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Abstract

The discipline of project management adheres to the dominant model of the project life cycle, or the phased stage-gate approach, of executing projects. This implies a clear definition of mission and system at the outset (to reduce uncertainty), and subsequent execution in phases with decision gates. This approach contrasts with the way the seminal projects were conducted that are credited with establishing the foundation of the discipline in the 1950s.

These projects started with missions that were beyond the currently possible, thus any solutions had to emerge over time. They succeeded by a combination of parallel trials (from which the best would then be selected) and trial-and-error iteration (allowing for the modification of solutions pursued over a period of time). Although the success of these approaches was documented and explained by scientific work in the 1950s, today they seem to fly in the face of accepted professional standards, making managers uncomfortable when they encounter them.

The explanation for this contradiction has its roots in the 1960s, when the so-called McNamara revolution at the Department of Defense gave a control orientation to the PM discipline. This shift was cemented by the encoded practices of the DoD and NASA, contemporary scientific writing, and the foundation of the Project Management Institute as a professional organization that translated the standard into the educational norm for a generation of project managers.

The project management discipline was thus relegated to a "grunt work niche" - the engineering execution of moderately novel projects with a clear mission. As a result, it has been prevented from taking center stage in the crucial strategic change initiatives facing many organizations today. This article describes the historical events at the origin of PM's reorientation, arguing that the discipline should be broadened in order to create greater value for organizations whose portfolios include push-the-envelope projects.

1. Introduction

The Project Management Institute, the most influential association governing the professional discipline, defines Project Management (PM) as the application of knowledge, skills, tools, and techniques to project activities in order to meet the "triple constraints" of scope, time and cost. A key concept in managing projects is the "project lifecycle", the phases that projects go through, each having an outcome and end-review that triggers the decision about whether to start the next one. Phase outcomes include, for example, the charter, scope statement, plan, baseline, milestone progress, acceptance, and handover.[i] In brief, Project Management has adopted a phased "stage-gate" approach as the professional standard.

"Modern" Project Management is often said to have begun with the Manhattan Project (to develop the nuclear bomb in the 1940s), and PM techniques to have been developed during the ballistic missile projects (Atlas and Polaris) in the 1950s.[ii] The Manhattan Project "*certainly displayed the principles of organization, planning and direction that typify the modern management of projects.*"[iii] "*The Manhattan Project exhibited the principles of organization, planning, and direction that influenced the development of standard practices for managing projects.*"[iv]

This characterization of the roots of PM represents a certain irony – the Manhattan Project did not even remotely correspond to the "standard practice" associated with PM today, and both the Manhattan and the first ballistic missile projects fundamentally violated the phased project life cycle: both applied a combination of trial-and-error and parallel-trials approaches in order to "stretch the envelope", that is, to achieve outcomes considered impossible at the outset.

However, the Project-Management discipline has now so deeply committed itself to a control-oriented phased approach that the thought of using trial-and-error makes professional managers feel ill at ease. In our seminars, experienced project managers react with distaste to the violation of sound principles of phased control when they are told the real story of the Manhattan Project (or other ambitious and uncertain projects). The discipline seems to have lost its roots of enabling "push the envelope" initiatives, *de facto* focusing on controllable run-of-the-mill projects instead.

How could this happen? And does it matter? In this paper we describe how the discipline lost its roots and we argue that it matters a great deal: it has prevented the project management discipline from taking center stage in the increasingly important efforts of organizations to carry out strategic changes and innovation.

2. The "Roots": Project Management in the 1950s

2.1. The Manhattan Project

A brief review of the history of the Manhattan Project reveals the extent to which it violated the phased stage gate approach.[v] Scientists had been aware since the 1930s that a nuclear fission chain reaction might offer a much greater source of energy than chemical reactions. "*A chain reaction had not been obtained but its possibility – at least in principle – was clear, and several paths that might lead to it had been identified. But the available knowledge was theoretical and very incomplete. (…) The theory was full of unverified assumptions, and calculations were hard to make. Predictions made in 1940 by different physicists of equally high ability were often at variance. The subject was in all too many respects an art, rather than a science.*"[vi]

Scientists and engineers faced two major problems: the production of fissionable materials and the design of the bomb itself. Two fissionable materials could be identified: enriched uranium and the recently (in 1941) discovered plutonium.

For bomb design, multiple ways could be imagined of bringing nuclear fission material together to obtain a critical mass for a self-sustained chain reaction (i.e., an explosion). For example, scientists drew five different designs in a seminar organized by Robert Oppenheimer in July 1942, as shown in Figure 1: from top to bottom, gun-shot, half-sphere, implosion, modified gun-shot, and diffusion designs.

