How To Fail In Project Management (Without Really Trying)

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The use of project management techniques has become an increasingly well-accepted method for performing a wide range of organizational tasks. More and more companies are coming to understand the unique benefits that can be derived from project management, including rapid product development, better and more efficient use of all resources (human and monetary), and increased and more productive cross-functional communication.

Even more important, project management is being used by a wide range of disciplines and corporations that had never previously considered it as a viable method for performing work. Legal offices, hospitals, and other services, as well as traditional manufacturing firms, have become enthusiastic about the ways in which project management is improving their delivery of services or creation of new products. Indeed, in their quest to continue to stay ahead of the curve, these organizations are finding that project management techniques are becoming an indispensable part of their operations.

Coupled with the increase in project management techniques must be the expectation that, without adequate training and with unrealistic expectations, many of these new projects will ultimately fail. This statement should not surprise any readers; we have all had experiences in which our organizations have adopted new methods of operations. A common side effect is the inevitable "teething pain" the company must go through as idealized theory meets practical reality. And yet, even with the problems these companies experience, there is a load of valuable information to be learned, particularly when it comes to understanding the nature of such failures.

In the continuing quest for better project management skills and techniques, experience plays a crucial role. It is, however, a two-edged sword, consisting of both positive and negative consequences. Nevertheless, even our failures have their use. We gain wisdom every bit as much—if not more—from failure as from success; we often discover what works by finding out what does not. Indeed, it can reasonably be argued that those who never made a mistake also never made a discovery. Horne Tooke used to say of his studies in intellectual philosophy that he had become all the better acquainted with the country through having had the good luck to lose his way. In a similar vein, many of us count as our most valuable management lessons those that were learned as the result of failure.

Why examine project failures and disasters in the first place? There are specific lessons to be learned, of course, because such studies also yield valuable data in relation to future projects. Unfortunately, the cost of these mistakes is usually painfully high. Real life examples of project disasters can be an invaluable source of information and provide real insight into the way mismanagement can wholly negate an otherwise successful project undertaking. One has only to make a cursory search of current management literature to see compelling examples of projects that have failed, usually with serious consequences for the firms. To illustrate, Borland's upgrade of DBaseIV was so poorly managed that the product had to be removed from store

> *There are lessons to be learned from failure, if only we are willing to find and examine them.*

shelves for comprehensive debugging, costing the company tremendous expense and customer goodwill. Denver's new multibillion dollar international airport was plagued by so many technical problems that its opening date was repeatedly pushed back, costing the city and airport authority more than $1 million a day in late penalties and interest.

In our research and consulting experience, we have seen that most companies spend thousands of hours to plan and implement a multimillion or even multibillion dollar investment, but far too little time critically evaluating and learning from their experiences. They could be asking simple but vital questions:

- Was the investment worthwhile?
- Did it go according to plan?
- If yes, how? If not, why not?

Because each project is unique to some degree, each differs greatly from traditional, functional business tasks. As a result, each project, if poorly managed, can have an immediate negative effect on the parent company. Learning is not easy—for people any more than for organizations. We don't seem to have much of a "memory" if we are to judge by the constant repetition of the same types of mistakes leading to similar disasters. Indeed, three of the worst tragedies ever—Bhopal, India, the Exxon Valdez, and the space shuttle Challenger—are constant reminders of this fact. In all cases, the agencies concerned were utterly confused just after the event, and their numerous contradictory statements made the confusion even worse. The real causes of failure in the vast majority of failed projects is often difficult to ascertain, thanks to human ingenuity for sweeping unpleasant facts under the carpet. This is indeed a pity, particularly in light of the fact that these causes eventually do surface, despite many companies' systematic damage control procedures. For example, once the details of the above mentioned failures became clear, headlines in the international press excoriated the organizations concerned—Union Carbide, Exxon, and NASA—as singularly ill-prepared to cope with such disaster.

When analyzing mistakes and their principal causes, two important lessons should be apparent to every careful reader. First, all organizations, no matter how successful they have been or will continue to be, make mistakes. The Ford Corporation has had a history of highs and lows in this regard. It dominated the automobile world with its Model-T and mass production techniques, then allowed itself to stagnate to the point at which General Motors took over the number one position. In 1960, Ford hatched the disastrous Edsel, yet within four years it was able to follow it up with the hugely successful Mustang. That is the nature of business events—the cycle moves through both highs and lows. For every project success, there will always be at least one failure.

