11

Creating Innovative Climates

31

A Skunkworks Tale

THOMAS J. PETERS

Innovation is unpredictable. It thrives in the chaos of "skunkworks," where product champions go scrounging for success.

Yellow Post-it Note Pads have quickly become as commonplace in the American office as paper clips. The product is a $100 million winner for 3M. The idea behind it came from a 3M employee who sang in a choir. The slips of paper he used to mark the hymnals kept falling out, and it dawned on him that adhesive-backed pieces of paper might solve his problem.

The requisite technology existed, and a prototype was soon available. "Great story," you say—but wait, this tale's not quite over yet. Major office-supply distributors thought the idea was silly. Market surveys were negative. But 3M secretaries got hooked on the product once they actually used it. Post-it's breakthrough finally came when 3M mailed samples to the personal secretaries of Fortune 500 CEOs, using the letterhead of the 3M chairman's secretary.

The Post-it story would amount to nothing more than a charming tale were this development

Tom Peters is founder and president of The Tom Peters Company.
For more information, see www.tompeters.com.

process not repeatedly played out at companies across the U.S. The course of innovation (idea generation, prototype development, contact with an initial user, and breakthrough to the final market) is highly uncertain. Moreover, it will always be sloppy, disorganized, and unpredictable, and that is the important point. It's important because we must learn to design organizations that explicitly take into account the unavoidable sloppiness of the process and use it to their advantage rather than fight it.

From America's best-run companies come tales of incredible perseverance, countless experiments, perverse and unusual product-users, five-person "skunkworks" sequestered in dingy warehouses for 60 days, plans gone awry, inventions made in the wrong industry at the wrong time for the wrong reason, and specifications for complex systems scrawled across the backs of envelopes. Innovation just doesn't happen the way it's supposed to.

THE 10 MYTHS OF INNOVATION

The hyperorganized approach leads companies to fall prey to the 10 myths of innovation management, which have already hampered many a firm. These false beliefs must be put to rest, and the sloppy side of innovation must be exploited. The 10 myths are as follows:

1. Specs and a market plan are the first steps to success.
2. Detailed strategic and technological plans greatly increase the odds of a no-surprises outcome.
3. Only a big team can blitz a project, especially if it is a complex one.
4. Contemplation stimulates creativity.
5. Big projects are inherently different from small projects and must be managed differently.
6. An organization must have a rigid hierarchy if would-be innovators are to get a fair hearing.
7. Product compatibility is the key to economic success.
8. Customers will tell you only about yesterday's needs.
9. Technology push is the cornerstone of success.
10. Perfectionism pays off.

Some companies love to make plans more than they love to make profitable new products. In these bureaucratic behemoths, someone's bright idea is turned into a six-month, $2 million study—a paper study. A paper evaluation of the study by various interested parties takes another three months. Some sort of design go-ahead is given, and writing the technical specs, at a cost of $3 million, takes six more months. The specs are evaluated in the four months after that.

During this last stage, a prototype is finally built. It costs $5 million to $10 million and takes four to six months to complete. And guess what? It doesn't work. Throughout the history of successful corporation innovation—from the development of French-fries seasoning at McDonald's to faded jeans at Levi Strauss & Company to the System 360 computer at IBM—neither the first nor the second prototype has *ever* worked. The successful innovators just go back to the drawing board.

But now the people in charge of the project really begin to sweat. By this time, careers are on the line and a lot of time, money, and pride has been invested in the design. So now they enter the "ignore the misfit data and make the damn thing work" stage. Meanwhile, the competitors have introduced three or four new products, each with several new features. As time goes by, the plodding planners fall further behind. So they recomplicate the product. "We're going to get it exactly right," they boast. But when they finally get it to the marketplace, it's adorned with so many bells and whistles that it doesn't work well.

