MANAGING PARALLEL STRATEGY IN PROJECTS WITH UNFORESEEABLE UNCERTAINTY: THE MANHATTAN CASE IN RETROSPECT.

SYLVAIN LENFLE

UNIVERSITY OF CERGY-PONTOISE – MANAGEMENT RESEARCH CENTER, ECOLE POLYTECHNIQUE, FRANCE

Accepted for presentation at the Academy of Management Conference, Chicago 2009

ABSTRACT

This paper discusses the literature on the management of highly innovative projects. Recent work demonstrates that, when confronted with unforeseeable uncertainties, managers can adopt either a learning, trial-and-error-based strategy, or a parallel approach. In the latter, different solutions are developed in parallel and the best one is chosen when enough information becomes available. Studying the case of the Manhattan Project, which historically exemplifies the power of parallel strategy, leads us to show that the either/or logic underlying the existing literature on parallel strategy management oversimplifies the question. The Manhattan case demonstrates that managers do not simply have to choose between options, but can also combine them or add new options during the project.

Keywords: Project Management, Parallel Strategy, Combination, Radical Innovation, Manhattan Project

Parallel: *being everywhere equidistant and not intersecting*

INTRODUCTION[1]

The strategic role of new product development and innovation makes design performance a central concern of managers (Nonaka and Takeuchi, 1986; Wheelwright and Clark, 1992; Brown and Eisenhardt, 1998; Doz and Kosonen, 2008). Project management therefore appears to be an adequate solution to the integration problems raised by those activities. Yet, in line with work on project classification (Wheelwright and Clark, 1992; Balachandra and Friar, 1997; Shenhar and Dvir, 2004 and 2007), we believe that a distinction should be drawn between the various design situations to which different types of projects will be suited. In a previous paper (Lenfle, 2008), we noted the gap between the definition of "project" that highlights novelty, and a mainstream literature which proposes an instrumental view of Project Management (typically the Project Management Institute Body of Knowledge, see Duncan, 1996). Though criticized in recent years,[2] this "rational" view of project management as the accomplishment of a clearly defined goal in a specified period of time, and in conformity with certain budget and quality requirements, remains dominant in most textbooks and discourses on project management. But is that view useful for understanding innovation management? Actually, innovation is first and foremost characterized by divergence and discovery (Van de Ven et al., 1999), as well as by unforeseeable uncertainties which render the rational approach irrelevant (Loch et al., 2006).

Contemporary research therefore argues for an alternative model in which project management is seen first and foremost as an experimental learning process (Loch et al. 2006), as a way to organize and structure exploration and search (Lenfle, 2001; Adler and Obstfeld, 2008). This emerging view raises important managerial questions, particularly how to manage the unknown, how to proceed in the dark (Loch et al. 2006; Lenfle, 2008b). In a situation in which nobody can anticipate how things will develop, the basic scheme of a project involves a Plan / Do / Check / Act cycle "*embedded in a process of a stream of learning events*" (Loch et al. 2006, p. 118). How to conduct the

---

[1] We are especially indebted to C. Loch for many useful suggestions on a previous version of this paper.

[2] See International Journal of Project Management, special issue on *Rethinking Project Management*, 2006, vol. 24, n° 8.

different experiments therefore becomes a central practical and theoretical issue (Loch and al, 1999). Should they be treated sequentially or in parallel?

In this paper we wish to analyze the problems raised by the management of parallel trials in projects with unforeseeable uncertainties. As we shall see, most of the research on this question deals with the choice between sequential or parallel strategy. While unquestionably crucial, such an orientation neglects the other side of the coin, namely the question of deciding how to manage the different trials once a strategy is chosen.

To deal with this issue we will go back to history. One of the most famous cases of a project relying on parallel strategy is the Manhattan Project which, during the Second World War, led to the development of the atomic bomb. This case is worth studying for at least two reasons. First, because the Manhattan Project brought about a major breakthrough in the history of technology. Second, because since early on it has been frequently cited to illustrate the power of the parallel approach (e.g. Nelson, 1959). The Manhattan Project thus constitutes an exemplary case that may offer valuable insights into the management of parallel trials in situations characterized by unforeseeable uncertainties (Yin, 2003).

This paper is organized as follows: We begin by discussing the literature on parallel strategy. Section 2 describes (§3) our methodology. Section 4 presents the history of the Manhattan Project. We thereafter analyze the dynamics of parallel strategy management (§5). Section 6 discusses the technical and organizational problems raised by this strategy. We conclude by pointing out that the dynamics of parallel strategy constitutes an important topic for researchers concerned with product development and innovation management.

LITERATURE REVIEW: PARALLEL STRATEGY IN PROJECT MANAGEMENT

The most important problem faced by managers of highly innovative project is that the entire endeavour is first and foremost characterized by unforeseeable uncertainties or unknown unknowns, i.e. "*the inability to recognize and articulate relevant variables and their functional relationships*" (Sommer and Loch, 2004, p. 1334). This means that the team faces a situation where events can occur that are outside its knowledge, and for which it cannot plan or prepare. In contemporary terms, Project

Risk Management is no longer efficient since nobody can identify and anticipate the risks (see Loch et al., 2006 for an excellent discussion of this question).

Considering these unforeseeable uncertainties, one solution consists of trying different approaches in parallel to find out which one works best. In a late-1960s study of R&D projects, Abernathy and Rosenbloom (1969) defined parallel strategy as "*the simultaneous pursuit of two or more distinct approaches to a single task, when successful completion of any one would satisfy the task requirements"*. They distinguished it from the sequential strategy, defined as the "*commitment to the best evident approach, taking up other possibilities only if the first proves unsuccessful*". As they explained, the benefits of a parallel approach are straightforward, since "*by following more than one approach, the manager avoids the risk inherent in trying to discern a priori which of the several uncertain avenues will prove best. By this means he can obtain information that will permit a better choice among approaches, hedge against the risk of outright failure, and perhaps gain indirect benefits by stimulating competition effort or building a broader technological competence for the organization*" (p. B-486). Abernathy and Rosenbloom identified two different parallel strategies, which are used in different project phases. What they called the *parallel synthesis strategy* is found in the earlier phase, and is aimed at enriching the learning process in the face of important uncertainties. It is a means "*of gaining information and maintaining options so that the best path may be selected for subsequent development*" (p. B-487). In contrast, the *parallel engineering strategy* occurs at a later stage, and its goal is to maximize the probability of success given the known requirements of the project (quality, time, cost). The problem is one of balance between the additional costs of the parallel strategy and the costs of the delay (in spending, reputation and opportunity costs). This is the case studied in their paper.

The roots of the parallel approach can be traced to the work of the RAND Corporation in the 1950s. Analyzing the functioning of the R&D process within the US Air Force, RAND economists criticized its excessively centralized character (see Hounshell, 2000). Given the inherent uncertainty of the R&D process,[3] they plead for a policy "*in which diversity [would be] the optimal principle of choice*" (Alchian and Kessel, 1954) since, in such cases, "*avoidance of duplication [would] not necessarily [be] a virtue*" (Arrow, 1955). Their criticism was targeted at systems analysis, which tried to specify the entire system before the onset of development. The result was the publication of pioneering work in the economics of parallel R&D effort (Klein and Meckling, 1958; Nelson, 1959). In particular Nelson (1959) argued that given the relatively low cost of preliminary studies, "*it may be economical* <u>*not*</u> *to choose one design or contractor for an R&D job on the basis of first estimates which experience has shown to be notoriously unreliable, but rather to initiate parallel-development efforts, cutting down the list of competing projects as estimates improves*" (p. 3-4, emphasis in the text). Nelson rely on the Atom Bomb project that we are going to study to illustrate the strength of such an approach. Nelson's 1959 article was important since it orientated most later research on parallel strategy.

First, Nelson insisted on the fact that parallel strategy is not duplication, since the designs pursued are different. Second, in his framework, as in most works on this topic, the project under consideration concerned a process that from the outset included several options, and in which one option was selected for development only after "*significantly improved estimates*" had been made available (Nelson, 1959). Third, finding how many trials should be run in parallel became the key question. RAND research concluded that the marginal utility of additional trials would decrease rapidly beyond three or four parallel options (Hitch and McKean, 1960). Finally, Nelson insisted that the parallel approach would be most suitable in situations where "*there is not sufficient scientific knowledge for a final choice to be made with any great confidence and the pressure of time is too great to permit alternatives to be tried one after another*" (p. 33). Such emphasis on the role of available time is crucial since "*if time is not of major importance (…) more background research should be undertaken before a development effort is undertaken at all*" (footnote, p. 35).

Contemporary research on the management of projects with unforeseeable uncertainty is rediscovering the power of parallel strategy but, interestingly, without any reference to these early

---

[3] And uncertainty was very great during the 1950's due to the development of new weapons such as Thermonuclear Intercontinental Ballistic Missile.

works. Christoph Loch and his colleagues refer to this strategy as *selectionism* (Pich et al., 2002; Sommer and Loch, 2004; Loch et al., 2006). They develop a framework to help project managers choose among different strategies according to the specificity of their situation. The criteria used are 1) the complexity of the project, and 2) the relative cost of learning and delay as compared to parallel trials. Loch rejoins Nelson's conclusions in demonstrating that the selectionist (i.e. parallel) strategy is best suited when the complexity of the project is high and the costs of delay and learning are high in comparison with the cost of parallel trials. His work thus considerably improves our knowledge concerning the information that is needed to choose between sequential and parallel strategies. Moreover, Loch proposes a distinction between a "pure" parallel strategy (called *Darwinian*), in which different trials are launched on the market, and an *exploratory strategy* closer to Nelson's framework, in which the best options are selected before development.