The second lesson is equally clear: Where there is failure, there is the potential for learning. Unlike the first lesson, which is obvious to most of us, the second may be threatening. It says, in effect, that failure is not to be pushed aside, but studied. Learn from mistakes—learn how *not* to do it. J. Edwards Deming's famous dictum on quality is to get the process right and then repeat it. The reverse is also true: Learn what did not work and then avoid it in the future. In either case, make the result, unpleasant though it may be, an opportunity for personal and organizational learning. Sometimes the project failures are so small in scope that their losses can quickly be erased. Other times the failure is more monumental, resulting in long-term or even permanent pain. In no case, however, should such failures be forgotten.

A QUICK GUIDE TO RUINING YOUR PROJECT

Not every project deserving of success achieves it. Conversely, not every project heading for the scrap heap arrives. Any number of events beyond the control of the project team and parent organization can help or hinder a project's chances of success.

Figure
How To Ensure A Project's Failure

1. Ignore the project environment (including stakeholders).
2. Push a new technology to market too quickly.
3. Don't bother building in fallback options.
4. When problems occur, shoot the one most visible.
5. Let new ideas starve to death from inertia.
6. Don't bother conducting feasibility studies
7. Never admit a project is a failure.
8. Overmanage project managers and their teams.
9. Never, *never* conduct post-failure reviews.
10. Never bother to understand project trade-offs.
11. Allow political expediency and infighting to dictate crucial project decisions.
12. Make sure the project is run by a weak leader.

Nevertheless, when we consider those activities and decisions that can play an important role in a project's failure, our research and experience point to some important contributing causes of project failure. Consider our list of a dozen sure-fire methods, shown in the **Figure**, for ruining your project's chances of success.

1. Ignore the project environment (including stakeholders).

One of the best ways to consign a project to almost certain failure is to manage it without regard for the organization's external environment, including those project stakeholders who can play such an important part in its success or failure. Project "stakeholders" is the term used to refer to any group, internal or external to the company, that has an active stake in the project's development. They include clients, the overall marketplace, internal functional departments, top management, the project team, and external groups that have been termed "intervenors" by Cleland (1988). Intervenors include any environmental, social, political, community-activist, or consumer group that can have an impact on a project's successful development and launch. To ignore the potential power of such stakeholder groups is foolhardy and often results from either ignorance or complacency on the part of the developing organization.

Consider the case of the Bailly nuclear power plant proposed by the Northern Indiana Public Service Company (NIPSCO) in 1972. It was originally intended to be located adjacent to the Indiana Dunes National Lake Shore in northwest Indiana. NIPSCO acquired all necessary construction permits and began work on the site in the belief that it was important to build such plants to shelter its customer base from the rate increases due to an overdependence on oil. At the time, the idea of the plant seemed to make sense, particularly in light of the "oil shocks" that were to become commonplace throughout the remainder of the decade.

Whether or not the idea made sense, neighbors of the proposed new facility had other ideas. Several neighborhood opposition groups formed, originally composed of affluent homeowners who found they would be adjacent to the nuclear power plant. Once united, they formed the "Save the Dunes Council" and set about constructing a legal mine field to oppose the licensure and operation of the facility. In time, these original opponents were joined by more and more environmental and special interest groups, which filed their own writs in support of the Save the Dunes Council's position. Their concerns ranged from safety (they termed NIPSCO's evacuation plan unworkable) to concern for the envi-

ronment (their fear was contamination of a large section of Lake Michigan).

At the heart of the dispute was the difference in goals between NIPSCO and the Save the Dunes Council. The public utility had a goal of providing cheap and long-term energy for its client base. The intervenor groups sought to maintain the pristine quality of life along the lake shore while protecting themselves and their property values from an unwelcome intrusion. After years of legal sparring in which the case went all the way up through the United States Supreme Court (where NIPSCO won), the utility determined that the continual legal and social battles in support of the facility were likely to continue indefinitely. Finally, in 1982, after ten years spent trying to develop the nuclear power plant, NIPSCO withdrew its proposal. All that remained was a very large hole NIPSCO had excavated at the proposed site and a total cost to the utility of more than $200 million dollars.