This mentality is the antithesis of the Wee Willie Keeler approach. Wee Willie Keeler was a consummate opportunist who played baseball from 1892 to 1910. He once said, "Hit 'em where they ain't," and he proved that strategy's worth by making it into the Baseball Hall of Fame even though he stroked only 34 home runs in 19 seasons. His approach is imitated to a T by firms like Hewlett-

Packard, 3M, McDonald's, Wang Laboratories, PepsiCo, Citicorp, Johnson & Johnson, Digital Equipment, and others like them. This philosophy says, in effect: *Start out by spending $25,000, or even as much as a quarter of a million dollars. Build a prototype, or a big hunk of one, in the first 60 to 90 days. And then poke it to see if it moves.*

Whether projects like these involve aircraft, missiles, or French fries, the results achieved by scores of companies suggest that something can always be built in this length of time. The evaluation of the prototype should take another 60 days. (Even at such an early stage, firms following this approach may decide—explicitly or not—to start up a second team doing roughly the same work as the first, just to get a different look.)

"We're already playing with something tangible," say project leaders at these companies. "Now we take the next little step. We build another new version in 90 days. It's a more developed prototype that will cost a little more, around $100,000 to $200,000. After it's built, we can probably get it, or part of it, into a user's hands—not an average user (that's still years away), but a lead user who's willing to experiment with us. Even an in-house lead user might do the trick." And on goes the process, always involving investments that increase little by little and time-frames that do the same.

At each step the innovators learn a little more, because they set up harsh reality tests with hard products and real users. If something doesn't work, they weed it out quickly before career lock-in and irreversible psychological addiction to hitting home runs take place.

In the aircraft manufacturing industry, one such harsh confrontation with reality is known as the chicken test. Aircraft engines have to be built to withstand possible ingestion of flocks of birds. To determine what would happen in that unlikely event, engineers buy 15 or 20 gross of chickens, stuff some into a cannon with a barrel four feet in diameter, and fire them at engines running full throttle. It's the ultimate of pragmatic tests. Rolls-Royce spent several years and several hundred million dollars on a new graphite-material engine. After all that, it failed the chicken test.

What the Wee Willie Keeler, or experimental, approach boils down to is getting your inevitable chicken test out of the way early. Every new product fails a chicken test or two at some point. The burning issue is, when does it fail? At the end of four years, by which time the competitors have a new array of products on the market? Or at the end of 90 days?

BUREAUCRACY UNDER ATTACK

Strategic planning is being attacked on all fronts. Many claim that it is too rigid. Others say it's too bureaucratic. Some believe that corporations should at least decentralize such planning (General Electric and Westinghouse, who were early pioneers in strategic planning, are doing just that). A few even suggest that we get rid of it altogether.

But do we really want to do that? The new "in" terms are *technology* and *production*. "Technology planning" and "manufacturing planning" are the preferred substitutes for strategic planning. Before heading off down a new trail, though, let's look at the record of technology planning. It's hardly spotless.

Think of recent inventions that we're all familiar with. "We do not consider that the aeroplane will be of any use for war purposes," declared the British minister of war in 1910. In the late 1940s, market research predicted that the total sales of mainframe computers would be about a dozen. Even though the robotics industry is crowded with such competitors as United Technologies Corporation, General Electric, Westinghouse Electric, and IBM, the first "intelligent mobile robot" will come from a less-than household name—Denning Systems Inc. of Washington, D.C., a classic three-inventors-in-a-garage operation.

A highly systematic analysis of this phenomenon may be found in the book *The Sources of Invention* by John Jewkes, a professor of economic organization at Oxford University. After studying the development of 58 of this century's major inventions, Jewkes concluded that at least 46 of them

occurred in "the wrong place." Note the unusual origins of the following inventions:

- Kodachrome film was invented by a couple of musicians.
- A watchmaker fooling around with brass castings came up with the process involved in the continuous casting of steel.
- The developers of the jet engine were told by reciprocating-aircraft-engine people that it was useless. (They finally peddled their invention not to engine-makers but to airframe-makers.)

According to Jewkes, there is no industry group in which much innovation has taken place as or when it was supposed to. On top of this, "the initial use and vision for a new product is virtually never the one that is ultimately of the greatest importance commercially," reports Jim Utterback, an MIT associate professor of engineering who for more than a decade has studied the development of inventions. He has concluded that users play a special role in this process. To support his point, he recounts the path to success of invention after invention.