Nevertheless, like earlier work, Loch's frame of reference remains mostly a static one, and is ultimately aimed at determining ways of choosing between different strategies. At the same time, Loch and his colleagues insist on the need, in parallel strategy,

- to organize communication among the different teams that pursue the parallel alternatives;
- to choose an approach leading to robust results, i.e. results "*that emerge from different trials and hold under a variety of conditions*" (p. 136); since costs are lower at the beginning of a project, the sooner options are selected, the better;
- to draw benefits from non-selected outcomes by exploiting the knowledge they generate.

The goal of such work has been to ensure that resources are committed to the chosen options, i.e. to the ones that emerge as the best in the context of a project and its environment. Loch and his colleagues, however, have not provided a satisfactory illustration of how such management process evolves. We therefore believe that a dynamic theory of parallel trials management is still lacking, and that we must analyze further the problems raised by the selectionist strategy. Indeed, some interesting questions arise: Are the parallel trials independent? Are they given at the beginning? Can they be combined? What are the organizational and managerial consequences of the parallel approach?

METHODOLOGY

What we are looking for is a process theory of parallel strategy management (Bohr, 1983). Longitudinal single case study is here the natural methodology (Yin, 2003). Finding appropriate cases, however, proves to be difficult. Most of the time, R&D projects with a high degree of innovativeness, and thus of uncertainty, are considered confidential, and are therefore closed to both quantitative and qualitative outside assessment. One way to overcome this problem is to go back to history. Surprisingly, such an approach, widely used by historians, sociologists of technology (Hughes, 1983) or economists (e.g. Freeman and Soete, 1997), is rarely used by scholars working on project management or innovation (a notable exception is Hargadon, 2001 and 2003). History, however, constitutes a powerful way to test the relevance of existing theory or to generate insights on contemporary questions (Kieser, 1994)[4].

The Manhattan Project was an obvious candidate for three reasons. First, the making of the atomic bomb unquestionably represents a major breakthrough in the history of technology[5]. It exemplifies the power of "Big Science," the mobilization of important resources (human, financial, industrial) to overcome major scientific and technical problems. As noted by Hoddeson et al. (1993), the managerial practices developed at the Los Alamos Laboratory were widely taken up in the American scientific and industrial community after World War II. Studying how the breakthrough happened may provide insights into innovation management.

Second, the Manhattan Project is the seminal reference in the early literature on parallel strategy in R&D projects. Revisiting this case can thus help to discuss the contributions and limitations of this works.

Third the Manhattan Project occupies a particular place in the literature on project management. It is frequently presented as proof of the power of projects. Gaddis (1959), in a seminal paper, highlighted its incredible success, and Morris (1994) argued that the development of the atomic bomb "*certainly displayed the principles of organization, planning and direction that typify the modern*

---

[4] Siggelkow (2007) develops a similar argument for case research in general, arguing that single case studies can be used both to illustrate the challenges raised by a situation and to discuss and extend existing theory.

management of project." (p. 18). More recently, Shenhar and Dvir (2007) wrote that "*The Manhattan Project exhibited the principles of organization, planning, and direction that influenced the development of standard practices for managing projects*" (p.8). As we shall see, however, a careful analysis of the Project does not confirm these claims. The early literature on parallel strategy in R&D project (Nelson, 1959; Hitch and McKean, 1960) discusses the Manhattan Project as a seminal case that does not fit the traditional view of project management. A closer look reveals that the Project leaders ignored most of the best practices in classical project management. Such a tension between the common perception of the Manhattan case and how the Project really developed is relevant for disentangling the strands that led to the current dominant view of project management, and therefore provides an opportunity to revise the concept of project management itself.

Fortunately for our attempt at understanding the dynamics of parallel strategy management, the Manhattan Project has been extensively studied and its relevance no longer needs to be proved. We were therefore able to draw on a large amount of historical material which, however, has not yet been used to study the management of innovation. We relied both on the "official" history of the project (Smyth, 1945; Hawkins, 1946; Hewlett and Anderson, 1962; Groves, 1962), and on more recent work (especially Rhodes, 1986). We also drew from research focused on a person (Thorpe and Sharpin, 2000; Norris, 2002), a specific part of the Project (Hoddeson and al, 1993; Thayer 1996) or a specific question (Serber, 1992; Malloy, 2008). Given the information available, we consider that the point of "theoretical saturation," which Glaser and Strauss (1967) proposed as criterion to stop collecting data, has been attained. Our analysis may therefore lack empirical originality, but will hopefully triangulate the data in original ways.

HISTORY OF THE MANHATTAN PROJECT

It is impossible, and unnecessary for our purpose, to provide here a complete history of the Manhattan Project (see Hewlett and Anderson, 1962; Rhodes, 1986; Gosling, 1999). We will instead

---

[5] We should add in history in general, since the Manhattan Project changed the course of World War II, leading to the quick surrender of Japan on August 15, 1945. Moreover, it marked the beginning of the Cold War, as well as of the nuclear arm race between the US and the Soviet Union.

summarize the main steps of the project by studying, first, its origins and the technical problems it had to face, and second, how these problems were overcome both at a general and at more micro level[6].

Origins of the project

Together with radar research at MIT, the Manhattan Project was one of the largest constituents of the global mobilization of US science during World War II. The military significance of atomic power was explained by Albert Einstein in his famous letter of August 2, 1939 to the American president Franklin D. Roosevelt. Awareness of the military potential of atomic energy did not immediately lead to the project of developing an atomic weapon, and until the beginning of the War, the question remained mainly an academic research topic. This changed in 1940 with the establishment of the National Defense Research Committee (NDRC), headed by Vannevar Bush, to connect different US research programs in view of a possible war. In July 1941 the United States received the report of the British MAUD (Military Application of Uranium Detonation) Committee, which synthesized British research on nuclear physics, and confirmed the possibility of using nuclear fission to build an atomic bomb. This strengthened American involvement in the field. On December 6 1941 – a day before Pearl Harbour – a Uranium (or S1) committee was created within the newly formed Office of Scientific Research and Development (OSRD) in order to coordinate US efforts on the A Bomb.

Up to that point, research on related issues had been carried out at several universities, including Chicago, Illinois, Columbia, and California, but their efforts remained only loosely coordinated. Things began to change during the summer of 1942 when Bush, who had become director of OSRD, and NDRC head James Bryant Conant (who was also president of Harvard University) decided to involve the Army Corps of Engineers. Colonel James C. Marshall was appointed to oversee the entire program. In July, a seminar was organized at Berkeley by Robert Oppenheimer to discuss possible bomb designs, and in August the project was code-named Manhattan Engineering District (MED). Colonel Marshall, however, did not succeed in accelerating the program. In September, he was replaced by General Leslie Groves, a member of the Army Corps of Engineers, who was a very experienced project manager (Norris, 2002). His appointment marked the take-off of the project,

---

[6] This section draws on Lenfle (2008b), which provides a more detailed description of the case.

which was aimed designing an atomic bomb before Germany and using it to end the war (Groves, 1962). Nevertheless, the ways for pursuing the accomplishment of such objectives were far from obvious given that, as we shall see, the relevant basic research and development had not yet been carried out.

A Scientific and Technical Everest

To understand the difficulties the Manhattan Project had to face we first have to delve a bit into nuclear physics and, second, identify the main design problems raised by the making of an atomic bomb.

Nuclear physics for dummies

The Manhattan Project did not start from scratch. As explained by Princeton physicist Henry DeWolf Smyth in his *Official Report on the Development of the Atomic Bomb under the Auspices of the United States Government*, which was released just after Hiroshima and Nagasaki, "*The principal facts about fission had been discovered and revealed to the scientific world. 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*. (p. 364)". "*All such information*," he added, "*was generally available; but it was very incomplete. There were many gaps and many inaccuracies. The techniques were difficult and the quantities of materials available were often submicroscopic. Although the fundamental principles were clear, the theory was full of unverified assumptions, and calculations were hard to make. Predictions make in 1940 by different physicist of equally high ability were often at variance. The subject was in all too many respects an art, rather than a science* (p. 365).

Scientifically the problem was the following (figure 1). As demonstrated by Lise Meitner and Otto Frisch in 1938, when a neutron hits an atom of uranium, the latter splits into two parts, releasing energy and additional neutrons, which in turn split and bring about a chain reaction. Some of the major scientific challenges were to establish the critical mass of fissionable material needed to start and sustain a chain reaction, and determine the number of neutrons released at each step (the reproduction factor, k), knowing that they can be lost or absorbed by other materials.

