Lost Roots: How Project Management Came to Emphasize Control Over Flexibility and Novelty

Sylvain Lenfle (CRG, Ecole Polytechnique) : slenfle@hotmail.com
Christoph Loch (INSEAD) : christoph.loch@insead.edu

Published in *California Management Review*, vol. 53, n°1, pp. 32-55, fall 2010

July 2010

Abstract

The discipline of project management (PM) adheres to the dominant model of the project life cycle or phased stage-gate approach to executing projects. This implies a clear definition of mission and system are given at the outset (to reduce uncertainty), and subsequent execution in phases with decision gates. It contrasts with approach applied in the seminal projects that are credited with establishing the foundation of the discipline in the 1940s and 50s.

Those projects started out with missions that were beyond the currently possible; 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 well documented and explained by scientific study in the 1950s, today they seem to fly in the face of accepted professional standards, making managers uncomfortable when they are encountered.

The explanation for this contradiction has its roots in the 1960s, when the so-called McNamara revolution at the Department of Defense (DoD) gave a control orientation to the PM discipline. This shift was cemented by the codes and practices of the DoD and NASA, contemporary scientific writing, and the foundation of the Project Management Institute, 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 an “order taker 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, and argues that the discipline should be broadened in order to create greater value for organizations whose portfolios include novel and uncertain 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 life cycle" – phases that projects go through, each having an outcome and end-review that triggers a decision about whether to start the next one. Phase outcomes may include the charter, scope statement, plan, baseline, milestone progress, acceptance, and handover.[i] In brief, project management takes the project mission and goals as given and has adopted a phased "stage-gate" approach as the professional standard.

"Modern" project management is often said to have begun with the Manhattan Project, which developed the first atomic 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] It "*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. Indeed, the Manhattan and the first ballistic missile projects fundamentally violated the phased project life cycle approach. Both applied a combination of trial-and-error and parallel trials in order to "push 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 puts professional managers 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 conduct an extensive review of the literature, including our own research over ten years, to explain how the discipline "lost its roots". We argue that this 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 change and innovation. By excavating the roots of the management of innovative projects, we attempt to connect PM to a growing body of work that emphasizes the need for flexible search in innovation and organizational change.[v] We propose that PM has an opportunity regain the central place it should never have lost in the management of strategic initiatives, innovation and change, but that this will require adding more flexible methods to the available toolkit.

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

2.1. The Manhattan Project

Even a brief review of the history of the Manhattan Project reveals the extent to which it violated the phased stage-gate approach.[vi] 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.*"[vii]

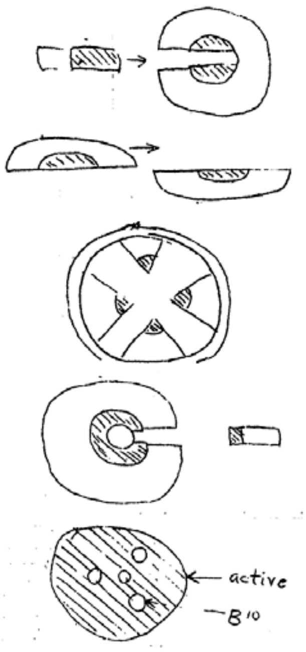

Figure 1: Alternative bomb designs drawn during 1942 Berkeley seminar (from Serber, 1992)

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.

But which one would work and with which material (uranium or plutonium) was entirely unclear, as 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.*"[viii]

Their 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.*"[ix]

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, 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 for plutonium

– Moreover, they performed the phases of research (to establish working principles) and development of the production plants (to obtain working materials) simultaneously. Ten years later, Bernard Schriever called this approach “concurrency”: the simultaneous (or overlapped) 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.

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 events. 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 rival effort by Nazi Germany.