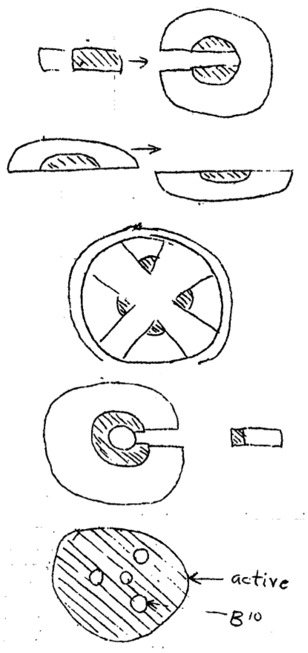

Figure 1: Alternative bomb designs drawn during 1942 Berkeley seminar (from Serber, 1992)

But which one would work and with which material (uranium or plutonium) was entirely unclear. The project manager, General Leslie Groves, stated: "*The whole endeavor was founded on possibilities rather than probabilities. Of theory there was a great deal, of proven knowledge, not much. Basic research had not progressed to the point where work on even the most general design criteria could be started.*"[vii]

The largely inexistent knowledge is illustrated by the following description of a meeting with scientists at the University of Chicago on October, 5, 1942, soon after Groves' nomination as project manager:

"*As the meeting was drawing to a close, I asked the question that is always of uppermost importance in the mind of an engineer: With respect to the amount of fissionable material needed for each bomb, how accurate did they think their estimate was? I expected a reply of 'within 25% or 50%' and would not have been surprised at an even greater percentage, but I was horrified when they quite blandly replied that they thought it was correct within a factor of ten. This meant, for example, that if they estimated that we would need one hundred pounds of plutonium for a bomb, the correct amount could be anywhere from ten to one thousand pounds. Most important of all, it completely destroyed any thought of reasonable planning for the production plants of fissionable materials. My position could well be compared with that of a caterer who is told he must be prepared to serve anywhere between ten and a thousand guests. But after extensive discussion of this point, I concluded that it simply was not possible then to arrive at a more precise answer. While I had known that we were proceeding in the dark, this conversation brought it home to me with the impact of a pile driver. There was simply no ready solution to the problem we faced.*"[viii]

Groves and his steering committee decided to explore and implement different solutions in parallel, both for the production of fissionable materials and for the design of the bomb itself. These principles were put into action as follows (see Figure 2):

- Uranium separation, plutonium production and bomb design proceed concurrently;
- for Uranium separation, two different methods were used in parallel. A third method, thermal diffusion, arose unexpectedly and was added late in the project, in September 1944;
- the Los Alamos laboratory explored several different bomb designs at the same time. The "gun" design (using uranium) was the "lead" first, but in July 1944 they had to switch to the "implosion" design.

– Moreover, they performed the phases of research (to establish working principles) and development of the production plants (to obtain working materials) simultaneously. During the Atlas project ten years later, Bernard Schriever coined the term “concurrency” for this approach: the simultaneous performance of logically sequential tasks. Groves had already used it in previous projects, but this was the first time it was extended to fundamental research.

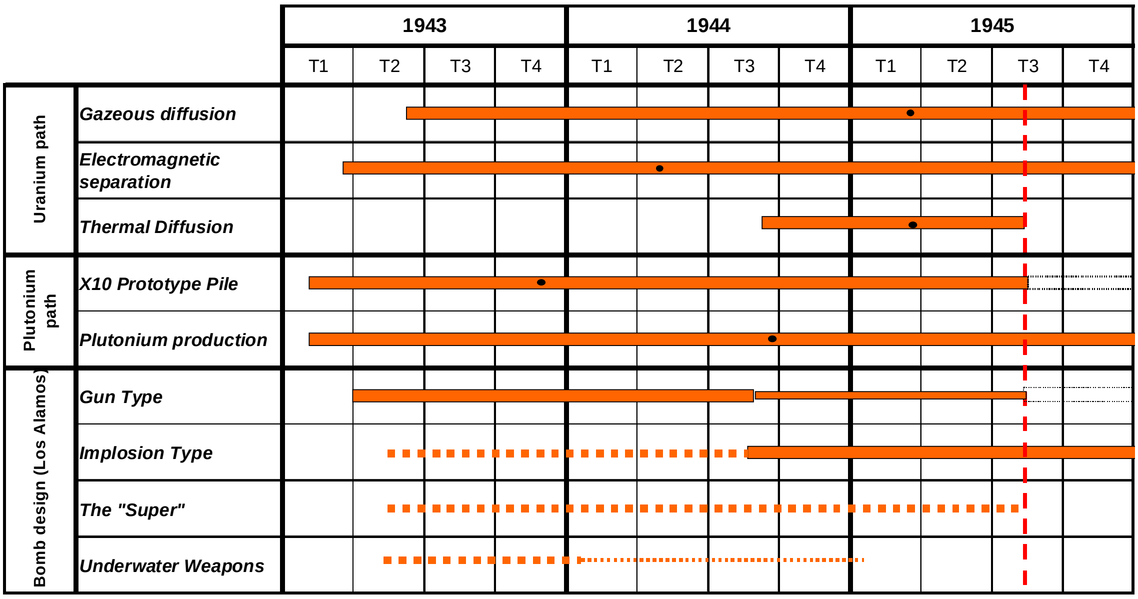

Figure 2: Gantt Chart of the main activities of the Manhattan project

In the face of high technical and scientific uncertainties, the willingness to modify and add solutions mid-course enabled the project to respond to emerging, unforeseen findings. In addition, the parallel pursuit of several alternatives increased the likelihood of success as well as the speed of obtaining a workable solution in the face of a competing effort by Nazi Germany.