This case is simply one example of the problems that can occur when project organizations forget their client base or assume they know more than their stakeholder groups. One clear message that comes through time after time is the prevailing power of such stakeholder groups in aiding or thwarting a project's successful development. The corollary is to bear in mind that not all stakeholders are external to the organization. Many projects have been derailed due to opposition (either overt or covert) from other functional groups or operating divisions. Prior to the "go" decision, one highly important factor must be considered: the receptivity of the organization's internal environment to the proposed project. If there is the faintest suspicion of disharmony, it is important to take time to reassess the reasons and take corrective action, including working with stakeholder groups to understand their concerns or making necessary adjustments to the project.

2. Push a new technology to market too quickly.

New technologies imply new and unknown risks. Sometimes the allure of being first to market with a new technology causes companies to cut corners, marginalize safety factors, or make quality trade-offs. In the end, though, these decisions almost invariably come back to haunt the firm's executives, sometimes with tragic results. The Tacoma Narrows Bridge employed a well-understood technology, suspension bridge construction, in a new way with unique physical characteristics—a very long but narrow structure set over a natural "wind tunnel" site. The result was to push "well-known" technology beyond the breaking point, resulting in catastrophic failure.

Likewise, DeHavilland's desire to be first to market with a commercial jet resulted in the creation in 1952 of the Comet, a faulty design that ultimately killed scores of people and by 1954 was withdrawn from the market. In contrast, the first American jetliner, Boeing's 707, made appropriate use of many of the engineering lessons from the Comet and created a safer and hugely profitable product.

New technologies are very tempting to exploit for exactly that reason: They *are* new. They offer the company a leg up on competition. Unfortunately, in the rush to push these new designs or technical achievements, there is a strong likelihood of inadequate or cursory pretesting that can result in disaster. There must be a proper balance between being the first to market and ensuring that the product will perform in positive, expected ways.

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Quality has been defined by Genichi Taguchi, a well-known Japanese engineer and writer, as avoiding "the loss a product causes society after being shipped" (Evans 1993). Taguchi's message is clear: When a project is developed too rapidly, to the point at which there are potential questions regarding its performance, it poses a threat to society and hence cannot be considered a quality offering.

3. Don't bother building in fallback options.

All projects run into trouble at one time or another. The question is not whether problems will occur, but to what degree. When difficulties begin to impede progress, one of the tests of good project management is how quickly the project is brought back on course. This point is important because it disputes the notion some readers may have that "good" project managers are those whose projects never get into trouble. That belief is patently untrue. Not all problems are foreseeable. Consequently, the true test of successful project managers lies in their flexibility and capacity to respond in clearheaded ways to problems once they occur.

A logical exercise in which project managers must engage is to continually ask a series of "What if?" questions. This forces the project manager and the team to search out likely problem areas actively rather than wait for trouble to find them. An important side note: Research has dem-onstrated that the project managers who spend adequate up-front time developing a series of "What if?" scenarios and their responses to them are more successful than those who operate in a purely reactive manner, waiting until problems occur before weighing their various responses. In fact, a large-scale study of project failure conducted by Pinto and Mantel (1990) found that the number one cause of failure was the lack of adequate troubleshooting. The study demonstrated the strength of this underlying message: namely, that problems are inevitable. However, successful project managers are those who are best able to adapt to the new situation with flexibility, look for opportunities, and bring their projects back up to speed rapidly.

4. When problems occur, shoot the one most visible.

Problems with projects, particularly on a large scale, tend to induce an element of panic from top management. Often such panic leads to foreseeable and regrettable outcomes: the knee-jerk sanctioning of key personnel. Once top management is in the panic state, it is common to see heads begin to roll, starting with the project manager and anyone else who is clearly seen as part of the decision-making team. Such a mind-set is no different from that which used to be regularly practiced within unsuccessful or obstreperous armies—shooting a scapegoat *pour encourager les autres*. The similar belief exists in many corporations: A public sacrifice will keep everyone else working productively.