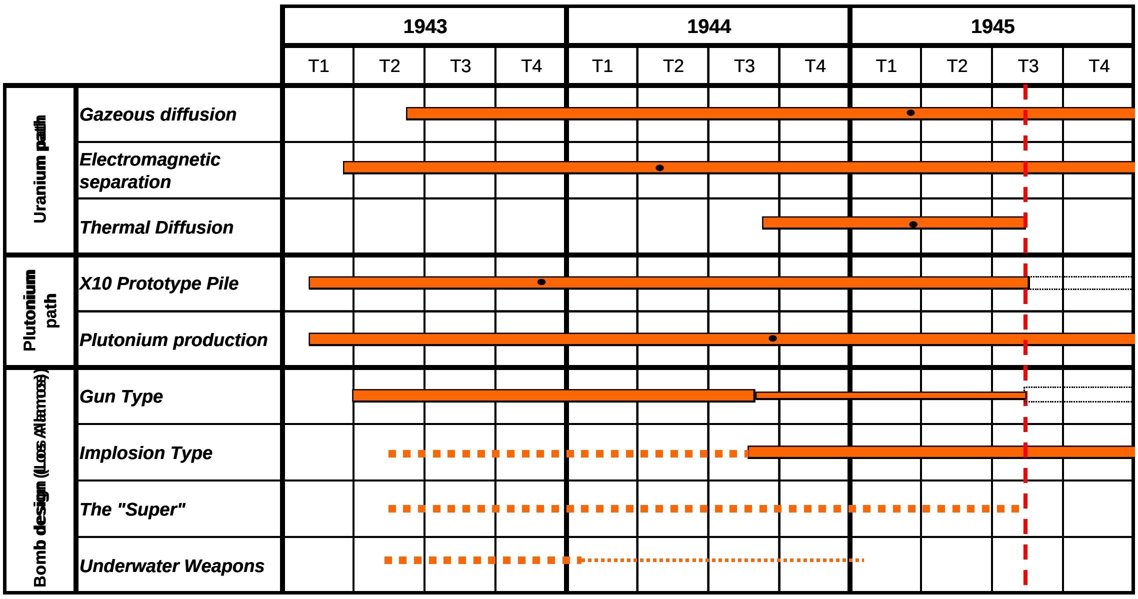

Figure 2: Gantt Chart of the main activities of the Manhattan project (from Lenfle 2008)

Unforeseen events 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). The flexible but redundant managerial project strategy now 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.[x]
- For bomb design, a second group of scientists had worked on an implosion design as a back up.[xi] 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 exemplified a willingness to pursue multiple approaches in parallel, although one of them working would have sufficed 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 discard 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).

This way of managing a project seemingly 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, albeit at the cost of a large budget overrun---the budget was the lowest priority.

2.2. Atlas and Polaris: the First Ballistic Missiles Projects

The development of nuclear weapons and ballistic missiles converged in the Cold War in 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 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.[xii] 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.[xiii] 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);[xiv] what is important for us is that, again, the principles of parallel trials and experimentation were used, contravening 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 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.[xv] 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'":[xvi]

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.*”[xvii] 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 versions deployed (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 military targets, which required less power but more accuracy.[xviii] The third-generation Polaris finally achieved the original requirements, together with submerged launch from a 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*”.[xix]

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 and missile projects, and with several other well-known projects of the period, [xx] decision theory in the 1950s advocated parallel trials and experimentation in 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*".[xxi] The reasoning was that no one could know at the outset which design might turn out to offer the highest performance. Nelson (1959) quantified the analysis:[xxii] R&D projects often suffer from considerable uncertainty with respect to which of several alternatives was best. When the designs are novel, the underlying scientific knowledge poor, and the decision maker 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 the time but also the information gained from the trials, even if they are ultimately abandoned. The result may be a better end result and, in addition, a lower cost (which many managers find counter-intuitive) stemming from the 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 was unproductive to shoot for an "optimal" design at the outset,[xxiii] because this optimal design was not known. At best, several alternative scenarios were known, hence optimizing for one was likely to be wrong when the uncertainty had settled. Therefore, a "generalist" approach was appropriate at the outset which could be modified over time,[xxiv] or multiple alternative approaches started which could be narrowed down as information became available.

In summary, by the end of the 1950s, spectacular examples of PM success 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" approach. However, none of this survived in 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. Thereafter, 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,[xxv] 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 one 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 (the Ballistic Missile Division of the USAF and the Special Projects Office of the Navy) that had managed the major projects of the 1950s.

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 become 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 Office of Statistical Control 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 management.

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, an 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.*"[xxvi] 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, this 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.[xxvii] 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*".[xxviii]

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 to be uncontrollable and risky, since, with immature technology, design changes might spread throughout the program, causing cost overruns and delays.

The so-called "McNamara revolution" 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 fixed-price contracts that increased the contractors' responsibility in achieving project objectives.[xxix] 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 limited the scope of project management in the ensuing decades. From now on, strategy was made at the DoD. Project management's role was henceforth to execute given missions – the (strategic) articulation of the mission was outside the scope of the discipline. A project started with a clearly defined objective in terms of cost quality and delay, and with a tested and solid solution concept. It proceeded in sequential phases that organized the convergence toward the goal. PERT/CPM and cost control tools provided ways to control the unfolding of the process, even in very complex cases. The top management of the organization oversaw the entire process.

4. Institutionalization of the Phased Approach

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

On the theoretical and academic side, the MacNamara revolution at the DoD had a counterpart in the early literature on project management (just as the parallel approach of the 1940s and 1950s had had but with a much wider impact on project management practice). Notably, *Systems Analysis and Project Management* by Cleland and King became a classic,[xxx] consisting of 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;[xxxi] it prescribed linear consecutive stages. Cooper pulled various stage templates together and subsequently coined the term “stage-gate process”,[xxxii] 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 were built on a clear mission and the elimination of uncertainty; trial-and-error iteration as well as parallel trials were excluded. Similarly, academics stressed the risks of overlapping stages (in other words, of concurrency), showing that increased costs would result from rework,[xxxiii] a view that continued for over 20 years.[xxxiv] 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 plethora of publications in the popular and academic press[xxxv] 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-knit 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 p*rojects. *(…) 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.*"[xxxvi] This is how Russell Archibald, Eric Benett, Jim Snyder, Ned Engman, J. Gordon Davis and Susan Gallagher came into 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, summarizing his experience in *Network-Based Management Systems* (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 as 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 next two decades.[xxxvii] Control became the keystone of professional PM, to the detriment of organization, innovation and strategy issues.[xxxviii]

The creation of the PMI was the last step in a process that started in the early 1960s 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.[xxxix]
- 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 PM for 20 years. Even P. Morris, in his brilliant history of project management, seemed to forget 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.****"*[xl] Thus the practical and theoretical knowledge from the projects and writing of the 1950s about how to systematically deal with high uncertainty was lost.