INSERT FIGURE 1 ABOUT HERE

The discovery of Meitner and Frisch was a true revolution since "*the newly discovered reaction was ferociously exothermic, output exceeding input by at least five orders of magnitude. Here was a new source of energy like nothing seen before in all the long history of the world*" (R. Rhodes, in Serber, 1992, p. xiii).

From theory to practice…

The first self-sustaining nuclear chain reaction was obtained, by Enrico Fermi and his team at the University of Chicago, in December 1942. Thereafter, the Manhattan Project faced two major problems: the production of fissionable materials and the design of the bomb itself. These problems were aggravated by time pressure. Indeed, the US government feared that Nazi Germany would build the bomb first; in November 1942 already, it had been therefore decided to skip the pilot phase and move directly from research to full-scale production.

The production of fissionable materials

At the beginning of the Project, two materials capable of sustaining a chain reaction were identified. One, uranium 235 is a component of natural uranium (U238), but represents only 0,72% of its mass. The other, plutonium (Pu239), is a by-product of nuclear fission discovered by Glenn T. Seaborg in 1941. In both cases, the production of fissionable materials raised huge scientific and technical problems:

- Separating U235 from U238 involves extremely complex processes, based on the slight differences in the atomic mass of the two isotopes (less than 1%). Seven different separation methods had been identified in 1941; as we shall see, three of them will finally be used (Smyth, 1945).

- Plutonium production involves the design and construction of nuclear reactors and the associated chemical separation plants. Twelve separation processes were studied at the University of Chicago "Met Lab" at the beginning of plant construction.

These were breakthrough innovations. The processes either did not exist before the project (plutonium production) or had never been used with radioactive materials (chemical separation). They entailed extremely tight requirements, and involved radioactive (and therefore very dangerous) materials. Above all, the available knowledge on the production, metallurgy and chemistry of plutonium and uranium separation was far from complete. Thus, commenting on the 1943 Met Lab plutonium research program, Smyth observed that "*Many of the topics listed are not specific research problems such as might be solved by a small team of scientists working for a few months but are whole fields of investigation that might be studied with profit for years.* [So] *it was necessary to pick the specific problems that were likely to give the most immediately useful results but at the same time it was desirable to try to uncover general principles*" (Smyth, 1945).

Alternative bomb designs

The team faced a similar situation regarding the design of an atomic bomb. In a seminar organized at Berkeley by Oppenheimer in July 1942, scientists discussed bomb designs (figure 2 on the right, from Serber, 1992). Several fission bomb assembly possibilities were envisioned: the gun method (at top), the implosion method (center), the autocatalytic method, and others. In the end, only the "gun" method and a more complicated variation of the "implosion" design would be used but, as we shall see, the path toward them was not simple. Furthermore, the Berkeley discussion was theoretical, since so far no prototypes had been built nor experiments undertaken. It remained to be shown, for example, whether a "gun" design worked for uranium and plutonium, or whether an "implosion" device was at all feasible.

INSERT FIGURE 2 ABOUT HERE

Managerial implications

Such a situation had fundamental managerial implications. The most important was that the entire project was first and foremost characterized by unforeseeable uncertainties or unknown unknowns. As Groves (1962, p. 19) wrote, "*the whole endeavour was founded on possibilities rather than probabilities. Of theory there was a great deal, of proven knowledge, not much*". He quickly realized the implications of such a situation. First, he recognized the impossibility of establishing a reliable plan of the project. A "*tentative construction program*" had emerged out of the Berkeley seminar. But "*[i]t soon became apparent that these target dates were wholly unrealistic, for basic research had not yet progressed to the point where work on even the most general design criteria could be started*" (Groves, 1962, p. 15).

In short, the required knowledge was largely non-existent at the beginning of the project. At the end of a meeting with scientists at the University of Chicago on October 5, 1942, soon after his nomination as military director of the Project, Groves: "*asked the question that is always of uppermost 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 twenty-five or fifty percent," 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 on 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"* (Groves, 1962, p. 40). He thus concluded: "*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, except to hope that the factor of error would prove to be not quite so fantastic*" (ibid.).

Managing the unknown[12]: parallel strategy and concurrent engineering

Considering unforeseeable uncertainties, Groves and the Steering Committee (most notably Bush and Conant) decided to explore and implement simultaneously the different solutions, both for the production of fissionable materials and for bomb design. Given the utmost importance of time, the various possibilities would be pursued concurrently; fundamental research on the one hand, and the design and building of the plant would be carried out at the same time. Groves had already used concurrent engineering, but it was the first time such a strategy was extended to fundamental research. As he explained: "*I had decided almost at the very beginning that we have to abandon completely all normal orderly procedures in the development of the production plants. We would go ahead with their design and construction as fast as possible, even though we would have to base our work on the most meager laboratory data* » (Groves, 1962, p. 72). Thayer (1996, p. 42) has shown that DuPont pushed this strategy to "*its ultimate extreme*" in the management of the Hanford Project that led to the production of plutonium. Following Groves's decision, DuPont engineers chose "*to design and build the plant and to develop its unprecedented components and processes in parallel with each other, with the development of supporting science, and with the design and operation of the semiworks* [at Oak Ridge]" (ibid. p. 41). Even if they were already familiar with concurrent engineering, their choice broke with their "*normal, peacetime commercial practice*," which consisted of holding off "*on construction until final design has reached 60% completion*" (ibid)[13]. Shortening project duration was clearly the goal: « *Always we assumed success long before there was any real basis for the assumption; in no other way could we telescope the time required for the over-all project. We could never afford the luxury of awaiting the proof of one step before proceeding with the next* » (ibid. p. 253).

We have used published sources to clarify the meaning of this strategy, emphasizing the development of the Project and its different phases. "SoP" designates "start of production (black points in Figure 4). Figure 3 summarizes the organization of the project and Figure 4 its progression.

---

[12] This title is borrowed from Loch et al. 2006.

[13] See Thayer (1996) for a description of how DuPont engineers built uncertainty into their design of the separation plant.

(A timeline is available in Kelly, 2007; we have completed it with Smyth, 1945; Hewlett and Anderson, 1962; Gosling, 1999; and Rhodes, 1986).

INSERT FIGURE 3 AND 4 ABOUT HERE

The simultaneity of the different tasks is striking:

- Uranium separation, plutonium production and bomb design proceeded concurrently;
- for uranium separation two different methods were used in parallel (electromagnetic separation and gaseous diffusion), and a third one was added in September 1944 (thermal diffusion);
- the Los Alamos laboratory explored several different methods at the same time, initially focusing on the “gun” design, and later (in July 1944) switching to “implosion.”

The rationale behind the parallel strategy was straightforward: given technical and scientific unforeseeable uncertainties, the simultaneous pursuit of different solutions increased the likelihood of success.

Case studies in the management of radical innovation

This overview of the project, while necessary to appreciate the global managerial strategy, is insufficient to grasp the processes at work at a more micro level. Thus to enrich our understanding of the innovative project management at work, we shall examine four well-known and studied aspects of the Project that exemplify the management of radical innovation.

Surprises in the production of fissionable materials

The canning problem in plutonium production.

Together with the “barrier” problem in the gaseous diffusion separation process, the “canning” problem was probably one of the hardest challenges of the Manhattan Project. It arose in the context of plutonium production. The raw material to produce plutonium in a nuclear reactor is uranium, which is used to sustain the nuclear chain reaction. Uranium, however, cannot be employed directly in the pile, but must be "canned" to protect it from the cooling water[15]. The challenge consisted of sealing the uranium slugs into protective metal jackets. This raised huge technical problems and was of crucial importance since the failure of a single can would require the shut-down of an entire pile. DuPont engineers, contractors and scientist explored several methods simultaneously. Their massive effort led to a solution in late August 1944, merely days before the start-up of the first pile (see Hewlett and Anderson, 1962, chap. 6 for a detailed account of the slug crisis).

Barrier design in gaseous diffusion separation.

The Manhattan Project encountered similar difficulties with the gaseous diffusion separation process. This process was based on the theory that "*if uranium was pumped against a porous barrier the lighter molecules of the gas, containing U-235, would pass through more rapidly than the heavier U-238 molecules. The heart of the process was therefore the barrier*" (Groves, quoted in Rhodes, 1986, p. 492). The method was completely novel and the design of the barrier became a real nightmare for the Kellex Corp., which was responsible for the design and construction of K25 (code name of the gaseous separation plant at Oak Ridge). Here again researchers and engineers adopted a parallel strategy. After considerable experimentation they chose, by the end of 1942, the "Norris-Adler" barrier as the most promising solution. But in the fall of 1943 another solution emerged and Groves, as usual, decided to continue with the Norris-Adler design while developing the second-type as insurance against failure. This was a good decision indeed, since the second type proved much more promising than the Norris-Adler design, which encountered huge technical problems. Thus, in a move typical of the Manhattan Project management, Groves ruled "*that two years of work on the barrier be set aside and that the fate of K25, and perhaps the whole project be placed on the mass production (within six months) of millions square feet of a new barrier which had scarcely been tested*" (Hewlett and Anderson, p. 137). In a risky move, it was decided to "*rip out all the carefully designed machinery in the Norris-Adler Plant and install the new process*" (ibid. p. 138); at the time, the British delegation at the plant remarked that "*If the Americans met their schedule, it would be*

---

[15] Or air , as in the case of the X10 prototype reactor at Oak Ridge. The X10 prototype reactor served as a testing ground for the Hanford production reactors, even if they proceed simultaneously and if the cooling technology was not the same.

something of a miraculous achievement" (quoted ibid.). Yet research continued on the Norris-Adler solution, and a suitable barrier was finally developed. Uranium separation by gaseous diffusion at Oak Ridge started on January 20, 1945, almost two years later than planned (see ibid., chap. 5 for a detailed account of the barrier design).