Unforeseen findings did arise, as illustrated by the crisis in the spring of 1944. By this date, none of the methods for producing enriched uranium had achieved sufficient accretion rates, and the “gun” design for the bomb was unsuitable for plutonium, which exhibited a much higher spontaneous fission rate than anticipated. The project had maneuvered itself into a dead end, with a fissionable material (plutonium) without a bomb design, and a bomb design (the “gun”) without a workable fissionable material (uranium 235). Now, the flexible and redundant managerial project strategy offered the means to overcome the crisis:

- For the production of fissionable materials, a breakthrough came when it was discovered that a new process, thermal diffusion, could provide slightly enriched uranium, which would then feed the gaseous diffusion and electromagnetic processes for further enrichment. The parallel processes were unexpectedly combined into a composite process that finally achieved the desired performance.

- A second group of scientists had worked on an implosion design as a back up.[ix] When it became clear in the spring of 1944 that the gun approach did not work for plutonium, the implosion design became first priority. Still, unprecedented challenges had to be overcome because the implosion had to be perfectly symmetrical in order to achieve a chain reaction. This demanded mastery of a new uncharted field: hydrodynamics of implosions.

The implosion design using plutonium was frozen in February 1945 and tested in the famous Trinity test, on July 16, 1945. On August 6 and August 9, 1945, the two first nuclear bombs exploded with terrifying impact over Nagasaki and Hiroshima.

In summary, the Manhattan Project exemplifies a willingness to pursue multiple approaches in parallel, although one of them working would suffice to achieve the mission ("Sounds expensive" would be the typical reaction of today's project managers who have grown up with the phased approach). In addition, the project proceeded with trial-and-error, illustrated by Groves' willingness, in the fall of 1943, to throw away two years of work on the gaseous diffusion process in order to test a new unproven, although very promising, approach ("Sounds chaotic and reckless" would be the response of project managers who have grown up with the phased approach).

Indeed, this way of managing a project flies in the face of professional project management principles as they are taught today. But the result was a technical performance that had been thought impossible in 1940 (except by a few theoretical physicists), achieved in less than three years.

2.2. Atlas and Polaris: the first ballistic missiles projects

The development of nuclear weapons and ballistic missiles converged in the cold war of the 1950s. The fear of a 'missile gap' with the USSR, reinforced by the success of Sputnik in October 1957, led to the launch of two ballistic missile projects, Atlas and Polaris, which constituted landmarks in the cold war and in the history of project management.

2.2.1. Atlas / Titan

The Atlas project started in the mid 1950s with the goal of developing an intercontinental ballistic missile (ICBM) capable of delivering a thermonuclear warhead over 5,000 miles with great accuracy. This constituted a huge technical challenge, since rocketry was still largely in its infancy and "light" thermonuclear warheads were not available. The Atlas project, again, violated many rules of modern project management. Indeed its organization and management mainly mirrored the Manhattan Project.[x] Three points are worth noting.

1. A dedicated organization, the Western Development Division of the USAF, was created to overcome the organizational conflicts of interest and divisions among various departments and factions raised by these new weapons.[xi] It was responsible for the entire program and relied on a contractor, the Ramo-Woolridge Corporation, for the management of system integration.
2. Given the huge technical uncertainty, project director Bernard Schriever and his steering committee decided to imitate the Manhattan Project and use a parallel approach. Thus a second missile, Titan, was developed as a backup for Atlas. Two sets of contractors were thus selected to develop two different designs (albeit with compatible interfaces). Beyond the management of technical uncertainty, the goal of having two sets of contractors was also to create a large industrial base and to encourage competition among the contractors.
3. Again, like the Manhattan Project, the Atlas Project was under time pressure and used concurrency, with a major overlap between the subsequent phases of research, development and construction.

This finally led, albeit in fits and starts and with some intermediate failures, to the successful development of the first ICBMs and their deployment in the late 1950s. We will not go into the details here (see endnotes);[xii] what is important for us is that, again, the principles of parallel trials and experimentation were used, violating the phased stage-gate approach.

2.2.2. Polaris

The Polaris project developed the first submarine-launched ballistic missiles (SLBM) carrying nuclear warheads. These offensive weapons, almost impossible to track and attack, became a key element of nuclear deterrence. The Polaris project is today credited with developing the "scientific approach to project management" with the first large-scale application of computerized planning techniques, particularly the critical path method (CPM) and the PERT

(Program Evaluation and Review Technique), a formal planning method with computerized flow charts.