5. Criticism and Reinvention

5.1. The Limits of the Phased Approach: an Example

To illustrate the limitations of a stage-gate process in a novel project, consider the construction of a first-of-its-kind facility for the conversion of iron ore into pure iron in Trinidad in 2000.[xli] After an initial risk analysis, the project was organized into the usual phases of planning, construction, ramp-up and operation. However, the facility represented a scale-up of a factor of

5,000 over the lab concept studies that had established the chemical reduction process. Essentially "the basics of the reaction kinetics were not understood", as the head of R&D commented. But this was not reflected in the project plan; the project manager found himself iteratively working through the process steps (pre-heater, hydrogen atmosphere insertion, first reduction reactor, second reactor, etc) to fix their working configurations, adding modifications that had not been foreseen in the original design nor in the risk management contingencies. Over 18 months, the project manager had to go to the board six times to report facility shut-downs and major changes, rather than the hoped-for ramp-up.

In the end, the project manager was demoted and a new team brought in. This team conducted a comprehensive "progress review", identified 130 "quality problems", and fixed these over another 12 months with a strict phased planning approach. After this period, ramp-up was successful and the facility reached its design capacity. The company concluded that the second team had won the day with better, more disciplined, methods. However, this analysis failed to recognize that the first project manager had made the fundamental design modifications that reduced the uncertainty level and enabled operation at the scale required; the subsequent "rigorous planning" phase succeeded only because this fundamental work had been carried out first. The project was a technical success in the end, but with a schedule delay of two years, and came at the expense of the careers of the first project team.

Generalizing the lessons from this example, the phased approach implicitly rests on two key assumptions, both explicitly desired by the MacNamara revolution: first, the project goals and targets are clear and given from above, and second, the means of reaching the targets are identifiable and plannable (possibly with refinements as the phases progress). But these assumptions are simply not fulfilled in ambitious novel projects or in major strategic initiatives, as we summarize in Table 1.

Table 1: Routine execution versus novel strategic projects[xlii]

	"Routine execution"	**"Novel strategic project"**
Targets	Defined and given from above	General vision and direction, but detailed goals not known and partially emergent
Activities	Can be articulated and derived from experience	Partially emergent
Capabilities	Existent or identified and thus sourceable	Not necessarily existent, not necessarily specifiable
Uncertainty	Variation (plan deviations) and risks (stochastic estimable changes in known project variables)	Unforeseeable uncertainty: new variables, new effects, new actions, which could not be anticipated at the outset
Examples of domains of relevance	• Known markets and customer reactions • Known performance drivers of the developed system • Known environmental parameters	• New markets and unknown customer reactions • New performance drivers of the developed system • Unknown technology • Complexity with unforeseeable interactions among drivers and variables • New geographies with unforeseeable regulatory challenges • New stakeholders with emergent demands

The assumption in the Circored example was that the technology was known except for identified risks; this underestimation of the technical challenge was sufficient to throw the project into turmoil. But long-term strategic initiatives, projects with external effects (and thus conflicting stakeholder views), and projects on novel domains (market or technology) often do not have defined goals[xliii] and need to shape emergent approaches over time.[xliv] The phased approach does not support emergent approaches and emergent goals—although it might do so *in principle* if re-conceptualized (a re-definition of phases becomes in fact an emergent approach), the usually implemented phased approach with its above described history does not.

5.2. Criticism and the Rediscovery of Iteration and Parallel Trials

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 as reducing innovation and increasing development times.[xlv]

Even some of the apostles of the phased approach warned against its negative effects. Charles Hitch himself, one of McNamara's key officers, identified "common pitfalls" of R&D management in his 1960 book mentioned before: (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. The notion of concurrency was re-imported from Japan via two landmark articles[xlvi] after increasingly competitive Japanese car companies, who had never abandoned the concurrency inherited from their own aerospace roots, began to threaten US car companies. Clark & Fujimoto (1991) reintroduced "concurrent engineering" into the US academic mainstream.[xlvii] Indeed, their landmark study of the automotive industry constituted a sharp criticism of the phased approach: they found that it led to communication problems and a need for rework that in turn generated delays, and increased costs and quality problems. Of course, concurrent engineering based on ample communication of changes across the overlapped teams does not solve the problem of project uncertainty—some of the earlier warnings (mentioned in Section 4) were right in that overlapping becomes very expensive and may even slow down a project because of rework, if the project uncertainty is not resolved at the outset.[xlviii] A phased approach with overlapping is not a sufficient answer for the novel projects from Table 1.

Similar criticisms were voiced in the field of software development against the traditional "waterfall model", another application of the phased approach. The emphasis on complete system definition before entering development proved impracticable in innovative projects, leading to potential managerial disasters.[xlix]

Recent theory building supports this criticism:[l] uncertainty causes the project workload to be initially underestimated, while the effective project team size is overestimated because it takes more time than expected to assemble a productive team. The combination of this with tight budgets and schedules in the stage-gate process' feasibility phase sets off a feedback dynamic. Underestimated workload and an understaffed team create schedule pressure; budget constraints limit the possibility to add resources for problem solving, which aggravates schedule pressure, prompts turnover in an already understaffed team, and results in the team missing schedules. This may eventually lead management to conclude that the project is unviable. The strict logic of "time and investment stages each leading to demonstrated progress" must be loosened to allow for iteration and duplication in order to handle unforeseeable events.