His analysis of incandescent lighting is typical. Its first use was on ships, which in retrospect seems natural enough: it's dangerous to keep gas lamps on a seafaring vessel, whose rolling motion can upset them. Thus the incandescent light found its first home in a highly specialized market niche. Then, in a move that *every* marketresearch department could easily have predicted, incandescent lighting spread to—baseball parks! Night games have been with us ever since. From there the invention moved to neighborhoods, where it replaced gas streetlamps, and only 15 years later did incandescent lights begin to make it into homes. As a more recent example of this pattern, transistors were first used for missile guidance systems; their use by home consumers lagged 20 years behind.

The role of corporations in all this is truly frightening. Organizations have an apparently inherent tendency to make exactly the wrong moves in trying to stimulate innovation, according to Utterback, who states, "In 32 of 34 companies, the current product leaders reduced investment in the new technology in order to pour more money into the old."

Not only, then, does the leader *not* embrace the new, he actually reduces his investment in the new to hold on to the old. The problems involved in switching to a new technology are manifold. First, there's scientific hubris (the engineer knows best, he can predict the use of the product most accurately); then comes marketing hubris (how could all those tons of data on the Edsel be wrong?). Jewkes offers three rules of thumb regarding technological planning, all of which are well worth heeding:

- Peering into the future is a popular and agreeable pastime that, if not taken seriously, is also comparatively innocuous.
- There is a great virtue in picking and choosing from a variety of available options.
- The industrial laboratory does not appear to be a particularly favorable environment for the inducement of innovation.

Does this mean that corporations should do away with central planning? Should centralized R&D activity be abolished? The answer is no. First, one does need to make general bets on technological directions: it's important to know the difference between, say, north and northwest. That's fine. What isn't sensible is trying to prespecify the difference between a course of 43 degrees and a course of 46 degrees. As a former managing director at the consulting firm McKinsey & Company liked to argue, "About the best you can hope for is to get the herd heading roughly west." And this is a task that centralized research can do.

"As a regimen or discipline for a group of people, planning is very valuable," notes Fletcher Bryom, the iconoclastic former chairman of Koppers Company. "My position is, go ahead and plan, but once you've done your planning, put it on the shelf. Don't be bound by it. Don't use it as a major influence on the decision-making process. Use it mainly to recognize change as it takes place."

QUICK-AND-DIRTY SOLUTIONS

When the U-2 spy plane emerged as the country's most sophisticated airborne surveillance system 30 years ago, many experts said that it would never fly. It's still doing yeoman service. The developers were a retired aeronautical engineer named Kelly Johnson and a small band of Lockheed Corporation mavericks. They called their off-line group "the Skunk Work"—the original business use of an apt term that (as far as I can determine) may have been coined by Al Capp, who drew the comic strip *Li'l Abner*.

Lockheed is not unique. At GE the same activity is called "bootlegging"; at 3M they label it "scrounging." It would not be difficult to argue that 3M, Hewlett-Packard, Digital Equipment, and Johnson & Johnson are today nothing more than collections of skunkworks.

The finding stands out more and more clearly as the evidence rolls in: whenever a practical innovation has occurred, a skunkwork, usually with a nucleus of six to 25 people, has been at the heart of it. Most skunkworks seem to do things in an incredibly short period of time. While visiting a Westinghouse lab, General Curtis LeMay, then Chief of Staff of the Air Force, found a pencil sketch of what was at the time a beyond-the-state-of-the-art product: a side-mounted radar. He asked if he might have one within 90 days. The next day he sent Westinghouse an airplane to hang it on. He got his device less than 90 days later. In the recent book *The Soul of a New Machine* by Tracy Kidder, Data General's computer-project leader, Tom West, speculates that the company's crucial breakthrough in microcoding may have taken place in less than a week. [*Editor's note:* Microcoding builds into a computer the instructions that make it operate.]

But what happens with a quick-and-dirty skunkwork project? Is the quality as high? Does it ever fit into the rest of the product line? The record shows, delightfully, that the stuff that comes from skunkworks is often of high quality, even though it was invented in a fraction of the so-called normal time.

The creative impetus behind skunkworks boils down to ownership and commitment. In *The Soul of a New Machine,* West describes the phenomenon: "There are 30 guys out there who think they've invented it; I don't want that tampered with." Firms like 3M, Johnson & Johnson, and Hewlett-Packard all agree that in creating the sense of ownership, intense commitment, and unbounded energy that comes from turned-on teams, a surprisingly small group is optimal.