The thermal Diffusion process.

In the Spring of 1944, the recurring problems encountered with gaseous diffusion and electromagnetic separation gave rise to a crisis. At this date none of the initial delivery schedules had been respected, and the Los Alamos laboratory was desperately waiting for samples of both uranium and plutonium to test its bomb designs.

Aware of the research Philip Abelson was conducting by for the Navy on the thermal diffusion separation process, Oppenheimer, director of the Los Alamos Laboratory, suggested to Groves in June 1944 that "*it might be well to consider using the thermal diffusion process a first step aimed only at a slight enrichment, and employing its product as a feed material for our other plants*" (Groves, 1962, p. 120). The leaders of the Manhattan project thus realized that the different process could be combined, instead as viewing them as competing solutions. On this basis, Groves acted very quickly. He appointed a committee to survey Abelson's work and contracted with the engineering firm H. K. Ferguson to build a thermal diffusion plant relying on the K25 power plant for electricity supply. They had 90 days to build "*twenty-one duplicates*" of the Navy experimental plant (Hewlett and Anderson, 1962, p. 296). Production of S50 (code name for the thermal diffusion plant) started in early 1945.

As a consequence, the production process at Oak Ridge was completely reorganized. Following Oppenheimer's suggestion, the production committee began to look for the best way to combine the different processes, knowing that "*they had to include several alternatives since the precise operating characteristic of K25 were not yet known"(*Hewlett and Anderson, 1962, p. 301). As soon as S50 began to produce slightly enriched uranium, they began to use it to feed the electromagnetic separation plant (code-named Y12). They did the same with K25 when its production started in January 1945. In February the committee foresaw that the best combination would be from

S50 to K25 to Y12 – as finally happened in April 1945, when K25 demonstrated its capacity to produce the desired amount of enriched uranium.

The paths to the A Bomb

Alternative Bomb design at Los Alamos

In March 1943, in parallel with work on the production of fissionable materials, building of the Los Alamos Laboratory began on a mesa in San Jose desert, New Mexico. Directed by physicist Robert Oppenheimer, the laboratory was the central node of the Manhattan Project network. Its goal was "*to produce a practical military weapon in the form of a bomb in which energy is released by fast neutron chain reaction in one or more of the materials known to show nuclear fission*" (Serber, 1992, p. 3).

The goal seemed straightforward but, as was the case for the production of fissionable materials, several bomb designs were possible[20]. Since the beginning of project Y at Los Alamos, three of them were under study:

1. The Gun design. This solution built on years of experience. Its principle is apparently simple: a piece of fissionable material is thrown against another piece by means of traditional explosives, thus starting the chain reaction (Figure 5). This design would be used in the "Little Boy" bomb dropped on Hiroshima on August 6, 1945.

INSERT FIGURE 5 ABOUT HERE

2. The Implosion design constituted a breakthrough innovation in weapon design. In this case, conventional explosives are placed around a plutonium core. When they detonate, they are

---

[20] Indeed at the beginning of Los Alamos there was some debates on the type of weapons to be developed. Some argued for an underwater weapon (nuclear depth charge or atomic torpedo) targeted at fleets and harbour. But, given the absence of decision from the government on nuclear targeting and the limited resources available, Oppenheimer quickly decided to focus on a bomb delivered by plane. However, though work on a deliverable underwater weapon appears to have ceased early in 1944, low-level theoretical work on such weapon continued at Los Alamos until at least February 1945. See Malloy (2008, p. 59-60).

blown inward and the core collapses, thus leading to an explosive chain reaction (Figure 6). This design would be used in the "Fat man" bomb dropped on Nagasaki on August 9, 1945.

INSERT FIGURE 6 ABOUT HERE

3. The "Super", suggested by Edward Teller and Enrico Fermi, was another radical innovation. It did not rely on fission, but on nuclear fusion. In this design, a fission bomb helps start a fusion reaction in deuterium or tritium, which would in turn produce a much more powerful explosion than fission bombs. The theoretical foundations of such a weapon, based on the analysis of the functioning of stars, were less solid than those of fission designs.

These alternative paths toward an atomic bomb were granted different priority ranks. Given the state of knowledge on weapons and its supposed robustness, pursuit of the "gun" design was top priority. Even if its use with fissionable materials raised important scientific and engineering questions (e.g., on interior ballistics, the shape of the uranium and plutonium parts, the explosives to be used, or detonation), it was believed it could work with both uranium and plutonium. Since plutonium was less known than uranium, most of the efforts at Los Alamos focused on the plutonium gun. Indeed, a success with plutonium would directly lead to an uranium gun with minor modifications.

Oppenheimer and Groves, however, decided at the outset of project Y that they could not rely on a single approach to bomb design. Uncertainties, particularly those surrounding plutonium, were too important. So, in parallel with the "gun" work, Oppenheimer assigned a small groups of scientist and engineers to work on the implosion design as a second priority. This was a backup for the plutonium gun but, as they soon discovered, implosion could also be a much more efficient assembly method than the crude "gun" design. A third group, smaller and with much lower resources, was assigned to work on the "Super". From the beginning, it was clear to Oppenheimer and his colleagues that the third design was too radical an innovation to be ready during in time. Its potential was nonetheless so high that theoretical work on it was conducted at Los Alamos during the entire project (in part due to E. Teller's obsession with it).

We thus find at Los Alamos the same managerial philosophy of the entire Manhattan Project: given unforeseeable uncertainties one has to study multiple approaches. And this was a good idea since the unforeseeable uncertainties soon arrived.

The spontaneous fission crisis (July 1944)

Indeed one important problems in the plutonium gun design was the instability of this new material. In particular, it exhibited a much higher “spontaneous fission” rate than uranium. This meant that the two parts of the gun had to strike each other at very high speed. Otherwise the chain reaction would start before the two parts collided (and thereby reached the critical mass), and the bomb would “fizzle”. i.e. pre-detonate but not explode.

Although the problem was identified early on, scientists did not master spontaneous fission because plutonium was a completely new material. Measuring and analyzing the phenomenon (under the supervision of Emilio Segrè) was therefore an important part of work at Los Alamos. This was a particularly difficult task since methods and tools had to be developed at a time when plutonium was available only in submicroscopic quantities. The problem turned to a crisis in April 1944, when Los Alamos received from the X10 air-cooled prototype at Oak Ridge the first reactor-produced samples of plutonium. The material exhibited a spontaneous fission rate five times higher than the already available sample, which had been produced by another process at the Berkeley Cyclotron. Research continued until July but the results were desperately the same. The conclusion was clear to Groves, Oppenheimer and their colleagues: the plutonium gun would never work. The entire plutonium path to the bomb (and the millions of dollars already spend) would be lost – and that, at a time when the separation of U235 encountered huge technical difficulties. The entire project therefore found itself in a critical situation.

To overcome the crisis, Oppenheimer completely reorganized the laboratory. In July 1944, the design of the gun was well advanced and, even if engineering questions remained open, under control, at least for uranium (see Hoddeson et al. 1993, p. 411). Furthermore, the research and experiments on implosion had led to important findings (particularly, John Von Neumann’s suggestions during the fall of 1943). In two weeks, Oppenheimer redeployed the resources of Los Alamos so that the entire laboratory would focus on saving the plutonium path. Two new divisions were created with people from the previously existing divisions (Hawkins, 1961, chap. 9; Hoddeson et al. 1993, chap. 14):

- The Gadget (G) Division, headed by Robert Bacher, was to investigate implosion experimentally and eventually design a bomb.
- The Explosives (X) Division, led by George Kistiakowsky, was in charge of designing the high explosives components of the implosion bomb and develop detonation methods.

The goal of the new structure was to enhance coordination among the various part of the program. Several committees were put in place to coordinate the work on implosion.

The technical and scientific challenges were colossal. Even if the research and experiments has produced crucial insights, some were questioning the possibility of an implosion design. Indeed "*implosion moved the Los Alamos scientists onto new terrain. In part, the move was into areas of physics with which they were less familiar: implosion is a problem in hydrodynamics rather than just in nuclear physics*" (MacKenzie and Spinardi, 1994, p. 56). Symmetry posed the hardest problem: to ensure the start of a chain reaction, the inward collapse of the plutonium core had to be absolutely symmetric. This had never been done before and explosives were not designed for such a purpose. Furthermore, since the implosion design would be a breakthrough innovation, the required knowledge was almost non-existent. Los Alamos had to explore simultaneously the hydrodynamics of implosion, the design of the explosives "lens" around the core and the initiator that would release the neutrons needed to start the chain reaction (see figure 7), as well as the electronics to coordinate the detonators around the bomb – while keeping in mind that its goal was to design a working weapon (Hoddeson et al., 1993). For each question the scientist and engineers of the lab used multiple and overlapping approaches to enrich their understanding of the phenomena at work, increase the likelihood of success, and save time. For example, seven experimental tests were used to understand the physics and engineering problems of implosions (Hoddeson et al., 1993). Scientists also relied heavily on small-scale models and numerical analysis (ten IBM calculators were installed at Los Alamos for that purpose).