Parallel trials and iterative experimentation were also rediscovered in the innovation domain, but took longer – well into the 1990s. Experimentation was revived by innovation researchers, who referred to it using terms such as "product morphing", "probe-and-learn" or "agility".[li] Flexibility includes the possibility of postponing the design freeze, allowing specifications to evolve (within the limits of a modular base) until market uncertainty has been resolved.[lii]

Parallel trials were observed in software development, in Toyota's "set-based engineering", or "product churning" among Japanese consumer electronics companies.[liii] 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". When multiple performance parameters interact, the theory showed that multiple trials simultaneously offer the best hope for finding a satisfactory solution.[liv] However, and more importantly for us, iteration and parallel trials have not re-entered the PM discipline as legitimate approaches.

How do the insights of the last 20 years relate to the case example from Section 5.1? Had the company recognized the level of uncertainty and applied more flexible methods, it would have budgeted for uncertainty reduction in the large-scale facility through iterative testing of the process steps, possibly in parallel, and possibly with simultaneous testing of multiple component candidates to find the one that worked best. This approach might have produced faster results than the de-facto iterations that they ended up doing in spite of the phased approach. Iteration would have accelerated learning and avoided the damage to the project manager's career after management expectations had been disappointed.

6. How to Increase PM Relevance by Leveraging the Roots

6.1. 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.[lv] This approach promises greater cost and schedule control but assumes that uncertainty can be limited at the outset and requires technical maturity.[lvi]

But this seems illusory. Today's defense projects continue to require leading-edge solutions, which often fall into the right-hand column of Table 1 because of technology uncertainty as well as emergent stakeholder issues. It is no wonder that many defense projects experience significant difficulties, many because of an underestimation of uncertainty.[lvii] 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. By the original design of the phased approach ("orders come from the DoD, and uncertainty is eliminated by analysis at the outset"), 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 the following example. A start-up company introduced a new metal surface-finishing process with a potential to reduce friction between moving parts by up to 30%.[lviii] The start-up 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 application allowed the company to survive the 2008 economic crisis, and to break 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., applying only to 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 undermines 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 two key assumptions underlying the phased approach, traceable to the original MacNamara revolution, have directly influenced PM as a discipline: first, a focus on *execution only*: PM executes decisions that have been taken by top management but does not play a role in taking those decisions (although some recent PM writers have called for PM to be aligned with strategy[lix]). Second, the phased approach rests on the firm assumption that *uncertainty elimination and control* are feasible. 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.[lx]

With its *de facto* in-built limitation to efficiently executing routine initiatives (as in the left column of Table 1), PM has confined itself in an "order taker niche" of carrying out tasks given from above, cutting itself off from two major areas of management that should be within the discipline's scope in line with the roots that the Manhattan Project laid down:

Strategy making and strategic search. Strategy is only partially regarded as a planned and deliberate choice of competitive position; to a larger degree it is seen as an emergent response to chaotic and unpredictable changes in a complex environment.[lxi] 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' strategy is substantially shaped by initiatives that emerge from the bottom-up and create new capabilities and opportunities.[lxii] Recent work by leading scholars has indeed proposed to include the definition phase in PM, and to link projects to strategy,[lxiii] but this has been proposed predominantly in a top-down sense, taking the strategy as given and structuring the project as consistent with it.[lxiv] A PM discipline that looks not only for alignment (that is, clear specifications that are certain to support strategic goals) but for the ability to develop new strategic opportunities would be able to move closer to the core 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. 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 them some believe that experimentation can be relegated to research (e.g., "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, still less to take advantage of it, when unforeseen events arise from technology, competition, user and regulatory changes at the same time. Relegating innovation projects to research is like using "crutches" that make you limp. Just consider the dismal statistics of project failures, most of which are caused not by simple incompetence but by not being prepared for the surprises that are intrinsic to ambitious projects.[lxv]

By focusing exclusively on the phased approach, the PM discipline has missed out on these two high-impact areas of management. This does damage at two levels: 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 *do* apply trial-and-error and parallel approaches in their novel projects because they have no choice, but in doing so they go against their professional PM training rather than being supported by it.

6.2. How to Broaden PM Again

Capturing the two missed opportunities for PM described in Section 6.1 requires revisiting the concept of the discipline of PM, going back to the roots of the 1950s as well as integrating new tools that have subsequently emerged in adjacent fields. Specifically, capturing the two opportunities requires: (1) allowing projects to not only execute existing plans and targets 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.

6.2.1. 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 following example of a plant manager who saw the age statistics of his plant (typical of the demographics in any Western country) 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 experimental production line with the worker mix forecasted for 2017. Still, no one knew what to do or how best to adapt the line to older employees. They then 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 staff had solved the problem initially raised by the unit head.[lxvi] The project had started with a question, and multiple parties had contributed to create a solution that became part of the corporate production system. This project was not about executing strategy; it was about creating 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.[lxvii] In this way, projects are vehicles for organizational (strategic) learning.[lxviii] The project challenge lies in between what is wanted and is feasible, but is also an essential source of insight about the strategic challenges of the organization and their solutions – indeed it is a central component of organizational learning.[lxix]

If the project management discipline is 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.