In spite of its reputation for introducing PERT, the Polaris project in reality was much more about strategic choices than about project management techniques.[xiii] The U.S. Navy initiated the project in order to secure resources from the Pentagon, given that the newly created Air Force was appropriating most of the vast resources available for nuclear and strategic defense. A key purpose of the program was to "get a share of the ballistic missile 'pie'":[xiv] Admiral Burke believed that "*The first service that demonstrates a capability for this is very likely to continue the project and others may very well drop out.*"[xv] The result was a clear prioritization of schedule over cost and specifications, and, in addition, a willingness to experiment and change the specifications over the course of the project—we recognize this flexibility from the Manhattan and Atlas projects.

Trial-and-error is illustrated by the fact that the first two deployed versions (in July 1960 and late 1961) of the Polaris missile had only about half the originally desired range (of 1,500 miles) and explosive capacity (of one megaton). The specifications were carefully differentiated from the competing Air Force systems, emphasizing the destruction of urban centers with limited accuracy required—as opposed to the Air Force's goals of destroying firm targets, which required less power but more accuracy.[xvi] The third generation Polaris finally achieved the original requirements, together with submerged launch from the submarine, in 1964.

The planning tool PERT served less for improving project control than for "*offering technological pizzazz that was valuable in selling the program. […] The image of managerial efficiency helped the project. It mattered not whether parts of the system functioned or even existed, it mattered only that certain people for a certain period of time believed that they did*". Project director Raborn organized weekly visits to the Special Projects Office to explain the management of the project to congressmen and businessmen---PERT advertised a managerial innovation with the goal to "*provide resources without interference*".[xvii]

In summary, the operational definitions, priorities, actions, and even "efficiency" itself were repeatedly changed and subordinated to the Navy's strategic organizational goal: securing resources in competition with the Air Force.

2.3. Project Management Theory in the 1950s

Consistent with the Manhattan Project, decision theory in the 1950s advocated parallel trials and experimentation for certain situations,. For example, Alchian and Kessel (1954) argued provocatively that "*resources are not [necessarily] wasted when perfectly sound aircraft are developed and then not procured, in fact, such an outcome is a necessary result of an adequate development program*".[xviii] The reasoning was that no one could know at the outset which design might turn out to be the one with the highest performance. Nelson (1959) quantified the analysis:[xix] R&D projects often suffer from considerable uncertainty with respect to which of several alternatives is best. When the designs are novel, the underlying scientific knowledge poor, and the decision maker is too pressed for time to postpone a decision until more scientific work has been done. The parallel pursuit of several alternatives, although seemingly expensive, is probably cheaper than to end up with an inappropriate system that has to be coaxed into working appropriately.

The advantage of a parallel strategy is not only time but also the information gained from the trials, even if they are abandoned. The result may be a better end result and, in addition, lower cost (which many managers find counter-intuitive) stemming from a better design ultimately chosen.

In addition to parallel trials, the theorists in the 1950s also recognized the need for trial-and-error approaches, changing the project plan mid course. For example, Arrow (1955) made the connection between parallel trials and sequential modifications by arguing that it is unproductive to shoot for an "optimal" design at the outset,[xx] because this optimal design is not known; at best, several alternative scenarios are known, and optimizing for one is likely to be wrong when the uncertainty has settled. Therefore, a "generalist" approach is appropriate at the outset, which is then modified over time,[xxi] or multiple alternative approaches are started, which are then narrowed down as information becomes available.

In summary, at the end of the 1950s, spectacular PM success examples existed that had used parallel trials and flexible trial-and-error approaches. Moreover, a scientific decision-making theory had been developed that could explain why and when these approaches should be used, as opposed to a planned "get it right the first time". But none of this has survived into the professional "bibles" of today; the phased stage-gate approach has been internalized so thoroughly by the profession that any mention of "parallel trials" today is met by incredulous reactions of the "This is unprofessional" type. We now turn to the story of how this happened.

3. From Performance to Control

The view of major projects began to change in the early 1960s. The deployment of the Atlas, Titan and Polaris ballistic missiles diminished the fear of the "missile gap" with the USSR. Therefore, the "national security" projects' sense of utmost urgency faded away.

This trend was expressed and accelerated by the 1960 publication of *The Economics of Defense in the Nuclear Age* by Charles Hitch (who would become comptroller of the Department of Defense, DoD) and R. McKean,[xxii] which introduced a broad audience to a view of defense as an economic problem of resource allocation to achieve a desired objective. This had major consequences for project management: the focus gradually changed from the "performance at all costs" attitude of the first missiles projects to an attitude of optimizing the cost/performance ratio.

This shift in focus also had a political counterpart. The *Defense Reorganization Act* of 1958 greatly increased the power of the Secretary of Defense over the armed services (air force, army and navy). It gave him the authority to "transfer, reassign, abolish or consolidate" service functions, and control over the budget. It also created the post of Director of Research and Engineering in order to control the R&D budget. The goal was to counterbalance the growing power of the project organizations that had been created to manage the major projects of the 1950s (namely, the Ballistic Missile Division of the USAF and the Special Projects Office of the Navy).