In the absence of obvious incompetence or misbehavior, however, the consequences of such an extreme reaction should be clearly considered before it is acted upon. Frederick Brooks, in his 1975 classic *The Mythical Man-Month*, demonstrates that personnel changes in the midst of a project, particularly when an element of urgency has crept in, are almost invariably counterproductive. Because of the learning curve, it takes that much longer to bring new personnel up to speed on the project, delaying it further into the future. Prior to making personnel changes, ask the question, "What do we hope to accomplish by this change?"

Many writers on Japanese management techniques have contrasted the common American mind-set of "summary execution" with the more measured Japanese response of going hard on the problem but soft on the people (to paraphrase Fisher and Ury 1981). Their attitude can best be summed up by the dictum, "Fix the problem, not the blame." The blame game involves a counterproductive, vicious cycle and ultimately does little to solve the problems that brought the project to its current state. Although there is no

doubt that in the face of ongoing problems with a project it is tempting to demonstrate decisive action in the form of reorganizing and punishing, it is important to consider first the reason for such actions and the message we intend to send. Project managers who are constantly looking over their shoulders out of paranoia and fear of retribution are not capable of taking necessary risks and acting in ways to further project success.

5. Let new ideas starve to death from inertia.

The flip side of pushing new technologies out the door without having spent adequate time assessing problem areas is to allow new products to remain in a holding pattern indefinitely. For example, few readers will recognize the Xerox Alto personal computer, despite the fact that personal distributed computing is a concept that was developed at the Palo Alto Research Center (PARC) in the early 1970s. In fact, Xerox had a working prototype of a personal computer, complete with mouse, word processing software, and a laser printer, by 1972. Yet during the 1970s, because of a combination of political infighting and bureaucratic roadblocks, Xerox never developed the Alto into a commercial product, thereby sacrificing millions in profits over the next two decades.

How did a company manage to create a surefire winner and then sit on it? Certainly organizational inertia played a role. There was no obvious avenue for bringing these products to market and Xerox executives lacked the will to take a gamble. Further, the training of their top management team was predominantly financial; the numbers game, with its low propensity for risk, enthralled them. The result was Xerox's apparent willingness to forgo huge profits in order to take the safe route. The irony, of course, is that this same company had made its reputation by taking a large risk in introducing the model 914 copier in the early 1960s and revolutionizing office technology.

In 1979, Steven Jobs, the aggressive, energetic founder of Apple Corporation, was given a tour of Xerox's research facility in Palo Alto. Shown a demonstration of the capabilities of the Alto, he was amazed that Xerox had failed to market the machine, which he was sure would set a new standard in the personal computer industry. What do you suppose would have been his reaction had he been told that Xerox had developed the machine almost six years earlier and had been sitting on it ever since? Jobs' sense of disbelief is understandable. For want of the will to take a risk, Xerox lost a dominant position in personal computers. The rest, as they say, is history.

6. Don't bother conducting feasibility studies.

Why waste time checking to determine whether a new technology will work? Why worry about harmful side effects? Why bother considering customer concerns? Obviously, the answer to each of these questions is because failing to do so is one of the surest roads to project failure. Feasibility planning implies an organization doing its up-front homework to put itself in the position to conclude a project successfully. Feasibility studies require that project managers and upper management devote sufficient time to understanding the project's risk analysis, cost analysis, time frame to completion, stakeholder analysis, and other relevant information before funding is approved. Certainly the real danger of such analyses is that they operate under a "Garbage in—garbage out" philosophy, whereby someone purposely loads up the evidence in one direction or the other to support an *a priori* attitude.

Some years ago, a team of mid-level managers at a large U.S. corporation was formed into an internal study group and given an assignment to assess the feasibility of investing in the development of a new product. Following extensive analysis over six months of study, the group reconvened to present its findings. The only observer at the first presentation was a corporate vice president, who listened impassively for the first five minutes. Once it became clear the study group's recommendation would be in favor of funding the new venture, the vice president quickly interrupted. "You came to the wrong conclusion!" he said, dismissing them to "rethink their position" prior to the presentation before the full executive committee the next day. As you might imagine, their presentation the next afternoon strongly recommended against funding the project.