6.2.2. An Expanded Process for Novel and Innovative 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 (see Table 1) — precisely as in the technologically novel situation that the Manhattan Project faced. Such projects are "*experimental learning processes*" or "*arenas for learning*":[lxx] via improvisation and learning-by-doing[lxxi] and targeted experimentation[lxxii] the project generates knowledge about external challenges, emergent system interactions, and the limits of organizational capabilities.

For such projects, processes have been developed to *complement* the phased approach, processes that involve the parallel trials and iterative trial-and-error cycles which have been known about (although forgotten) since the Manhattan Project. While the era of national security priorities may be over, Table 1's highly novel or strategic challenges under time pressure still exist in today's organizations. A key contribution of contemporary research on project management has been to broaden the one-size-fit-all phased approach and propose typologies to distinguish between different types of projects, and the corresponding management practices.[lxxiii] Of course, no project ever consists exclusively 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 in the following ways:

1. In novel projects, targets are not given but come from a broad desired (strategic) direction and a vision, but details are initially hypotheses and may evolve. Thus, an influence on targets needs to be integrated in PM.

2. 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.[lxxiv]
3. Manage routine project modules with a standard phased approach.
4. Manage highly uncertain project pieces by identifying questions that must be answered in order to reduce uncertainty, then apply a combination of parallel trials and iteration: design parallel prototypes or iterative cycles of activities that aim to answer those questions. In other words, the Gantt Chart contains not only activities that produce "progress" toward the end goal, but also activities that answer questions about knowledge gaps or assumptions, with answers that may well force a modification of the initially identified project goal.
5. Put a governance structure in place that empowers the project manager to reassess the situation repeatedly depending on the emerging status.

Such flexible methods are currently emerging but do already exist as templates; one template is shown in Figure 3.[lxxv] Developing such templates into robust and professionally taught standards would help to bring the project management discipline out of its self-imposed "order taker niche" into the mainstream of managing strategic initiatives.

The self-restriction of the PM discipline was never consciously imposed by any decision body; it arose from the series of historical "accidents" described in this paper. However, the discipline should overcome its self-imposed constraints and go back to its roots of "making the impossible happen" from the 1940s. PM has a critical role to play in organizational challenges, particularly in the aftermath of the economic crisis of 2008. First prototypes of tools are already available that could allow PM to contribute to strategy formulation and start improving its record on push-the-envelope initiatives. What is needed now is the will of the community to pick up the challenge.

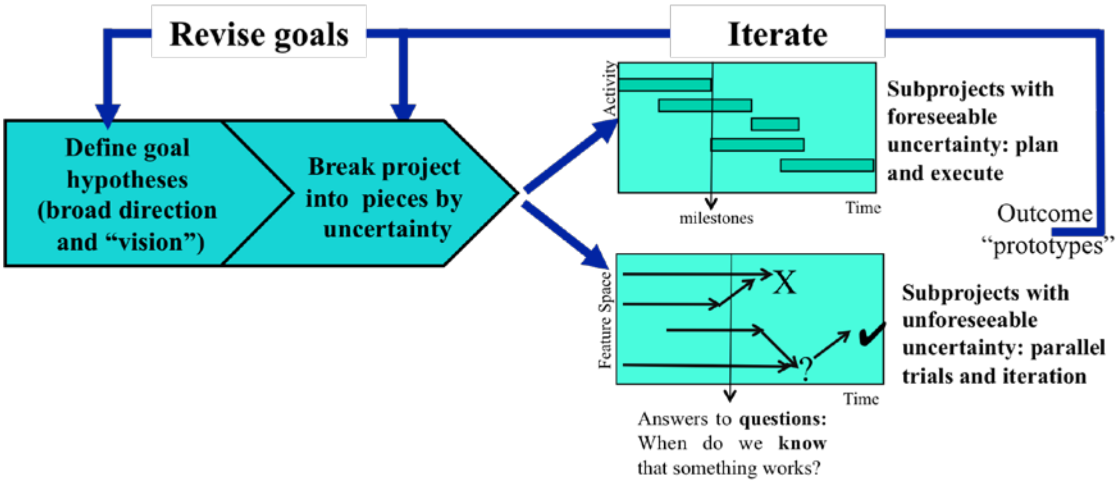

Figure 3: Template of flexible process to complement the phased approach

Endnotes

---

i PMI. 2004. *A Guide to the Project Management Body of Knowledge* (3rd ed.). Project Management Institute: Philadelphia.: 8, 23.

ii See Kerzner H. 2005. *Project Management: A Systems Approach to Planning, Scheduling and Controlling* (9th ed.), Hoboken, NJ: Wiley; Meredith, JR, Mantel SJ. 2003. *Project Management: a Managerial Approach*. Hoboken, NJ: Wiley.

iii Morris, PWG. 1994. *The Management of Projects*. London: Thomas Telford: 18.

iv Shenhar A., Dvir D. 2007. *Reinventing Project Management.* Harvard Bus. School Press, Boston: 8.