The Defense Reorganization Act did not produce major changes immediately, but things changed dramatically with the arrival of Robert McNamara as Secretary of Defense in early

1961. McNamara had been named president of the Ford Corporation in November 1960, the first non-Ford family member in the post. He had earned a Harvard MBA in 1939 and, after a year at Price Waterhouse, served in the Statistical Control office of the Air Force, where he had become known for his analysis of B29 bomber efficiency and effectiveness. He joined Ford in 1946 as manager of planning and financial analysis and then advanced rapidly to top-level management positions.

The US automotive industry in the 1950s enjoyed strongly expanding markets, at home and abroad, and a key success factor was discipline and cost control (as opposed to breakthrough innovation). McNamara was a brilliant analyzer and organizer. This ability he brought to the Pentagon. He started a complete reorganization of the planning process in the DoD. His objective was to consolidate planning and budgeting which hitherto had been two separate processes. Having two processes "*caused huge cost overruns since each service could and did plan for more than could ever be paid for, attempting to secure expanding budgets for current and future years. In the early phases of development, weapons systems cost far less per year than during their future procurement. Thus getting a small appropriation today to develop a much larger system for tomorrow virtually guaranteed a large budget for the future. This was known as the 'foot in the door' strategy*".[xxiii] To solve this problem, McNamara brought in Hitch and his colleagues from the Rand Corporation. They created the famous Program Planning and Budgeting System (PPBS) which emphasized the up- front analysis, planning and control of projects; for example, it required life-cycle cost estimates before the decision to develop a weapon system.

This analysis and planning emphasis clashed head-on with the project management practices of the early missile projects, with their approach of parallel trials, experimentation and modifications in response to emerging events, and concurrency of subsequent steps in order to save time. The PPBS system led to three fundamental changes.

1. It provided "scientific" decision tools, based on the systems analysis approach that was all the rage in the 1950s and 1960s, to evaluate competing programs. In doing so, it favored cost-effectiveness ratios over (technical) performance. This reflected changed priorities at

the national level: from performance at any cost (to beat first the Nazis, then the Soviets) to efficiency and plannability.

2. It centralized defense policy making in the DoD, which experienced dramatic manpower growth between 1960 and 1967.[xxiv] This centralization "*was not in itself something to lament* [since] *there were substantial costs associated with project independence*". However it became a problem when "*the structural changes have eliminated the opportunities for subunit initiatives by centralizing decision making authority and by restricting competition*".[xxv]
3. The first two changes fundamentally affected the way defense projects were managed. They emphasized the complete definition of the system before its development in order to limit uncertainty; lower uncertainty eliminated the need for parallel trials and experimentation. Furthermore, a strict insistence on a phased approach, ending each phase with a review before the next phase could be started, suppressed concurrency with its associated risks of having to "un-do" work because the preceding (but concurrently executed) stage had to make a change. MacNamara considered concurrency as uncontrollable and risky since, with immature technology, design changes might spread throughout the program, causing cost overruns and delays.

The so-called "McNamara revolution" has had a tremendous impact on project management practices and thinking in two forms. First, the phased-planning approach became the project management model of the DoD and the newly formed NASA. Evaluation procedures paid special attention to the *concept formulation and contract definition* phases of the project. This was enforced by the diffusion of managerial tools like PERT. A *NASA/DoD PERT/Cost Guide* was issued in 1962 and became part of the bidding process of both administrations, transforming these tools into *de facto* standards for project management.

Second, starting in 1963, the DoD switched from cost-plus-fixed-fee to incentive contracts that increased the contractors' responsibility in achieving project objectives. This decision was rather controversial as it greatly increased the paperwork and legal disputes around contract definition. Moreover, it shifted the risks associated with innovation to the contractors, which further discouraged the pursuit of "push-the-envelope" domains. It helped to cement the McNamara revolution, which emphasized project plannability and control, and centralized decision making.

This also limited the scope of project management for the coming decades. From now on, strategy was made at the DoD. Project management's role is now to execute given missions - the (strategic) articulation of the mission is outside the scope of the discipline. A project starts with a clearly defined objective in terms of cost quality and delay, and with a tested and solid solution concept. It proceeds in sequential phases that organize the convergence toward the goal. PERT/CPM and cost control tools provide ways to control the unfolding of the process, even in very complex cases. The top management of the organization oversees the entire process.

4. Institutionalization and Reinvention in Different Fields

4.1. Institutionalization of the Phased Approach

If the 1960s defined the form of project management in practice, the discipline was still in its infancy. It lacked a recognizable academic status as a field, and it also lacked professional recognition, since project manager was still a new role.

On the theoretical and academic side, the MacNamara revolution at the DoD had a counterpart in early literature on project management (just as the parallel approach of the 1940s and 1950s had). Notably, *Systems Analysis and Project Management* by Cleland and King became a classic.[xxvi] The book had two parts that corresponded to the two key project phases. The first advocated the power of systems analysis to analyze and understand complex strategic issues (and thus project missions), with PPBS being the most advanced managerial system to date to produce clear project objectives. The second part dealt with project execution and emphasized 1) the need to create a specific project organization to integrate stakeholder contributions, 2) project planning and control using formal methods.