The benefit of accurate and reasonable feasibility planning is that it locks the company into a mode of planning, then execution. This approach is equivalent to the "Ready, aim, fire" model that typifies effective companies. The alternative, "Ready, fire, aim," leads to an incredible amount of waste as project after project is initiated with only minimal up-front assessment.

The Eurotunnel is indeed a monument to technological achievement. Boring three tunnels under the English Channel to link Great Britain with France will become a textbook example in Civil Engineering courses. Yet one year after

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commissioning, the Eurotunnel Corporation was set to default on more than $12 billion of debt. Economic analysis was demonstrating that the revenue streams the "Chunnel" was hoping for were not materializing—nor are they likely to through the end of this decade. Quite simply, most travelers find the prospect of traveling in a dark, stuffy environment not worth the prospect of merely saving one hour in travel time. The Channel Tunnel represents a technical achievement to be sure, but it is also a financial morass.

7. Never admit a project is a failure.

Sooner or later, every project will turn itself around, right? Wrong. Many projects fail because of mismanagement, miscalculation, or fundamental changes in the external environment. To continue to push a project through to fruition regardless of whether or not it is still viable is obstinacy bordering on foolishness. It is the equivalent to the well-known story of the optimist who, when placed on a large pile of horse manure, began enthusiastically digging in the belief that there must be a pony down there somewhere!

One of the most difficult lessons to learn about managing projects is to recognize the circumstances when, due to impending or inevitable failure, it is no longer sensible to continue them. Making termination decisions is extremely difficult, particularly as it must often be done in the face of resistance from the project manager, team members, and upper management proponents. Their opposition is understandable because by this time they have a personal, ego stake in the project. Consequently, they keep digging, convinced that somewhere under all the detritus of escalating costs, poor performance, and sliding schedules, there must be a pony!

A common mistake when confronted with evidence of impending disaster is to overreact in the belief that throwing more money at a project will somehow "buy" success. Although this response is also understandable, it is an action that should be taken only after considerable thought has gone into it. Our experience here is that unless a project is truly suffering from a dearth of funding, increasing its budget will usually not bring the kinds of returns hoped for. The money will get spent, of course. But the larger question is whether or not the firm will receive due value for the additional monetary support. The answer to this question is much more difficult to assess, but generally our experience has been that the extra money given

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One of the common threads that runs through many of the better-known project failures is the company's unwillingness to back away from a poorly managed development process or product introduction, even when the project manager, team, and top management know the project is in trouble. Staw and Ross (1987) refer to this phenomenon as escalation of commitment to a bad decision. In essence, the theory demonstrates that more often than not, managers do recognize the serious (even fatal) problems that exist in their projects. Nevertheless, there is a strong tendency to follow the prescribed course of action in the face of such failure. Worse, it is common to actually commit more and more resources to a losing hand. Research bears out this point; managers are usually loath to admit to a bad decision and will actually continue to support that course, even in the face of compelling evidence of failure.

One final point: It is important here to distinguish between adding resources to a project that is in trouble and simply reacting in a "knee-jerk" fashion by increasing funding. It is true that, conscientiously applied, additional resources in the form of personnel, support, and money can help a project. This is particularly true in situations in which initial funding was too low, throwing the project's completion into question from the beginning. However, before simply reacting in a panic mode to project troubles, the first step is to conduct a realistic analysis of where the project currently is, how it got there, and how additional funding can bring it back on target. The project manager is the one who needs to sell top management on the need for more funding. That "sale" can only lead to reasonable returns if the request for money and how it will be used productively is well thought out.

When termination decisions have to be made, a willingness to acknowledge an error is required. In place of continued support of failure, even admitting such errors and wiping the slate clean, though financially painful in the short term, is in itself a form of success. AT&T's recent admission that its $7.5 billion investment in acquiring NCR Corporation five years ago was a mistake will, in the long run, work more to its advantage than to its discredit. When similar project problems are apparent and irrecoverable, it is important not to throw good money after bad. When the patient is past revival, acknowledge it, learn the relevant lessons, and move on.

