuing growth and competitive advantage to be leaders in every element of their business. In sustaining technologies, in fact, evidence strongly suggests that companies which focus on extending the performance of conventional technologies, and choose to be followers in adopting new ones, can remain strong and competitive. This is not the case with disruptive technologies, however. There are enormous returns and significant first-mover advantages associated with early entry into the emerging markets in which disruptive technologies are initially used. Disk drive manufacturers that led in commercializing disruptive technology grew at vastly greater rates than did companies that were disruptive technology followers.

Despite the evidence that leadership in commercializing disruptive technologies is crucial, large, successful innovators encounter a significant dilemma in the pursuit of such leadership. In addition to dealing with the power of present customers as discussed in the last chapter, large, growth-oriented companies face the problem that small markets don’t solve the near-term growth needs of large companies. The markets whose emergence is enabled by disruptive technologies all began as small ones. The first orders that the pioneering companies received in those markets were small ones. And the companies that cultivated those markets had to develop cost structures enabling them to become profitable at small scale. Each of these factors argues for a policy of implanting projects to commercialize disruptive innovations in small organizations that will view the projects as being on their critical path to growth and success, rather than as being distractions from the main business of the company.

This recommendation is not new, of course; a host of other management scholars have also argued that smallness and independence confer certain advantages in innovation. It is my hope that chapters 5 and 6 provide deeper insight about why and under what circumstances this strategy is appropriate.

NOTES

1. The benefits of persistently pursuing incremental improvements versus taking big strategic leaps have been capably argued by Robert Hayes in "Strategic Planning: Forward in Reverse?" *Harvard Business Review*, November–December, 1985, 190–197.

I believe that there are some specific situations in which leadership in sustaining technology is crucial, however. In a private conversation, Professor Kim Clark characterized these situations as those affecting *knife-edge* businesses, that is, businesses in which the basis of competition is simple and unidimensional and there is little room for error. An example of such a knife-edge industry is the photolithographic aligner (PLA) industry, studied by Rebecca M. Henderson and Kim B. Clark, in "Architectural Innovation: The Reconfiguration of Existing Systems and the Failure of Established Firms," *Administrative Science Quarterly* (35), March, 1990, 9–30. In this case, aligner manufacturers failed when they fell behind technologically in the face of sustaining architectural changes. This is because the basis of competition in the PLA industry was quite straightforward even though the products themselves were very complex: products either made the narrowest line width on silicon wafers of any in the industry or no one bought them. This is because PLA customers, makers of integrated circuits, simply had to have the fastest and most capable photolithographic alignment equipment or they could not remain competitive in their own markets. The knife-edge existed because product functionality was the only basis of competition: PLA manufacturers would either fall off one side to rapid success or off the other side to failure. Clearly, such knife-edge situations make leadership in sustaining technology very important.

In most other sustaining situations, however, leadership is *not* crucial. This far more common situation is the subject of Richard S. Rosenbloom's study of the transition by National Cash Register from electro-mechanical to electronic technology. (See Richard S. Rosenbloom, "From Gears to Chips: The Transformation of NCR and Harris in the Digital Era," Working paper, Harvard Business School Business History Seminar, 1988). In this case, NCR was very late in its industry in developing and launching a line of electronic cash registers. So late was NCR with this technology, in fact, that its sales of new cash registers dropped essentially to zero for an entire year in the early 1980s. Nonetheless, the company had such a strong field service capability that it survived by serving its installed base for the year it took to develop and launch its electronic cash registers. NCR then leveraged the strength of its brand name and field sales presence to quickly recapture its share of the market.

Even though a cash register is a simpler machine than a photolithographic aligner, I would characterize its market as complex, in that there are multiple bases of competition, and hence multiple ways to survive. As a general rule, the more complex a market, the less important is leadership in sustaining technological innovations. It is in dealing with knife-edge markets or with disruptive technologies that leadership appears to be crucial. I am indebted to Professors Kim B. Clark and Robert Hayes for their contributions to my thinking on this topic.

2. This is not to say that firms whose product performance or product cost consistently lagged behind the competition were able to prosper. I assert that there is no evidence that leadership in sustaining technological innovation confers a discernible and enduring competitive advantage over companies that have adopted a follower strategy because there are numerous ways to "skin the cat" in improving the performance of a complex product such as a disk drive. Developing and adopting new component technologies, such as thin-film and magneto-resistive heads, is one way to improve performance, but there are innumerable other avenues for extending the performance of conventional technologies while waiting for new approaches to become better understood and more reliable. This argument is presented more fully in Clayton M. Christensen, "Exploring the Limits of the Technology S-Curve," *Production and Operations Management* (1), 1992, 334–366.