The herculean scientific and engineering efforts finally led to a radical innovation in weapon design: the implosion bomb. The design was frozen very late, probably on February 28, 1945.

Oppenheimer then created the “cowpuncher committee” to oversee the final phase (Hoddeson et al., 1993, chap. 15 and 16). Yet, the remaining uncertainties concerning the new device were so great on that Groves, finally but reluctantly, approved Oppenheimer’s request to test the bomb, despite the considerable cost of such an experiment. The Trinity test marked the dawn of the nuclear age. On July 16, 1945, the Manhattan Project tested, in a remote area of the New Mexico desert, the implosion bomb. The test was a success. The “gadget”, as it was nicknamed, exploded with an estimated power of 20,000 tons of TNT and the bombing of Hiroshima and Nagasaki followed three weeks later.

ANALYSIS: FROM PARALLEL TO MULTIPLE APPROACHES MANAGEMENT

This summary of the unfolding of the Manhattan Project demonstrates the power of parallel strategy. Given unforeseeable uncertainty and time constraints, a parallel approach greatly improves the likelihood of success. In the making of the atomic bomb all the paths, both for the production of fissionable materials and for bomb design, were highly uncertain. It would have therefore been unreasonable to rely on only one of them. Early on, the Manhattan Project leaders knew that if one discarded from the beginning some of the available methods, “*one may be betting on the slower horse unconsciously*” (Conant to Bush, April 14, 1942, quoted in Nelson, 1959). Groves translated this strategy directly when he explained, in October 1942, that *“a wrong decision that brought quick results was better than no decision at all. If there were a choice between two methods, one of which was good and the other promising, build both. Time was more important than money, and it took times to build plants*” (Hewlett and Anderson, p. 181). As explained by Hoddeson et al. (1993, p. 406), “*the most notable and costly example of multiple approaches was the Pu239 program, created as a backup for U235 production. The decision to create the plutonium program was justified by the complementary uncertainties of producing the two fissionable isotopes – U235 although relatively well known, was difficult to separate chemically from U238, and Pu239, although easy to separate chemically from U238, was almost completely unknown. To save time, the research and production of uranium and plutonium proceed simultaneously*”. Finally, the decision to explore all the way two completely different bomb designs (uranium / gun and plutonium / implosion) explains the success of the project. It should be kept in mind that urgency was the ultimate motivation of the parallel approach. Had there been more time, the Project managers would probably have invested in fundamental research before entering development… and, in all likelihood, the bomb would have not been available before years. This explains why the Manhattan case is the seminal example of the early literature on parallel strategy in R&D projects (e.g. Nelson, 1959).

However we believe that this literature has missed some interesting characteristics of the case. Indeed, the Manhattan Project also highlights the possibility of combining different trials and adding new options <u>during the project</u>. This constitutes a first step in the construction of a dynamic theory of parallel strategy. In order to understand what we mean, we have to distinguish three sequences in the Manhattan Project:

- From its inception in the summer of 1942 until mid-1944, the Project is characterized by a pure parallel strategy. The different paths were considered like competing horses in a race:
    - Plutonium was a backup for uranium 235;
    - for uranium separation, the electromagnetic process, the first and most promising, competed with gaseous diffusion;
    - the implosion design was considered a backup for the much more reliable gun design.

  Such strategy of competition led to a dead end, particularly for the separation of uranium, and gave rise to a major project crisis in the summer of 1944. None of the separation methods reached the expected results, and the gun design proved to be unreliable for plutonium. So the Project had produced a bomb design without fissionable materials on the uranium side, and fissionable material without a bomb design on the plutonium side.
- This crisis opened the way for the second sequence of the Manhattan project, which ended in February 1945 with the design freeze of the implosion bomb and the production committee’s conclusion at Oak Ridge that the best process to enrich uranium would be from S50 to K25 to Y12. The main feature of this sequence was the abandonment of the pure parallel strategy. Indeed, to overcome the crisis, the Manhattan Project leaders, most notably Groves and Oppenheimer, took three major decisions:

1. they decided to add a new separation methods, thermal diffusion, which was considered less promising and had therefore been discarded at the beginning of the atomic efforts;
2. they realized that the combination of the separation methods for U235 was a better strategy than pure competition; thermal and gaseous diffusion were thus used to feed electromagnetic separation;
3. they redeployed Los Alamos resources from the gun, which had by then become merely an engineering problem, to the implosion design, which required fundamental research as well as engineering.

- The last sequence, from February to August 1945, saw increasing pressure to finalize the designs, organize the tests, and build the base on the Pacific island of Tinian from which the American atomic bomb attacks on Japan would be launched. The Los Alamos laboratory thus became more and more structured (*weaponized* is the term used by Thorpe and Shapin, 2000), moving quickly from research to development and production in late 1944 and throughout 1945 (see Hewlett and Anderson, 1962, p. 313-315; Hoddeson et al, 1993 chap 14 to 16). It is remarkable, however, that long-term research in atomic physics and bomb designs was continued throughout this period.

The second step is the most interesting for our topic, since it extends existing literature on parallel strategy. Various authors, from Nelson in 1959 to Loch et al. in recent years, have thought of parallel strategy as an “either/or” choice between options that are given at the beginning of the project. The benefits of the strategy are described as laying in the flexibility offered by the possibility to delay choices until enough information is available, and, to a lesser extent, in the competition between different teams. The problem for managers is thus supposed to reside in the choice between competing solutions, and the main question is whether the choice takes place before development or after market introduction (Loch et al., 2006). Of course, scholars are aware that, when a trial is stopped, managers seek to leverage the benefits of non-selected outcomes by exploiting the knowledge they created and the newly available resources. But the second sequence of the Manhattan Project reveals that the array of options is larger than an either/or choice, and that the parallel approach may not be strictly “parallel”.

Indeed, the summer of 1944 crisis brings to light an infringement of the parallel logic, and the existence of three additional choices in the management of multiple (rather than parallel) approaches.

First, managers could simply pursue the different paths to their completion, but redeploy the resources among them according to their strategic priority or level of advancement. This is what happened when Oppenheimer switched most Los Alamos resources from the gun, which had reached sufficient maturity, to the implosion design. Such strategy allows managers constantly to adapt the intensity of the efforts under way to newly available knowledge, and to face unforeseeable uncertainties.

Secondly – and this is the greatest transgression of parallelism – managers can combine the different options in order to exploit their complementarities, rather than see them as competing solutions. For instance, the decision to "*consider thermal diffusion as a portion of the process as a whole*" and to use it (and later gaseous diffusion) as a way to feed the Y12 plant very probably saved the uranium path to the bomb. It could thus be very profitable to consider the various paths as different faces of the same problem[23]. Thinking of them as complementary could lead to solutions better than those that would be reached by the pure parallel approach.

Finally, the second sequence of the Manhattan Project shows that it is also possible to add new options that have not been selected or even envisioned. In a situation of exploration, it cannot be assumed that every possibility will be identified early on. The essence of unforeseeable uncertainties is to be unforeseeable. Therefore changes in the state of technology, in customers's needs, in competition during the project could make the original options obsolete. It seems very important for managers to keep in mind that they are engaged in a search process, and that the initial plan may not be the good one.

INSERT FIGURE 7 ABOUT HERE

---

[23] This has been identified by D. L. Marples in his classic "The Decisions of Engineering Design" (1961), an article focused on nuclear reactor design. As he explained, the parallel approach "*has other advantages. No one will deny that a problem cannot be fully formulated until it is well on its way to solution. The real difficulty, the nub of a problem lies somewhere amongst the subproblems. (…) The nature of the problem can only be found by examining it through proposed solutions and it seems likely that its examination through one, and only one, proposal gives a very biased view. It seems probable that at least two radically different solutions needs to be*

To summarize, in the evolution of the Manhattan Project, the original parallel strategy depicted in Figure 4 mutateed radically during the summer of 1944 to a combine/switch/add strategy (Figure 7). Such a development demonstrates that the managerial interest of the parallel approach lies not only in the ability to pick ex post facto the best solution, but also in the options it gives managers to redeploy resources, combine the trials, or add new ones according to changing needs. Managing parallel strategy must thus be considered as a dynamic search process within an unknown design space.

DISCUSSION: TECHNICAL AND ORGANIZATIONAL IMPLICATIONS

Some important question on the management of multiples approaches remain nevertheless open. Specifically, we wish to know what are its technical and organizational implications.