A struggle against others is also important. It, too, engenders feelings of ownership and commitment. Interestingly, its most important form is rivalry with others *inside* the company, not with an outside competitor. Few companies are really familiar with their competitors, but their divisions sure know one another. Constructive internal competition is difficult to manage. There are a great number of subtleties and traps. The net result, however, is almost always positive.

The skunkwork cannot do all things. On the other hand, the empirical indications seem to say, loud and clear, "Ignore this form of organization for innovation at your own peril." The alternative is *de novo* design of the tiniest parts, excessively long product-development cycles, large teams in which ownership and commitment are missing, do-everything-inside attitudes, overcomplexity, and situations in which competing central staffs make the decisions on technical issues or delay them endlessly on the basis of the most tenuous market or financial projections. The show just doesn't get on the road.

HELL-BENT ON SUCCESS

If big, well-orchestrated teams were at the heart of successful innovation, we would expect to find them populated with powerful thinkers who regularly ascended to their mountaintop retreats to look out over the pines. As a result of such reflection, they would accomplish the necessary breakthroughs, presumably on schedule. If, on the other hand, rough-and-tumble skunkworks, hell-bent on outproducing some formal group, were the norm,

we would expect to find bleary-eyed folks staring at computer screens or test tubes in dirty, forgotten basement corners.

It does turn out that bleary eyes play quite a large role in innovations. When a year's worth of work is routinely accomplished in five weeks, someone called a "champion" will be found at the heart of the operation. Formal IBM in-house studies of research projects always unearth a champion. National Science Foundation studies suggest that the champion's role in pushing an idea to fruition is crucial. When the brand manager of a consumer-goods company, even in a highly structured system, becomes a determined champion, the odds of success go up tenfold. Looking back over his career in *Adventures of a Bystander*, Peter Drucker, the noted business expert, remarks, "Whenever *anything* is accomplished, it is being done, I have learned, by a monomaniac with a mission."

A crucial corollary is that the corporation that would nourish inventors must also tolerate, even praise, failure. Going through 3M's roster of senior officers with one of the company's executives a couple of years ago, I discovered that virtually every 3M officer had reached the top because he himself had introduced several important new products. Moreover, each story, as it was recounted in conventional form, focused on the rough places in the road: the 10 years of ups and downs when the product was too advanced for the marketplace, when it had to be reformulated, when the manufacturing scale-up didn't work. Setbacks are considered standard operating procedure. Above all else, the winners are those who persist.

SMALL WITHIN BIG

Massive projects like the manned space program or the development of the transistor at AT&T Bell Labs aren't that different from less complex undertakings; they're just bigger. They too can be treated, to a substantial degree, as collections of skunkworks. In an important sense, the principle "small within big" turns out to be essential to the success of big projects. Most of the breakthroughs in these cases are the results of champions' operating off-line. Charles Brown, chairman of AT&T, said recently, "Today the long-distance network looks like one big, perfectly conceived solution. The reality that we often forget when we think about innovation planning is that the network is a collection of thousands of small breakthroughs that occurred here and there, and certainly not according to schedule or by courtesy of a flawless master plan."

The story of Boeing's recent development of the air-launched cruise missile is even more pertinent. The system is complex. Undoubtedly it should have been developed all at once, with the aid of a 100,000-bubble PERT chart (a "program evaluation and review technique" diagram that indicates the relationships among the phases of a project). The missile-development program was in fact broken down into seven major pieces. Modest-size teams were assembled to deal with the seven projects. Each task was then accomplished in a remarkably short period of time relative to the norm. Each had a champion. Each was in competition with all the others on several vital fronts.

Then what happened? You guessed it. Put the seven pieces together and they don't fit exactly right. So you have to spend some time, as much as a few months, getting the interfaces just right, despite the prior effort that went into interface specifications. (Twice-a-week meetings of a "tie-breaker" group sorted out many of the issues in question.) The final design isn't as technically beautiful as ideals of theoretical perfection suggest is possible. But multiple passes usually take less time and result in the development of simpler, more practical systems than a single everything-at-once pass. (Boeing's cruise missile was delivered more than a year ahead of schedule and well under budget.)