By the early 1970s, the phased approach had already become "natural" and was transferred to the product development field;[xxvii] linear consecutive stages were prescribed. Cooper pulled various stage templates together and subsequently coined the term "stage gate process",[xxviii] which over time became a widely used new product development project template, and shaped the conceptual picture of (new product) projects over two generations.

Linearly executed stages are built on the elimination of uncertainty and a clear mission, and they exclude trial-and-error iteration as well as parallel trials. Similarly, academics stressed the risks of overlapping stages (in other words, of concurrency), showing that increased costs would result from rework,[xxix] a view that continued for over 20 years.[xxx] All this further cemented the phased approach.

On the professional side, the institutionalization process began with the creation of the US Project Management Institute in 1969. The success of Polaris had been an extraordinary advertising campaign for PERT. The following years saw a burst of publications in the popular and academic press[xxxi] and intense promotion of the method by numerous consulting firms. PERT/CPM had become *de rigueur*, viewed as synonymous with success in the management of large projects. The idea of a professional association arose in the tight community of PERT and CPM users in 1967: "*PERT, CPM, and related versions – actually 'network planning and scheduling systems' in general – became the first widely-used management systems beyond accounting that required computers for practical application to reasonably large* projects*. (…) And there were very few widely used* PERT/CPM *software packages in use, so people who were using these packages fairly easily got to know each other*."[xxxii] This is how Russell Archibald, Eric Benett, Jim Snyder, Ned Engman, J. Gordon Davis and Susan Gallagher came in contact and discussed the possibility of creating a professional organization.

The professional trajectory of Russell Archibald was typical of the PMI's founders. Archibald was first introduced to PERT when he worked at Aerojet General on the Polaris project. He quickly became responsible for the implementation of PERT on the thrust vector control system and then took over responsibility for the Polaris project control department. He left Aerojet in 1961 as an established expert to become a successful consultant on PERT and project control. The book *Network-Based Management Systems (PERT/CPM)* summarized his experience in 1967.

Since all its founders were project control experts, it was natural for the PMI to focus on control tools, such as PERT/ CPM. Indeed, it was first envisioned to be a "National CPM Society" before the scope was enlarged to project management in general. "Modern project management" became equated with PERT/CPM after Polaris and the MacNamara revolution, and this remained true for the following two decades.[xxxiii] Control became the keystone of professional project management, to the detriment of organization and strategy issues.[xxxiv]

The creation of the PMI was the last step in a process that started in the early 60's with McNamara and progressively led to the dominant definition of a control-oriented model of project management. In the early 1970s, all elements were in place:

- Phased planning defined the mission (reducing uncertainty) and governed the project evolution; project management tools like PERT/CPM helped to control it.
- NASA and the DoD contributed to making this approach a *de facto* standard by incorporating this model in their bidding process.
- Exemplary cases, such as Polaris and Apollo, served as showcases, demonstrating the power of this approach to manage large-scale and complex R&D projects.
- A professional association, the PMI, widely publicized the phased approach. Using it as the keystone of its certification process, the PMI reinforced it as a standard in the US (and the international) PM communities.

As a result, parallel strategies, experimentation and concurrency disappeared from professional project management for 20 years. Even P. Morris, in his brilliant history of project management, seems to have forgotten the lessons from the 1950s when he described the projects of the late 1960s: *"Several major projects were experiencing traumatic difficulties (Concorde, SST, TAPS…). (…) With regard to the development of Project Management as a discipline, curiously, many of the difficulties that these projects were experiencing were due to issues that* ***PM had not yet addressed formally – notably technical uncertainty and contract strategy.****"*[xxxv] Thus, the knowledge had been lost of the systematical treatment of high uncertainty, practical and theoretical, in the weapons projects and writings of the 1950s.

4.2. Criticism and Reinvention

The McNamara revolution had its critics right from the start. Up-front system definition and strict monitoring in the phased approach led to the creation of a complex system of committees, which some in the DoD viewed as "*creeping centralization*". They saw the phased approach reduce innovation and increase development times.[xxxvi]

Even some of the apostles of phased planning warned against its negative effects. Charles Hitch himself, one of McNamara's key officers in implementing the new policy, identified "common pitfalls" of R&D management in his 1960 landmark book: (1) too little duplication, (2) too little competition, (3) premature, optimistic, and over-detailed advanced system requirements, (4) excessive centralization of decision-making, (5) premature commitment of large funds, and (6) too little emphasis on the early stage of R&D. The first four of these six problems were, ironically, outcomes of the MacNamara revolution that Hitch helped to shape.