The Manhattan case may be illuminating with respect to the technical point of view of the combination option. Indeed, the different processes to enrich uranium were combined. The conditions for doing so were ideal, since each process produced the same output, i.e. U235, with different level of enrichment. One could therefore mix and match them according to need: thermal and gaseous diffusion were used to feed electromagnetic separation, and, later, became steps in the transfer of materials from S50 to K25 to Y12. In contemporary terms, we could say that the interfaces between the different processes were perfectly specified and reduced to their minimum. Except for enriched materials, there was no interaction between the processes. They could then be combined without modifying production. Even though this is not always the case, the combination of multiple options remained a possibility. This raises the question of the architecture of the product / processes under development – a question that has given birth to an important stream of research in management science (Henderson and Clark, 1990; Langlois and Robertson, 1992; Ulrich, 1995; Sanchez and Mahoney, 1996; Baldwin and Clark, 2000). This literature demonstrates that the more modular the product, the easier it is to mix and match components according to the situation. Thus, if we assume that a given project P has adopted a parallel approach for sub-system X for which three solutions X_1, X_2 and X_3 are developed simultaneously, the possibility of combining the three options probably

---

attempted in order to get, through comparison of subproblems, a clear picture of the 'real nature' of the depends on the architecture of X_1, X_2, X_3. If it is integral, then the only option is to choose the best one, since a change in one component leads to a complete redesign of the sub-system, or to pursuing all options concurrently. In the Manhattan Project, the latter was the case with the uranium and plutonium paths. Nevertheless, if the system is even partially modular, with design rules specifying some interfaces between components (but not necessarily all), then some type of combination is possible[26]. This issue, at the intersection of design theory and project management, deserves further research.

From the organizational point of view, the dynamic management of multiple approaches also raises important challenges.

At the most general level, the existence of a powerful management structure in charge of the entire project is a condition sine qua non. Somebody must be able to supervise the entire project, i.e. define the different options, understand their relationships (and therefore their architecture), evaluate their progression, and make decisions according to the situation (stop, keep both, combine, redeploy resources, or add a new option). In our case this was the role of the project leaders, first and foremost Leslie Groves and Robert Oppenheimer, who where among the few to have a global view of the entire effort, understand the processes at work and, last but not least, benefit from full US government support.

But at a more micro level, the analysis of the cases reveals the difficulty of the task. Los Alamos is an extraordinary place to study the question of multiple approaches management since it was the final step of the entire endeavour. It received materials from the different sites, put them in the required form (both in metallurgy and geometry), designed and produced the bombs. The leaders constantly had to keep the different options in mind. A letter from Oppenheimer to Groves shows the extent to which the task was organizationally demanding. At the height of the spontaneous fission crisis, Oppenheimer argued for not abandoning the plutonium gun completely. As he explained, the

---

problem" (Marples, 1961, p. 64).

[26] In this framework, the Manhattan Project is again a special case from the point of view of production processes. But the literature deals mainly with product. As shown by Fujimoto & al. (2006), when dealing with

results on spontaneous fission “*are new and since there is a possibility that the interpretation place on them may not be completely correct, it was agreed that although the discontinuance of the purification and neutron-less assembly program* [part of the plutonium gun program] *should be started immediately, it should be so conducted that at any time within the next month a return to these programs can be made without loss of more than a month’s time. In particular, no essential personnel or installations should be permanently lost to the project within that period*” (Oppenheimer to Grove, July 18, 1944; quoted in Hoddeson et al., p. 243).

Oppenheimer’s position exemplifies the organizational problems raised by the existence of “unk unks”: it is never possible to be in advance certain that an option is the good one. Thus, one must stay ready to organize the transfer of people and knowledge from one option to another. Resource fluidity (Doz and Kosonen, 2008) plays here a fundamental role. It gives managers the ability to move quickly among options. The corollary and challenge of such an approach is the absence of a stable structure. Los Alamos, indeed, was an organization “*whose structure was by nature ephemeral; experiments and responsibilities changed overnight as priorities that the war gave to the project changes*” (Hoddeson and al, 1993, p. 247). As we have seen, the laboratory experienced a sequence of reorganizations during the War, moving from an academic-like institution to a huge scientific-industrial complex employing at its peak almost 9000 persons.

The *normative uncertainty* (Thorpe and Shapin, 2000) that ruled Los Alamos was nonetheless balanced by a few rigid rules and focal points. Robert Oppenheimer was one of them. Victor Weisskopf (1967) remembered by, “*it was his continuous and intense presence, which produced a sense of direct participation in all of us; it created that unique atmosphere of enthusiasm and challenge that pervaded the place throughout its time*”. The colloquium, a weekly plenary meeting, was another powerful means to help laboratory members understand the meaning of their work, and set the direction and pace of action. As explained by Thorpe and Shapin (2000, p. 570 and 572), “*The Colloquium, more than any other local organizational form, was understood both to express and to enable solidarity and integration. Los Alamos scientists were, almost without exception, highly*

---

processes, the question is whether it is possible to specify interfaces between the different steps in order to have an input / output relationship where the input is completely specified.

concerned that each should have an overall sense of how their specialized work fitted into the specialized work being done by others, and into the instrumental goals of the laboratory as a whole. Information, they reckoned, should circulate throughout the laboratory as efficiently as practicable. (…) The solution was simply to provide for more face-to-face and free interaction, to encourage meetings and discussions at as many levels as possible and among as many specialized work groups as possible. This is how and why the weekly Colloquium for all staff members assumed such importance. The Colloquium was considered important as a means of disseminating information, and also as a way of creating solidarity and face-to-face accountability (…) General Groves agreed that the Colloquium 'existed not so much to provide information as to maintain morale and a feeling of common purpose and responsibility'.” Los Alamos thus provides a most valuable large-scale case for the analysis of innovation management in highly dynamic environments characterized by the need to balance structure and flexibility in design processes confronted with unforeseeable uncertainties (Cusumano and Selby, 1995; Brown and Eisenhardt, 1997).

CONCLUSION

The study of the Manhattan Project helps us advance our knowledge on parallel strategy in projects with unforeseeable uncertainty. Our analysis shows that the managerial interest of the parallel approach lies not only in the ability it affords to pick the best solution once enough information is available, but also in the possibilities it opens for redeploying resources, combining the trials or adding new ones according to the unfolding of the project. We have discussed the technical and organizational implications of a dynamic search process into an unknown design space. In so doing, we have tried to complete the growing body of research on the management of highly innovative projects, specifically the framework proposed by Loch et al. (2006).

There was of course some specificity of the case under study. Given the wartime context, speed was of the essence. The Manhattan Project benefited from the full support of President Franklin D. Roosevelt and his administration, and this ensured almost unlimited resources, as well as the mobilisation of the entire US industry (DuPont, Union Carbide, General Electric, Chrysler, Westinghouse, Tennessee Eastman and many others) and science (E. Fermi, J. Franck, E. Lawrence,

A. Compton, J. Chadwick, N. Bohr, E. Wigner, H. Urey were all Nobel Prize winners). However, we believe that the wartime circumstances do not constitute a limitation of our findings. Of course the availability of resources made it easier to implement a massive parallel strategy. But it also gave Manhattan Project managers the freedom to use a wide variety of strategies, thus providing the opportunity to study the complexity of the dynamic process of managing multiple approaches. Moreover, both the problem of managing unforeseeable uncertainty, and multiple strategy as one of the solutions, are in fact generic. The technical and organizational questions they raise deserve further research, particularly in order to analyze and formalize the conditions that allow the combination of options.

At a more general level, we have tried to contribute to a renewal of research on project management. The Manhattan Project foreshadows fundamental practices aimed at managing what we call “exploration projects” (Lenfle, 2008). In such a perspective, projects are a way of organizing the exploration of emerging innovation fields. But engaging in exploration entails a fundamental shift in project management methodology. As shown by our work and other recent research (Loch et al., 2006; Shenhar and Dvir, 2007), exploration situations do not allow for an ex-ante definition of the goal and the means to reach it. Projects themselves thus became highly uncertain and reflexive probe and learn processes. They became a fundamental component of *search* processes (Adler and Obstfeld, 2007). We should therefore revisit our notions about the very nature of projects, which are not only sets of management tools, but also, more generally, a way to construct the future and break with past routines (Adler and Obstfeld, 2007; Emirbayer and Mische, 1998). We thus hope that our historical detour through the Manhattan Project may help build an alternative model of project management.