But back to the question of whether big differs from small. There is no question that it does. The Boeing 767 and the French-fries seasoning change at McDonald's are not the same. On the other hand, commitment, championing, small within big, piece-versus-piece competition, the overtight deadline, and the turned-on modest-size

group are the keys to breaking down a big, forbidding task into smaller, more manageable ones.

CHARGED-UP TEAMS

The conventional wisdom holds that only a strong functional monolith will keep the engineers' (and innovators') viewpoints to the fore. It's a nice argument on paper, but it doesn't hold much water in practice. What actually happens is that engineers lose out to marketing and finance people in divisional organizations. The divisions are interested only in short-term profit.

By definition, the functional monolith is almost always bureaucratic; it's not oriented to commitment and small-team action. Too many firms force creative people to work on five or six projects that span three or four divisions. But my experience on this one is crystal-clear. No one with one-seventh of the responsibility for anything ever felt committed to it. Peter Drucker's "monomaniacs with missions" were not monomaniacs with *seven different* missions.

Under some forms of management, divisional organizations that grow too big become hopelessly bureaucratic. On the other hand, "the division is the solution" (and the strategy) for Hewlett-Packard, 3M, Johnson & Johnson, Emerson Electric, and the like. Johnson & Johnson constantly creates new divisions. Its corporate watchword is simple: "Growing big by staying small."

These companies carefully monitor the size of their divisions. At HP, divisions are kept to less than a thousand people so that, in president and CEO John Young's words, "the general manager will know all his people by their first names." Bill Gore, chairman of W.L. Gore & Associates, comments, "As the number of people in an organization approaches 200, the group somehow becomes a crowd in which individuals grow increasingly anonymous and significantly less cooperative." The low numbers, whether 200 or 1,000, are all aimed at enhancing ownership and commitment.

Another vital part of the small-team, small-division mentality is the ability to manage, with relatively little muss and fuss, the bureaucratic conflicts that fatally delay much development. As an old hand at skunkworking once said, "Let's be clear about the magnitude of the effects that small teams have. The charged-up team that contains 10 to 50 people isn't in the '10 percent productivity improvement' game. Its results are often 300, 400, even 700 percent beyond those achieved by larger groups."

GET IT OUT THE DOOR

Some firms don't believe in meeting product-release dates. First the date is pushed back three months; then it gets shoved back another 45 days. All the while the bosses are thinking, "We've got to make sure that the software is totally compatible with all the rest of the product family." So the logic goes.

Compatibility is important, particularly in the case of systems-related high-technology products. But sometimes the last 2 percent that's needed for 100 percent compatibility takes 12 months to achieve. Meanwhile, 10 competitors have found a solution to the problem and gotten their products to the marketplace. In such extremely fast-paced markets as data handling, computers, and telecommunications, though, there are literally thousands of entrepreneurs who will fill in the spaces and do the last 2 percent of the work for you.

Digital Equipment's products overlap; users occasionally found that some of its products are incompatible with products that they're supposed to be compatible with. HP's engineers, marketers, and salesmen also lament the incompatibility of some of their products. But companies that wait, trying to achieve the last percentage point of compatibility, may well go belly-up.

The same principle holds true in many other markets, although they show a little less intensity. That's the reason Proctor & Gamble, 3M, Mars, and Johnson & Johnson are so insistent about spurring competition among their own divisions and brand managers. Bloomingdale's does the same thing with buying and floor-space assign-

ments in its stores, and Macy's has done extremely well emulating Bloomingdale's. In most markets, new things are happening all the time. The lion's share is often virtually invisible—that is, you frequently don't see it until it's too late. To keep up with the competition, you have to keep getting new items into the market.

Errors of premature release can be (and frequently are) disastrous. Often a product hits the marketplace before the bugs have been worked out. Its technical superiority is blunted by poor reliability or insufficient support. This type of nightmare must be avoided at all costs. But getting that last possible feature, that last degree of complexity (read "overcomplexity"), that last percentage of compatibility, may cost you more of the market than you would have gained by making a perfect product. Unfortunately, the perfectionists tend to get their way because they always use the argument "It'll only take us another 30 days." But we all know that those 30-day projects always seem to take 120 days—if you're lucky.