Concurrency was the first of the 1950s concepts to be rediscovered in the innovation domain, where the problem of high uncertainty could not be ignored. Concurrency was re-imported from Japan via two landmark articles[xxxvii] after increasingly competitive Japanese car companies, who had never abandoned the concurrency they had inherited from their own aerospace roots, began to threaten US car companies. Clark & Fujimoto (1991) reintroduced concurrency into the US academic mainstream.[xxxviii]

Parallel trials and iterative experimentation have also been rediscovered in the innovation domain, but it has taken longer, well into the 1990s. Experimentation was noticed by innovation researchers, who called it "product morphing", "probe-and-learn" or "agility".[xxxix] Parallel trials were observed, for example, in software development, Toyota's "set-based engineering", or "product churning" among Japanese consumer electronics companies.[xl]

Although observed during the 1990s, iteration and parallel trials were not recognized as fundamental approaches to high project uncertainty until ten years later, when search theory explained why they were required to explore "unknown terrains".[xli] More importantly, iteration and parallel trials have not re-entered the PM discipline as legitimate approaches.

5. How to Increase PM Relevance by Leveraging the Roots

5.1. How the Exclusive Focus on the Phased Approach Limits PM

With their focus on the phased stage-gate approach, the PMI, and even the DoD as a key driver and major customer, have gotten what they asked for. The DoD's preferred approach to systems development is based on a time-phased plan to develop a new system in increments with shorter acquisition cycle times.[xlii] This approach promises greater cost and schedule control but assumes that uncertainty can be limited at the outset; in other words, it requires technical maturity.[xliii]

But this is a fiction. Today's defense projects continue to require leading-edge solutions, which by necessity means uncertainty. It is no wonder that many defense projects experience significant difficulties, many because of an underestimation of uncertainty.[xliv] Of course, uncertainty should be limited wherever possible by using proven components, but defense projects with ambitious performance goals intrinsically necessitate going beyond proven solutions. As the phased approach does not handle novelty and uncertainty well, uncertainty stemming from novelty has been declared non-existent. The phased approach is applied as a catch-all, but as a result its cost and schedule advantages have proved illusory.

It is true that the 1940s tools of parallelism and iteration are still used, as illustrated by a start-up company that introduced a new metal surface-finishing process with the potential to reduce friction between moving parts by up to 30%.[xlv] The start-up heavily used parallel trials—it needed only one market but pursued several in parallel (medical, auto, hydraulics). The company also used experimentation and iteration—as of 2007 they discovered that the underlying mechanism worked differently than they had thought, and thus changed the primary application to solar power plants, where surface treatment of the pipe that transported the heated fluid to the turbine reduced energy losses (due to radiation) by 20%. This new application allowed the company to survive the 2008 economic crisis, breaking even at a low level.

But these actions happen *outside* the discipline of project management. When discussing such examples, professional project managers view them as either "special" (e.g., chaotic start-ups) or simply sloppy ("Why did they not perform better risk planning beforehand?"). Companies that do end up applying iteration and parallel trials feel uncomfortable doing so and feel it violates their professionalism. Such companies apply parallel trials and experimentation *despite* their professional PM training, not because PM training has given them the tools to deal with push-the-envelope projects.

The PM discipline's commitment to the phased approach has caused, first, a focus on *execution only* (stemming from the history of centralization): PM executes decisions that have been taken by top management but does not have a role to play in taking those decisions (although some recent PM writers have called for PM to be aligned with strategy.[xlvi] Second, the phased approach rests on the firm assumption that *uncertainty elimination and control* are feasible (stemming from the history of cost control instituted by the PPBS). Indeed, Russell Archibald recently described the future of PM in a guest editorial as further enhanced information systems and organizational maturity - still emphasizing uncertainty avoidance rather than embracing uncertainty as a source of opportunities.[xlvii]

With its *de facto* self-limitation to a one-size-fits-all methodology to efficiently execute routine initiatives, PM has manoeuvred itself into a "grunt work niche" of bureaucratic work, cutting itself off from two major areas of management that should be within the discipline's scope in light of the roots provided by the Manhattan Project:

Strategy making and strategic search. Strategy is seen today only partially as a planned and deliberate choice of a competitive position, and to a larger degree as an emergent response to chaotic and unpredictable changes in a complex environment.[xlviii] This requires *search* by the organization in addition to planning, and causes strategy to be developed bottom-up as well as top-down; indeed, firms shape their strategy substantially by initiatives that emerge bottom-up and create new capabilities and opportunities.[xlix] A PM discipline that looks not only for alignment (that is, clear specifications that are assured to support strategic goals) but for the ability to develop new strategic opportunities would be able to move into the center of managerial relevance.