3. For the purposes of this analysis, a technology was classed as "new or unproven" if less than two years had elapsed from the time it had first appeared in a product that was manufactured and sold by a company somewhere in the world or if, even though it had been in the market for more than two years, less than 20 percent of the disk drive makers had used the technology in one of their products.

4. In this analysis, *emerging markets* or value networks were those in which two years or less had elapsed since the first rigid disk drive had been used with that class of computers; *established markets* or value networks were those in which more than two years had elapsed since the first drive was used.

5. Entry by acquisition was a rare route of entry in the disk drive industry. Xerox followed this strategy, acquiring Diablo, Century Data, and Shugart Associates. The performance of these companies after acquisition was so poor that few other companies followed Xerox's lead. The only other example of entry by acquisition was the acquisition of Tandon by Western Digital, a manufacturer of controllers. In the case of Xerox and Western Digital, the entry strategy of the firms they *acquired* is recorded in Table 6.1. Similarly, the start-up of Plus Development Corporation, a spin-out of Quantum, appears in Table 6.1 as a separate company.
6. The evidence summarized in this matrix may be of some use to venture capital investors, as a general way to frame the riskiness of proposed investments. It suggests that start-ups which propose to commercialize a breakthrough technology that is essentially sustaining in character have a far lower likelihood of success than start-ups whose vision is to use proven technology to disrupt an established industry with something that is simpler, more reliable, and more convenient. The established firms in an industry have every incentive to catch up with a supposed sustaining technological breakthrough, while they have strong disincentives to pursue disruptive initiatives.
7. Not all of the small, emerging markets actually became large ones. The market for removable drive modules, for example, remained a small niche for more than a decade, only beginning to grow to significant size in the mid-1990s. The conclusion in the text that emerging markets offer a higher probability for success reflects the average, not an invariant result.
8. The notions that one ought not accept the risks of innovating simultaneously along both market and technology dimensions are often discussed among venture capitalists. It is also a focus of chapter 5 in Lowell W. Steele, *Managing Technology* (New York: McGraw Hill, 1989). The study reported here of the posterior probabilities of success for different innovation strategies builds upon the concepts of Steele and Lyle Ochs (whom Steele cites). I was also stimulated by ideas presented in Allan N. Afuah and Nik Bahram, "The Hypercube of Innovation," *Research Policy* (21), 1992.
9. The simplest equation used by financial analysts to determine share price is *P* [H11505] *D/(C-G)*, where *P* [H11505] price per share, *D* [H11505] dividends per share, *C* [H11505] the company's cost of capital, and *G* [H11505] projected long-term growth rate.
0. This evidence is summarized by Clayton M. Christensen in "Is Growth an *Enabler* of Good Management, or the *Result* of It?" Harvard Business School

working paper, 1996.

1. Scott Lewis, "Apple Computer, Inc.," in Adele Hast, ed., *International Directory of Company Histories* (Chicago: St. James Press, 1991), 115–116.

2. An insightful history of the emergence of the personal computer industry appears in Paul Frieberger and Michael Swaine, *Fire in the Valley: The Making of the Personal Computer* (Berkeley, CA: Osborne-McGraw Hill, 1984).

3. "Can 3.5[H11033] Drives Displace 5.25s in Personal Computing?" *Electronic Business*, 1 August, 1986, 81–84.

4. Personal interview with Mr. William Schroeder, Vice Chairman, Conner Peripherals Corporation, November 19, 1991.

5. An insightful study on the linkage among a company's historical experience, its capabilities, and what it consequently can and cannot do, appears in Dorothy Leonard-Barton, "Core Capabilities and Core Rigidities: A Paradox in Managing New Product Development," *Strategic Management Journal* (13), 1992, 111–125.

6. Personal interview with Mr. John Squires, cofounder and Executive Vice President, Conner Peripherals Corporation, April 27, 1992.

7. See, for example, George Gilder, "The Revitalization of Everything: The Law of the Microcosm," *Harvard Business Review*, March–April, 1988, 49–62.

8. Much of this information about Allen Bradley has been taken from John Gurda, *The Bradley Legacy* (Milwaukee: The Lynde and Harry Bradley Foundation, 1992).