CUSTOMERS GENERATE IDEAS

The evidence is overwhelming: the great majority of ideas for new products come from the users. Eric von Hippel, a professor at MIT's Sloan School of Management, has studied scientific-instrument equipment manufacturers, and his results are revealing. He reviewed 160 inventions and found that more than 70 percent of the product ideas originated with users. And these weren't just bells-and-whistles ideas, either. Sixty percent of the minor modifications came from users, as did 75 percent of the major modifications. But astonishingly, *100 percent* of the so-called "first of type" ideas for sophisticated devices like the transmission electron microscope were user-generated. According to von Hippel's studies, users that got their ideas across to the producers did a lot more than whisper into their ears. The users came up with the ideas, they prototyped them, they debugged them, and they had them working. Only then did they tap the producers for their experience in reliable production of multiple copies.

Lead users don't have to be Ph.D.'s or work in germ-free labs. One classic lead user was a housewife whose husband worked at the Corning Glass labs. One day he took home a new glass container that he was going to store acid in. She accidentally used it to heat some food in the oven, and it didn't break. Such is the origin of Pyrex cookware!

Stay in touch with users. It's important in every industry from fast food to computers. Hewlett-Packard has coined the term MBWA—"Management by Wandering Around." Wandering around should mean listening to the user in a direct, not an abstract or shorthand, way. A general manager who designed a major new computer describes a neat trick he pulled off: "I bought my uncle a computer store. I spent nights and weekends working there. My objective was to stay close to the ultimate user, to observe his frustrations and needs firsthand and incognito." What he learned was reflected in the eventual computer design in a thousand little ways and several big ones.

SERVICE AND QUALITY COUNT

"More scientists in bigger labs" seems to be the conventional watchword, along with "Better planning, better tools." The heck with skunkworks. But it's more than skunkworks. It's more than listening to users, too. Service and quality hold as much value as gee-whiz technology—or more.

Recently I talked with the president of a technology company about commodities. He was disturbed by some people's unfortunate tendency to call high-technology products (chips, instruments, personal computers) "commodities." The problem with this is that if you label a specific product a commodity, you'll start to behave as if it is one, neglecting service and quality. For instance, let's take a mundane product: toilet paper. If you go to your local grocery store and purchase a four-roll, 220-square-foot package of one-ply generic-brand

toilet paper, the price will be around 79 cents. But if you go to a Seven-Eleven-type grocery, a package of Procter & Gamble's Charmin will cost you $1.99. The difference in distribution channels (Seven-Eleven) and the quality difference (P&G) is obviously enough to add $1.20 to a 79-cent product—or, more accurately, to add $1.20 to a product that cost about a quarter to produce.

Technology push is crucial, but it is not the principal reason that America is undergoing so many industry setbacks. User-unfriendliness, the inability to realize that the customer perceives a product in his own terms, is at least as big a weakness. If you don't believe me, ask 'em in Detroit.

PERFECTIONISTS FINISH LAST

If it weren't for people, 10,000-person research groups would be the most efficient. If it weren't for people, execution via 100,000-bubble PERT charts would be the most efficient. If it weren't for people, huge amounts of money invested in technical forecasting would allow companies to anticipate competition, customer-related problems and technological surprises. If it weren't for users, in-house development of every part that went into every invention would be the best way to assure quality.

Optimization. What's optimal? It's hard to believe, but the "suboptimal" system is often the most truly optimal. Go back to the big-versus-small debate. As a way to do the job, skunkworking is faster, cheaper, and higher-quality than the optimization route. Getting 90 percent compatibility and letting the marketplace do the rest turns out to be optimal, not suboptimal. Getting the last 10 percent may cost you 60 percent of the market.

Tom West of Data General didn't care a whit about building a machine that the "technology bigots" would like. He was interested in people who "wanted to get a machine out the door with their name on it." The stories about the U-2, the missile-development program broken down into seven parts, and the Post-it pads seem to be the same. Committed people, people competing against the market and other corporations and other divisions, those are the people who get the job done. Hail to the skunkworks!