Innovation. Highly innovative initiatives do not fit the linear phased approach; they require looping back (iteration) and parallelism, as well as finding ways to explain to stakeholders that the scope and deliverables of a project may change. But the exclusive focus on the phased approach has handicapped the ability of many firms to pursue such innovative, push-the-envelope initiatives. Of course, many firms do not perform novel projects, and there is nothing wrong with that if it fits their strategy. But many firms do, and, among those, some believe that experimentation can be relegated to research ("When the new technology is proven and ready, we'll incorporate it into our market delivery initiatives"). Others think they can use a "stage-gate-light" approach with less precisely defined phases. However, neither enables a company to respond to uncertainty, or even take advantage of it, when unforeseen events come from technology, competition, user changes, regulatory changes, at the same time. Relegating innovation projects to research and "stage gate light" is like "crutches" that make you limp. Just consider the dismal statistics of project failures, most of which are caused not by simple incompetence, but by an inability to be prepared for intrinsic surprises that are part of ambitious projects.[1]

By focusing exclusively on the phased approach, the PM discipline has missed out on these two high-impact areas of management. This is causing double damage: damage to the discipline by relegating it to an engineering-execution niche rather than occupying the influential center stage, and damage to companies because it denies them a powerful weapon in innovating and evolving strategy. Again, companies *are* using trial-and-error and parallel approaches in their novel initiatives because they have no choice, but they do so against their professional PM training rather than being supported by it.

5.2. How to Broaden PM Again

The two missed opportunities for PM described in Section 5.1 require the re-opening the concept of the discipline of PM, going back to the roots of the 1940s, as well as integrating new tools that have since emerged in other adjacent fields of study. Specifically, developing the full potential of project management as a discipline requires: (1) allowing projects to not only execute existing plans but to create novel solutions that modify and improve those plans, and (2) developing a more flexible alternative to the staged product life cycle for novel and innovative projects.

Projects as Strategy Making Tools

Projects do not only execute strategy ("senior management decides, the project manager carries out tasks") but can be used to *make* strategy. Consider the example of the plant manager who saw the age statistics of his plant (which are representative of Western country demographics) and raised the question: "We as a workforce are getting older. Do we have any idea how we are going to maintain productivity?" No one had an answer until two production line managers proposed running a pilot experimenting line with the worker mix forecasted for 2017. Still, no one knew what to do, how best to adapt the line to older employees. They empowered frontline people in the pilot line, who developed (with help from specialists) close to 100 implementable solutions via process changes. After a year, the line achieved the same productivity and quality as lines with younger workers. Frontline people had solved the problem initially raised by the unit head.[li] The project started with a question, multiple parties contributed and created a solution that became part of the corporate production system. This project was far from executing strategy; it created a new strategic solution to a problem that the organization faced.

A recent study showed that in six high-performance manufacturing organizations, on average 50% of strategic improvement projects were generated bottom-up by ideas from operational and frontline employees. These projects addressed not only processes and methods but also the product/market positioning.[lii]

If the project management discipline wants to contribute to the strategic use of improvement projects as outlined above, it must develop expertise and methods for including projects in the strategy process of the organization. Strategy processes connect the business strategy to the operational action plans; they run both ways, top-down and bottom-up. This requires broadening the traditional concepts of a project "mission" and a "specification" from given targets to open problems for which the project proposes solutions..

An alternative process for high innovation projects.

Larger, complex projects with the ambition to contribute to strategy must intrinsically accept a higher level of risk and events that are unforeseeable at the outset (for example, a new technology or a new market)—precisely as in the push-the-envelope situation that the Manhattan Project faced (if not necessarily on the same scale). Such projects are "*experimental learning processes*" or "*arenas for learning*".[liii]

For such projects, project management should develop an alternative process that involves parallel trials and iterative trial-and-error cycles, as has been known (although forgotten) since the Manhattan Project. While the era of national security priorities may perhaps be over, push-the-envelope innovation challenges under time pressure still exist in today's organizations. Of course, no project ever consists only of push-the-envelope activities with high uncertainty; every project has parts that are relatively routine. The project management discipline can contribute to the organization’s ability to carry out novel projects by developing processes that allow targeted flexibility:

1. Diagnose the uncertainty profile of the project; in particular, identify project modules that are subject to looming unforeseeable events. Although the events themselves may be unforeseeable, the areas of the project that are affected by knowledge gaps are often identifiable.[liv]
2. Manage routine project modules with a standard phased approach.
3. Manage highly uncertain project pieces by identifying questions that must be answered in order to reduce uncertainty, then apply parallel trials and iteration: design parallel prototypes or iterative cycles of activities that aim to answer those questions.
4. Put a governance structure in place that empowers the project manager to reassess the situation repeatedly depending on the emerging status.

Such flexible methods already exist as templates; developing them into robust and professionally taught standards would help to bring the project management discipline out of its self-imposed “engineering grunt work niche” into the mainstream of managing strategic initiatives.

The self-restriction of the PM discipline has never been consciously decided by anyone or any decision body; it arose from the historical “accidents” described in this paper. However, the discipline should overcome its self-imposed constraints and remember its roots of “making the impossible happen” from the 1940s. PM has a critical role to play in organizational challenges, especially after the economic crisis of 2008. First prototypes of tools are available that allow PM to start contributing to strategy formulation and improving its record on push-the-envelope initiatives. What is needed is the will of the community to pick up the challenge.

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