REFERENCES

On the Manhattan Project

- Gosling F. 1999. The Manhattan Project. US Department of Energy (DOE/MA-0001 - 01/99)
- Groves L. 1962. *Now It Can Be Told. The Story of the Manhattan Project*. Da Capo Press: New-York
- Hawkins D. 1961. Manhattan District History. Project Y, the Los Alamos Project. Vol. I: Inception until August 1945. Los Alamos National Laboratory
- Hewlett R, Anderson O. 1962. *The New World, 1939-1946. Volume I of a History of the United States Atomic Energy Commission.* The Pennsylvania State University Press: University Park, PA
- Hoddeson L, Henriksen P, Meade R, Westfall C. 1993. *Critical Assembly. A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945*. Cambridge University Press: New-York
- Kelly C. 2007. *The Manhattan Project*. Black Dog & Leventhal: New-York
- MacKenzie D, Spinardi G. 1995. Tacit Knowledge, Weapons Design, and the Uninvention of Nuclear Weapons. *The American Journal of Sociology* **101**(1): pp. 44-99
- Malloy S. 2008. *Atomic Tragedy. Henry L. Stimson and the Decision to Use the Bomb Against Japan*. Cornell University Press: New-York
- Norris R. 2002. *Racing for the Bomb. General Leslie R. Groves, The Manhattan Project's Indispensable Man.* Steerforth Press: South Royalton, Vermont
- Rhodes R. 1986. *The Making of the Atomic Bomb*. Simon & Schuster: New-York
- Serber R. 1992. *The Los Alamos Primer. The First Lectures on How to Build an Atomic Bomb.* University of California Press: Berkeley
- Smyth H. 1945. *Atomic Energy for Military Purposes*. Princeton University Press. Reprinted in Reviews of Modern Physics, vol. 17 n°4, pp. 351-471: Princeton
- Thayer H. 1996. *Management of the Hanford Engineer Works in World War II. How the Corps, DuPont and the Metallurgical Laboratory fast tracked the original plutonium works.* American Society of Civil Engineers Press: New-York

- Thorpe C, Shapin S. 2000. Who Was J. Robert Oppenheimer? Charisma and Complex Organization. *Social Studies of Science* **30**(4): pp. 545-590
- Weisskopf V. 1967. A memorial to Oppenheimer. *Physics Today* **20**(10)

Other references

- Abernathy W, Rosenbloom R. 1969. Parallel Strategies in Development Projects. *Management Science* **15**(10): pp. B486-B505
- Adler P, Obstfeld M. 2007. The role of affect in creative projects and exploratory search. *Industrial and Corporate Change* **16**(1): pp. 19-50
- Arrow K. 1955. Economic Aspects of Military Research and Development. D-3142. RAND Corporation
- Balachandra R, Friar J. 1997. Factors of Success in R&D projects and New Product Innovation: A Contextual Approach. *IEEE Transactions on Engineering Management* **44**(3): pp. 276-287
- Baldwin C, Clark K. 2000. *Design rules: the power of modularity*. The MIT Press: Cambridge, MA
- Brown SL, Eisenhardt KM. 1997. The art of continuous change: linking complexity theory and time-paced evolution in relentlessly shifting organizations. *Administrative Science Quarterly* **42**(1): pp. 1-34
- Brown SL, Eisenhardt KM. 1998. *Competing on the edge. Strategy as structured chaos*. Harvard Business School Press: Boston, MA
- Cicmil S, Williams T, Thomas J, Hodgson D. 2006. Rethinking Project Management: Researching the actuality of projects. *International Journal of Project Management* **24**(8): pp. 675-686
- Cusumano M, Selby R. 1995. *Microsoft Secrets*. The Free Press: New-York
- Doz Y, Kosonen M. 2008. *Fast Strategy*. Wharton School Publishing: Harlow
- Duncan W. 1996. *A Guide to the Project Management Body of Knowledge*. PMI Publishing Division
- Eisenhardt KM. 1989. Building theories from case study research. *Academy of Management Review* **14**(4): 532-550

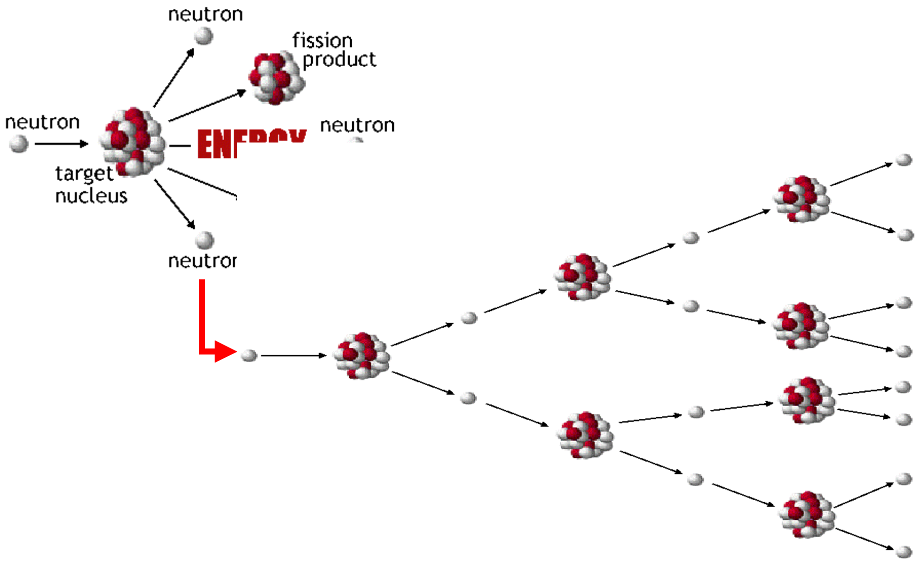
- Emirbayer M, Mische A. 1998. What is Agency? *American Journal of Sociology* **103**(4): pp. 963-1023
- Freeman C, Soete L. 1997. *The Economics of Industrial Innovation* (3rd ed.). The MIT Press: Cambridge, MA
- Fujimoto T, Dongsheng G, Jewheon O. 2006. Competition and Co-operation in Automotive Steel Sheet Production in East Asia. *MMRC Discussion Paper Series* (MMRC-F-73), march.
- Gaddis P. 1959. The Project Manager. *Harvard Business Review* **37**(3): pp. 89-97
- Hargadon A. 2003. *How Breakthroughs Happen*. Harvard Business School Press: Boston, MA
- Hargadon A, Douglas Y. 2001. When Innovations Meet Institutions: Edison and the Design of the Electric Light. *Administrative Science Quarterly* **6**(3): pp. 476-501
- Henderson R, Clark K. 1990. Architectural innovation: the reconfiguration of existing product technologies and the failure of established firms. *Administrative Science Quarterly* **35**(1): pp. 9-30
- Hitch C, McKean R. 1960. *The Economics of Defense in the Nuclear Age*. Harvard University Press: Cambridge, MA
- Hounshell D. 2000. The Medium is the Message, or How Context Matters: The RAND Corporation Builds an Economics of Innovation, 1946-1962. In A Hughes, T Hughes (Eds.), *Systems, Experts and Computers. The Systems Approach in Management and Engineering in World War II and After*: pp. 255-310. The MIT Press: Cambridge, MA
- Hughes T. 1983. *Networks of Power. Electrification in Western Society, 1880-1930*. The John Hopkins University Press: Baltimore
- Johnson S. 2002. *The Secret of Apollo. Systems Management in American and European Space Programs*. The John Hopkins University Press: Baltimore
- Kieser A. 1994. Why Organization Theory Needs Historical Analysis - And How This Should Be Performed. *Organization Science* **5**(4): pp. 608-620
- Langlois R, Robertson P. 1992. Networks and innovation in a modular system: lessons from the microcomputer and stereo components industries. *Research Policy* **21**: pp. 297-313

- Lenfle S. 2001. Compétition par l'innovation et organisation de la conception dans les industries amont. Le cas d'Usinor. Université de Marne-la-Vallée: Champs-sur-Marne, France
- Lenfle S. 2008a. Exploration and Project Management. *International Journal of Project Management* **26**(5): pp. 469-478
- Lenfle S. 2008b. Proceeding in the dark. Innovation, project management and the making of the atomic bomb. *CRG Working Paper*(08-001)
- Loch C, DeMeyer A, Pich M. 2006. *Managing the Unknown. A New Approach to Managing High Uncertainty and Risks in Projects*. John Wiley and Sons, Inc.: Hooboken, New Jersey
- Loch C, Terwiesch C, Thomke S. 2001. Parallel and Sequential Testing of Design Alternatives. *Management Science* **45**(5): pp. 663-678
- Marples D. 1961. The decisions of engineering design. *IEEE Transactions of Engineering Management* **2 :** pp. 55-71.
- Morris P, Crawford L, Hodgson D, Shepperd M, Thomas J. 2006. Exploring the role of formal bodies of knowledge in defining a profession – The case of project management. *International Journal of Project Management* **24**(8): pp. 710-721
- Nelson R. 1959. The economics of parallel R and D efforts: a sequential-decision analysis. *Rand Corporation Working Papers* (RM-2482)
- Pich M, Loch C, DeMeyer A. 2002. On Uncertainty, Ambiguity and Complexity in Project Management. *Management Science* **48**(8): pp. 1008-1023
- Sanchez R, Mahoney J. 1996. Modularity, flexibility and knowledge management in product and organization design. *Strategic Management Journal* **17**: pp. 63-76
- Shenhar A, Dvir D. 2007. *Reinventing Project Management.* Harvard Business School Press: Boston, MA
- Siggelkow N. 2007. Persuasion with case studies. *Academy of Management Journal* **50**(1): pp. 20-24
- Sommer S, Loch C. 2004. Selectionism and Learning in Projects with Complexity and Unforseeable Uncertainty. *Management Science* **50**(10): pp. 1334-1347

- Thomke S. 2003. *Experimentation Matters*. Harvard Business School Press: Boston, MA
- Ulrich K. 1995. The role of product architecture in the manufacturing firm. *Research Policy* **24**: pp. 419-440
- Van de Ven A, Polley D, Garud R, Venkataraman S. 1999. *The innovation journey*. Oxford University Press: New-York
- Wheelwright S, Clark K. 1992. *Revolutionizing product development. Quantum leaps in speed, efficiency and quality*. The Free Press: New-York
- Yin R. 2003. *Case Study Research. Desing and Methods.* (3rd ed.). Sage Publications: Thousand Oaks, CA