8. Overmanage project managers and their teams.

Large corporations, loaded with layers of oversight and bureaucracy, are increasingly becoming

some of the worst settings for achieving cutting-edge innovation. We see the same phenomenon with so-called "big science" projects, involving hundreds of researchers, multibillion-dollar budgets, and bloated bureaucracies and administration. Is it possible to achieve greatness inside a large corporation? Certainly, but the odds are stacked against you. Consider the example of IBM, which for years regularly devoted 10 percent of its revenues to research and development. In 1989 alone, "Big Blue" spent more than $6.8 billion dollars on R&D—spending by itself the near equivalent of what was spent in all of Japan for that year. And yet, in spite of these huge expenditures, innovations never seemed to find their way to the marketplace. The sheer size and inertia of the organization made it virtually impossible to react quickly or expedite technology transfer to exploit commercial opportunities.

The term "lean and mean" has come into our vocabulary regarding the types of organizations that enjoy better than average success with new product development. The lean and mean organization is one that has not layered itself in the cloak of bureaucracy. It is flexible and has pushed decision-making authority down to lower levels, where project managers can make product development decisions without endless rounds of review and modification. The lean and mean philosophy has the potential to transform the U.S. corporation precisely to the degree that companies practice what they are beginning to preach.

Companies must begin to ask themselves how many internal steps, checks, and balances are involved in bringing new products to market. Do they suffer from excessive bureaucracy—poignantly termed "staff infection" by one interviewee? The answers to these questions will go far toward determining the flexibility and reaction time necessary to bring products to market in an opportunistic fashion.

9. Never, *never* conduct post-failure reviews.

What can we possibly learn from a failed project? We have heard this question voiced many times, usually by managers who are frustrated and/or embarrassed with the leftovers from a failure. The first inclination is to sweep the results under the rug as quietly as possible and then move on as though nothing had happened. Failures are written off as flukes or due to events beyond their control.

We strongly disagree with such an attitude. Mistakes are a natural side effect of many new ventures. Learning from them without an occasional push, however, is a trait that is much more difficult to acquire.

Although the attitude of denial is psychologically appealing, it is the worst attitude one can

possibly take toward business in general and project management in particular. Failure teaches us a number of valuable lessons, provided we can review them objectively and non-defensibly. Some of the most effective heads of project management organizations are those who can painstakingly walk their project managers back through the development process of a failure to see where the wheels fell off the cart. The process should not be accusatory, but instructive. One of the best techniques we have ever witnessed was used by a project director who developed a chronicle of past failures and their causes. He became so attuned to the evidence of potential failure that he was often able to detect problems before they had become apparent to others in his organization.

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Consider the alternative: Ignore the evidence and lessons of past project failures and treat each situation and challenge as though it were unique and not previously understood. The results are predictable—they point to the difference between a manager with ten years' experience and one with one year's experience ten times. Clearly such an attitude cannot be in the best interests of the firm. Nor does it help a project manager's career, particularly in the long run. Learning from mistakes becomes more than simply a personal luxury; it is a duty.

Rita Mae Brown once defined insanity as doing the same old things in the same old way and expecting a different result. If we continue to operate in such a way that we refuse to learn from the past, not only are we condemned to repeat it (to paraphrase Santayana), but we perpetuate a cycle of personal and professional failure. In the end, perhaps that is the true insanity.

10. Never bother to understand project trade-offs.

Like it or not, when managing projects we are often faced with a series of unappetizing alternatives. These trade-offs often come down to a "dollar-day" determination. In other words, to what degree are we willing to sacrifice money in exchange for our schedule, and vice versa? This question points to the nature of project trade-off decisions: They are frequently balancing acts among rival (and seemingly equally compelling) demands. Do we understand the implications of crashing a project? Have we taken the time to

consider the budget impact of such a decision? If the answer to either or both of these questions is no, clearly we are not making decisions on the basis of rational insight. Hard decisions are the perquisite of project management. Uninformed decisions, however, are its bane.

11. Allow political expediency and infighting to dictate crucial project decisions.

It will not surprise most canny readers that many operating decisions are made with less-than-perfect motives, that is, the desire to maximize corporate success and profitability. Unfortunately, in the politicized environment of most firms, any number of potentially momentous decisions are motivated far more by personal agendas than by a desire to satisfy overall corporate needs. Examples of this phenomenon abound. When AT&T determined, following the breakup of its telephone empire in the early 1980s, that it needed to create a new marketing and sales-based mentality, it hired as a corporate vice president a former marketing executive from IBM. Unfortunately, the story only began there. AT&T had always been dominated by its R&D function in strategic decision making, and it was not about to abrogate willingly its preeminent position in the organization. What followed was over a year of active political infighting as R&D sought to subvert any moves by Marketing to alter the culture and focus of AT&T's strategic mission. Finally, the inevitable occurred as the ex-IBM executive resigned his increasingly untenable position.