v Loch C, DeMeyer A, Pich M. 2006. *Managing the Unknown. A New Approach to Managing High Uncertainty and Risks in Projects*. John Wiley & Sons, Inc.: Hoboken, New Jersey; Lenfle S. 2008. Exploration and Project Management. *International Journal of Project Management* **26**(5), 469-478; Shenhar A, Dvir D. 2007 from endnote *iv*.; O'Reilly III C, Tushman M. 2004. The Ambidextrous Organization. *Harvard Business Review* (62), 74-82.

vi Lenfle S. 2008. Proceeding in the dark. Innovation, project management and the making of the atomic bomb. *CRG Working Paper* (08-001); Lenfle, S. 2009. Managing parallel strategy in projects with unforeseeable uncertainty: the Manhattan case in retrospect. Paper presented at the *European Academy of Management* conference, Roma, 2010.

vii Smyth H. 1945. *Atomic Energy for Military Purposes*. Princeton University Press. Reprinted in *Reviews of Modern Physics* 17 (4), 351-471: 364-365.

viii Groves L. 1962. *Now It Can Be Told. The Story of the Manhattan Project*. Da Capo Press: New-York: 15.

ix Ibid : 40.

x For more details see Lenfle (2010) from endnote vi.

xi A third group led by Edward Teller, with much smaller resources, began work on the "super," that is, a thermonuclear weapon, which was fully developed after the war ended.

xii And this was no accident: Schriever discussed strategy with Groves and Oppenheimer; see Hughes T. 1998. *Rescuing Prometheus*. Vintage Books: New-York.

xiii Beard E. 1976. *Developing the ICBM. A study in bureaucratic politics*. Columbia Univ. Press: NY.

xiv Neufeld J. 1989. *The Development of Ballistic Missiles in the United States Air Force 1945-1960*. Office of Air Force History, US Air Force: Washington D.C.; and Hughes 1998 from endnote xi.

xv Sapolsky H. 1972. *The Polaris System Development*. Cambridge, MA: Harvard University Press.

xvi Spinardi G. 1994. *From Polaris to Trident: The Development of the US Fleet Ballistic Missile Technology*. Cambridge, UK: Cambridge University Press:25.

xvii Ibid : 26.

xviii Ibid : 34.

xix Ibid, 35-36.

[xx] The development of nuclear submarines in the 1950s constitutes another illustration of the use of the parallel approach in innovative military projects. There were two competing technologies (sodium-cooled and water-cooled reactors) proposed by different contractors. Since nobody was able to foresee which would work best, two different reactors were built and tested in submarines, and the best chosen ex-post (the Navy opted for water-cooled reactors in 1958). See Hewlett R, Duncan F. 1974. *Nuclear Navy, 1946-1962*. University of Chicago Press: Chicago.

[xxi] Alchian, AA, RA Kessel. 1954. A proper role of systems analysis. Rand Corp. Doc. D-2057: 16.

[xxii] Nelson, RR. 1959. The economics of parallel R&D efforts: a sequential decision analysis. US Air Force Project Rand Research Memorandum RM-2482, The Rand Corporation. See also Abernathy, W., RS Rosenblom. 1968. Parallel and sequential R&D strategies: application of a simple model. *IEEE Transactions on Engineering Management* EM-15 (1), 2-10.

[xxiii] Arrow K. 1955. Economic aspects of military research and development. Rand Corp. Document D-3142.

[xxiv] Klein B., W Meckling. 1958. Application of Operations Research to development decisions. *Operations Research* 6 (3), 352-363.

[xxv] Hitch C, McKean R. 1960. *The Economics of Defense in the Nuclear Age*. Harvard University Press: Cambridge, MA.

[xxvi] Johnson S. 2000. From Concurrency to Phased Planning: An Episode in the History of Systems Management. In A Hughes, T Hughes (Eds.), *Systems, Experts and Computers. The Systems Approach to Management and Engineering, World War II and After*. MIT Press: Cambridge, MA: 93-112: the citation is on pp. 98-99.

[xxvii] See Sapolsky 1972 from endnote xiii.

[xxviii] Ibid : 203.

[xxix] On this move to incentive-type contracts and the ensuing debates see Morris, 1994 (from endnote iii), p. 58-59.

[xxx] Cleland D, King W. 1968. *Systems Analysis and Project Management*. McGraw-Hill: New York.

[xxxi] Mansfield E. 1972. *Research and Innovation in the Modern Corporation*. Macmillan. See also Albala, A. 1975. Stage approach for the evaluation and selection of R&D projects. *IEEE Transactions on Engineering Management* EM-22 (4).

[xxxii] Cooper, RG. 1983. A process model for industrial new product development. *IEEE Transactions on Engineering Management* EM-30 (1), 2-11. Also Cooper, RG. 1990. Stage-gate systems for managing new products. *Business Horizons* 33(3), 44-54.

[xxxiii] See Mansfield 1972 from endnote xxvii.

[xxxiv] Brown M. 1992. *Flying Blind. The Politics of the U.S. Strategic Bomber Program*. Cornell University Press: Ithaca.

[xxxv] Vazsonyi A. 1970. L'histoire de Grandeur et de la Décadence de la Methode PERT. *Management Science* **16**(8), B449-B455.