FIGURES

Figure 1. The principle of nuclear chain reaction



Source : http://www.cfo.doe.gov/me70/manhattan/resources.htm

Figure 2. Alternative bomb designs at the Berkeley seminar (July, 1942)

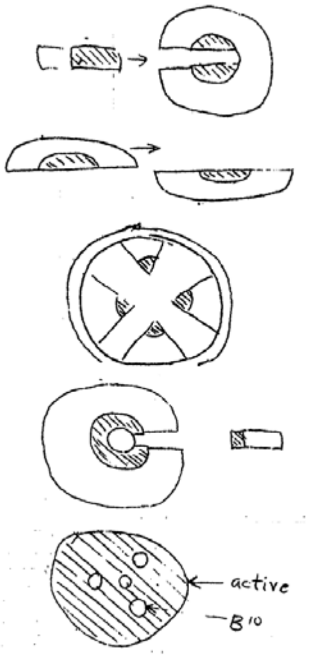


Figure 3: organization of the project[27]

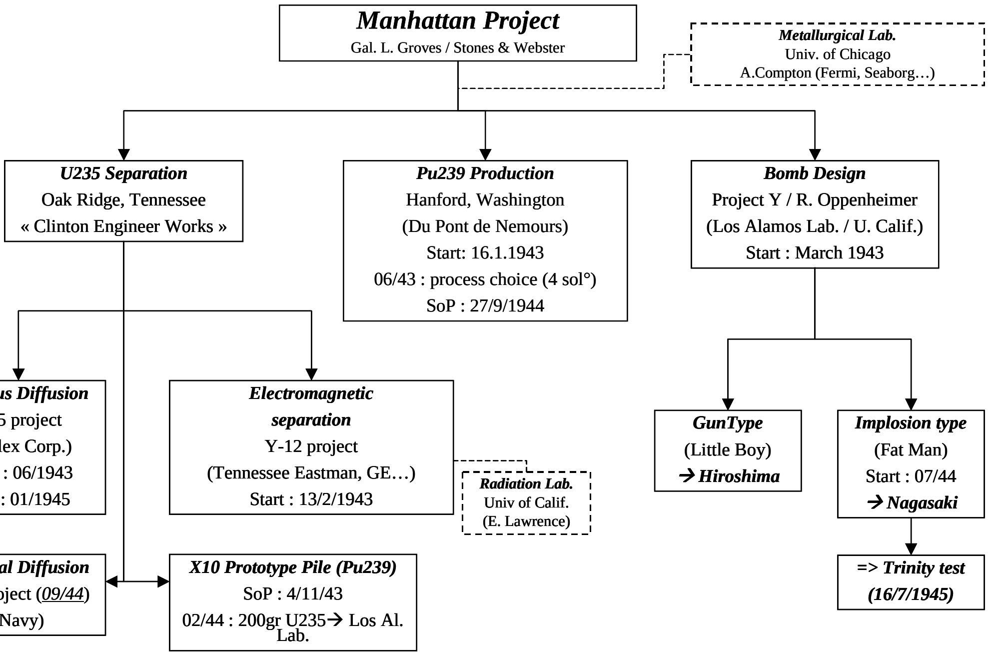
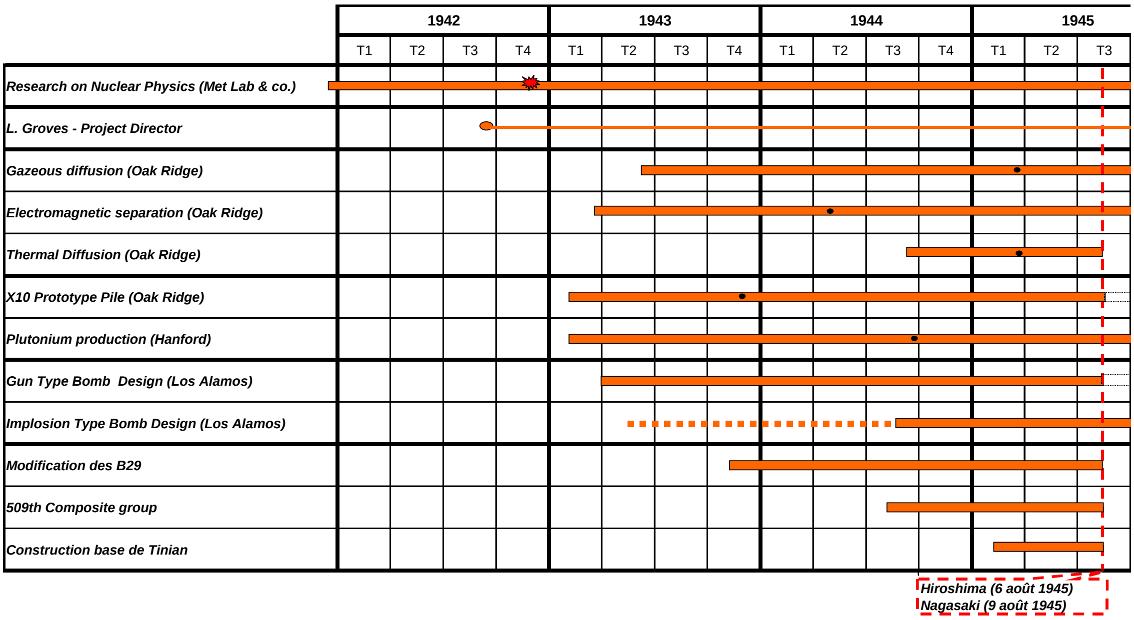


Figure 4: planning of the Manhattan Project



---

[27] This figure describes the "scientific" part of the Manhattan Project. We deliberately leave aside the other dimensions (uranium supply, espionage, B29 modifications, Tinian base construction in the Pacific and crew

Figure 5. Gun type fission bomb

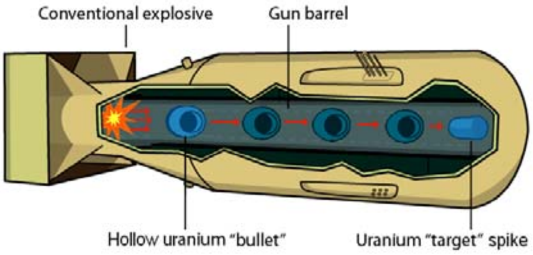


Figure 6. Implosion type fission bomb

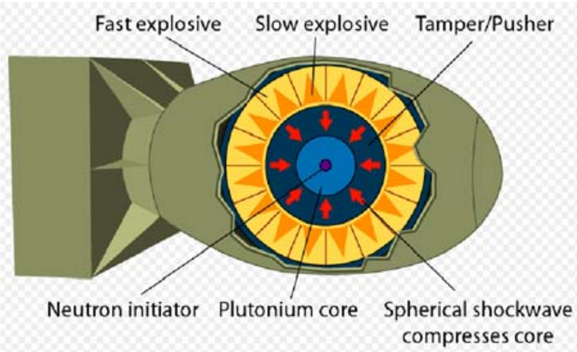
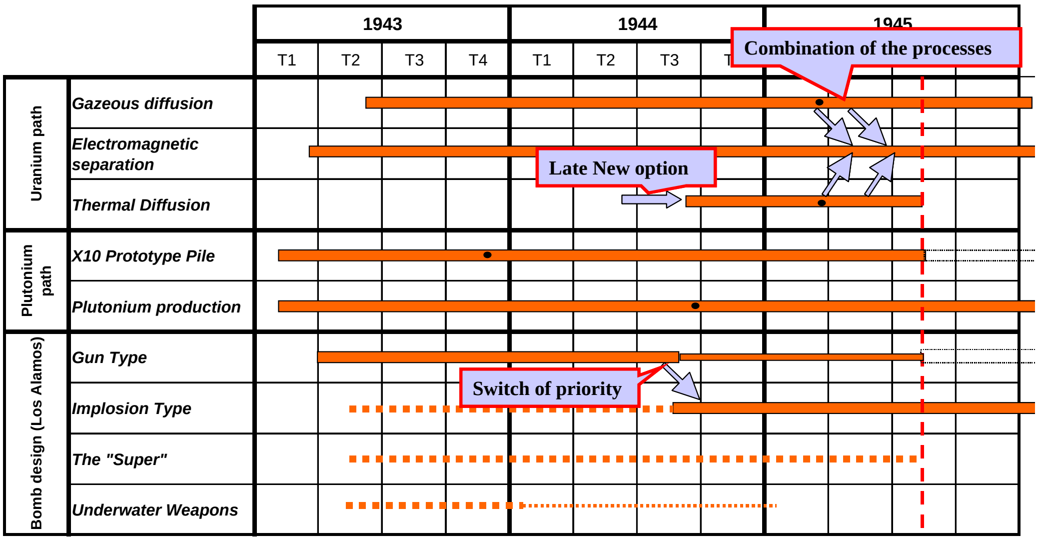


Figure 7. The evolution of the Manhattan Project



---

training) that are less important for the analysis of parallel strategy management. A complete description is available in Lenfle (2008b).