What is often lost in this story is the central point that, objectively, AT&T *did* need to reshape its strategic focus. It was about to go head-to-head with a number of upstart, long-line competitors such as Sprint and MCI. But R&D saw any strategic shift as threatening its position, so it actively opposed the change, regardless of the negative effect on the overall corporation's profitability. That is the nature of political decision-making—it typically emphasizes parochial needs, even at the expense of overall organizational effectiveness.

Project decisions made on the basis of power plays and maintaining executive prerogatives are bound to be less than effective. In effect, the project becomes a hostage pawn in a larger, more personal game of acquiring and keeping power. Under such circumstances, it is not surprising that excessively political environments have a much more difficult time in successfully developing innovative projects.

12. Make sure the project is run by a weak leader.

The term "weak leader" is oxymoronic; successful leaders exhibit many traits, but a fundamental weakness is not one of them. Leadership is an essential ingredient in project success. To borrow a concept from the physical sciences, projects, if left to their own devices, tend to run toward entropy. In other words, the natural project state is more often chaotic and disordered than logical and pragmatic. In the absence of a strong leader to keep the project team operating on track, most projects begin to experience the vacuum of indecision, orders given and rescinded, and a general sense of aimlessness. Weak leaders are not merely unhelpful to a project's successful completion, they are actively counterproductive. In the entropic state into which a project can easily fall, money and time are wasted and productivity is minimized, all because there is no firm hand at the tiller.

The key is the project leader. This is the one person who has to make the project succeed by marshaling resources, motivating team personnel, negotiating with stakeholders, cheerleading the development process, and constantly keeping an eye on the ultimate prize: the successfully completed project. Naturally, when described in these terms, it is no wonder successful project managers are a special breed, one that needs to be carefully cultivated and guarded within our companies. Their role in successful project development is almost always highly visible. Conversely, in the preponderance of projects that failed, the project manager either was essentially invisible to team members or exhibited the worst sorts of characteristics a project manager can have: weakness and laxity in place of decisiveness and determination.

In the end, what conclusions can we draw from these cases and these guidelines on how to ruin a project? First, that failure is often a by-product of risky ventures. Projects often involve untested or novel technologies and processes. Risk of technical failure is always present in these circumstances. Further, projects upset the status quo of the organization. They operate outside formal channels with temporary groups of diverse individuals pulled together for one purpose, and oftentimes violate political relationships and established chains of command. Given the environment in which many projects operate, it is not surprising that failure is a very real possibility with any project undertaking.

The second conclusion is that past failure need not discourage us from future efforts. Indeed, it is through these past failures that we gain the experience and wisdom to push on toward successful conclusions. There are two equally erroneous responses managers can have toward past failure. The first is to brush it aside with as little thought as possible—in effect, to push it out of sight and out of mind. The other

error is the mirror opposite: to become so focused on past failure that it handcuffs an organization from taking the necessary steps for new ventures and project start-ups. Consequently, these firms suffer from a form of paralysis that precludes quick responses to competitors, to say nothing of their inability to move actively. It is important not to become a victim of past failure, either through a mulish unwillingness to learn from it or through excessive timidity in trying again.

Our goal here is to offer the middle ground. Past examples of famous (and not so famous) project failures give us the opportunity to point to the relevant lessons that can be derived from their study. No project is worth analyzing if it has no object lesson to offer. This article is a collection of object lessons, culled from several different types and several different projects. They all share one common characteristic in that, through some degree of management error, they failed.

This last point is important: There are lessons to be learned from failure, if only we are willing to draw them. The first step is to learn precisely what project "failure" has come to mean. That is the easy part. Far more difficult is taking the next logical step and looking inside for the causes, particularly when those errors bear an uncomfortable similarity to our own past experiences. It is, however, in this honest assessment that we can come closest to deriving the power from this article and its lessons. ❏

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