[xxxvi] Archibald R. D. 2008. Interview with Russ Archibald. *PM World Today* **10**, 9-11.

[xxxvii] Snyder J. 1987. Modern project management: how did we get there - where do we go? *Project Management Journal* **18**(1): 28-29.

[xxxviii] See Morris, 1994 from endnote iii.

[xxxix] In the Apollo case the phased approach was used but quite late in the project (August 1965) and *after* the main design choices have been frozen. At this date the problem was mainly to control of an extremely complex development process.

[xl] Ibid: 78, emphasis added. Morris does mention a “multiple approach” in the Manhattan project, but without explaining the rationale for parallel trials, nor mentioning the term “parallel”.

[xli] Loch C.H, and C. Terwiesch. 2002. The Circored Project. INSEAD Case Study. See also , Loch et al. 2006 from end note *v*.

[xlii] There is a rich literature on novel projects but, unfortunately, no agreement on the terms used. They are alternatively called breakthrough, disruptive, exploratory, vanguard… What is important to our argument is that, in each cases, neither the goal, nor the means to reach it are clearly defined at the outset. See Loch & al, 2006 from endnote v; Lenfle, 2008 from endnote v; McGrath R, McMillan I. 2009. *Discovery-Driven Growth. A Breakthrough Process to Reduce Risk and Seize Opportunity*. Harvard Business School Press: Cambridge, MA

[xliii] Rittel, H.W.J., M.M. Webber. 1973. Dilemmas in a general theory of planning. *Policy Sciences* 4, 155-169.

[xliv] Miller, R., D. R. Lessard. 2000. *The Strategic Management of Large Scale Engineering Projects*. Cambridge: MIT. See also Loch et al. 2006 (op. cit).

[xlv] Johnson 2000 from endnote xxiii.

[xlvi] Imai, K., I. Nonaka, H. Takeuchi. 1985. Managing the new product development process: how the Japanese companies learn and unlearn. In Clark, KB, RH Hayes, C. Lorenz (eds.): *The Uneasy Alliance*. Harvard Business School Press, Boston. See also Takeuchi H., Nonaka I. 1986. The new product development game. *Harvard Bus. Review* 64, 137-146. The second article is Clark K, Chew B, Fujimoto T. 1987. Product development in the world auto industry. *Brookings Papers on Economic Activity*(3): pp. 729-771.

[xlvii] Clark, KB, Fujimoto T. 1991. *Product Development Performance: Strategy, Organization and Management in the World Automotive Industry*. Harvard Business School Press: Boston.

[xlviii] Empirical evidence for this can be found in Terwiesch, C., and C. H. Loch. Measuring the Effectiveness of Overlapping Development Activities. *Management Science* 45, April 1999, 455 - 465.

[xlix] Boehm B. 1988. A Spiral Model of Software Development and Enhancement. *Computer* **21**(5): pp. 61-72; Brooks F. 1995. *The Mythical Man-Month. Essays on Software Engineering.* (20th Anniversary Edition). Addison-Wesley: Boston; Brooks F. 2010. *The Design of Design. Essays from a Computer Scientist.* Addison-Wesley: Boston, MA

[l] van Oorschot, K., K. Sengupta, H. Akkermans, L. Van Wassenhove. 2010. Get Fat Fast: How to Survive Stage Gates in NPD. Working Paper, Eindhoven Technical University, INSEAD, and University of Tilburg.

[li] Leonard-Barton, D. 1995. *Wellsprings of Knowledge*. Boston: HBS Press; Lynn, GS, JG Morone, AS Paulson. 1996. Marketing and discontinuous innovation: the probe and learn process. *California Management Review* 38(3) 8-37 ; Thomke, S.H. 1997. The role of flexibility in the development of new products: An empirical study. *Research Policy*. 26. 105-119.

[lii] Angelmar, R. 1991. Capital France. INSEAD Case Study. See also Iansiti, M., and A. McCormack. 1996. Living on Internet Time: Product Development at Netscape, Yahoo, NetDynamics and Microsoft. Harvard Business School Case Study. See also Shenhar and Dvir 2007 from endnote iv.

[liii] Beinhocker, ED. 1999. Robust adaptive strategies. *Sloan Management Rev*iew 40(3), 95-106; Sobek, DK, AC Ward, JK Liker. 1999. Toyota’s principles of set-based concurrent engineering. *Sloan Management Review* 40, 67-83; Stalk, G.Jr., A.M. Webber. 1993. Japan’s dark side of time. *Harvard Business Review*. 71(4) 93-102.

[liv] Sommer, SC, CH Loch. 2004. Selectionism and learning in projects with complexity and unforeseeable uncertainty. *Management Science* 50 (10) 1334 – 1347; Sommer, SC, CH Loch, J Dong. 2009. Mastering complexity and unforeseeable uncertainty in startup companies: an empirical study. *Organization Science* 20(1), 118-133.

[lv] Department of Defense, Directive Number 5000.01, Washington, DC, May 12, 2003; Defense Acquisition University, Interim Defense Acquisition Guidebook, https://acc.dau.mil/dag, Fort Belvoir, VA, June 15, 2009.

[lvi] U.S. Government Accountability Office, *Best Practices: Capturing Design and Manufacturing Knowledge Early Improves Acquisition Outcomes*. GAO-02-701, Washington, DC, July 15, 2002.

[lvii] U.S. Government Accountability Office, *Best Practices: Better Matching of Needs and Resources will Lead to Better Weapon System Outcomes*, GAO-01-288, Washington, DC, March 8, 2001.

[lviii] Loch, CH, Zott C, Jokela P, Nahmias D. 2008. FriCSo. INSEAD Case Study.

[lix] Morris, PWG. 2006. Initiation strategies for managing major projects. Chapter 4 in: Dinsmore, PC., J. Cabanis-Brewin (eds.): *The AMA Handbook of Project Management*. New York: AMACOM; Artto K. A., P. H. Dietrich. 2004. Strategic Business Management through Multiple Projects. In: Morris P. W. G. and Pinto J. K. (eds.): *The Wiley Guide to Managing Projects*, John Wiley, London, 144-176; Loch, CH., S. Kavadias. 2010. Implementing Strategy Through Projects. Chapter 8 in: Morris, P., J. Pinto and J. Söderlund (eds.), *The Oxford Handbook on the Management of Projects*. Oxford: Oxford University Press 2010.

[lx] Archibald, R. D. 2009. Five Decades of Modern Project Management: Where It Came From – Where It’s Going. Guest Editorial, *PM World Today*, October, 1-9.

[lxi] Rivkin, JW. 2000. Imitation of complex strategies. *Management Science* 46(6), 824-844; Winter, SG., Cattani G, Dorsch A. 2007. The value of moderate obsession: Insights from a new model of organizational search. *Organization Science* 18(3), 403-419; De Meyer, A., Loch CH. 2007. Technology Strategy. Chapter 2 in: Loch CH., Kavadias S (Eds). *Handbook of New Product Development Management.* Butterworth Heinemann/Elsevier.

[lxii] Burgelman, RA. 1991. Intraorganizational ecology of strategy making and organizational adaptation: theory and field research. *Organization Science* 2 (3), 239-262; Sting, F, Loch CH.

2009. How Top-Down and Bottom-Up Strategy Processes are Combined in Manufacturing Organizations. INSEAD Working Paper, November.

[lxiii] Morris, PWG, JK Pinto. 2004. *The Wiley Guide to Managing Projects*. Hoboken, NJ : John Wiley, p. xix.

[lxiv] Jamieson, A., PWG Morris. 2004. Moving from corporate strategy to project strategy. Chapter 8 in Morris and Pinto, op. cit in endnote lxiii.

[lxv] Morris PWG. 2010. A brief history of project management. Chapter 1 in: Morris PWG, Pinto J, Söderlund J (eds), *Oxford Handbook on the Management of Projects*, Oxford: Oxford University Press.

[lxvi] Loch, CH, Sting F, Bauer N, Mauermann H. 2010. How BMW Diffused the Demographic Time Bomb. *Harvard Business Review*, March, 99-104.

[lxvii] See Sting and Loch (2009) from endnote xlix.

[lxviii] Brady. T., A. Davies. 2004. Building project capabilities: From exploratory to exploitative learning. *Organizational Studies* 25(9), 1601-1621.

[lxix] Nonaka I, Toyama R, Hirata T. 2008. *Managing Flow. A Process Theory of the Knowledge-Based Firm*. Palgrave Macmillan: New-York. Particularly relevant to our topic is their middle-up-down concept to describe the role of middle management in the knowledge creation process.

[lxx] Lundin RA, Midler C. 1998. *Projects as Arenas for Renewal and Learning Processes*. Kluwer Academic Publishers: Dordrecht; Lenfle S. 2008 from endnote v; Loch et al. 2006 from endnote *xli*.

[lxxi] Austin, R., and L. Devin. 2003. *Artful Making: What Managers Need to Know about How Artists Work*. Upper Saddle River, NJ: FT-Prentice Hall.

[lxxii] Thomke, S. 2003. *Experimentation Matters.* Cambridge, MA: Harvard Business School Press. See also Shenhar, AJ, D. Dvir. 2004. How projects differ and what to do about it. In Morris, PWG and JK Pinto (eds.), *The Wiley Guide on Managing Projects*, Hoboken, NJ: John Wiley, p. 1265-1286.

[lxxiii] See, for example, Shenhar & Dvir's Diamond framework (2007 from endnote iv), which classifies project according to four criteria (novelty, complexity, technology and pace) and Loch & al. (2006, from endnote *v*).

[lxxiv] De Meyer A., Loch CH, Pich MT. 2002. Managing project uncertainty: From variation to chaos, *Sloan Management Review* 43 (2) 60-67; Loch, CH, Solt ME, Bailey E. 2008. Diagnosing Unforeseeable Uncertainty in a New Venture. *Journal of Product Innovation Management* 25 (1), 2008, 28-46; McGrath, RG., IC. MacMillan. 2000. *The Entrepreneurial Mindset*. Boston: Harvard Business School Press.

[lxxv] See Loch et al. 2006 (endnote v), the references from endnote *li*, and Runde, J., and A. Feduzi. 2010. Foreseeing the unforeseeable in Project Management? On the Baconian method of eliminative and variative induction. Cambridge University Working Paper.