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Proceeding in the Dark.

Innovation, project management and the making of the atomic bomb.

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"*The whole enterprise constitutes...a far deeper interference with the natural course of events than anything ever before attempted, and its impending accomplishment will bring about a whole new situation as regards human resources. Surely we are being presented with one of the greatest triumphs of science and engineering, destined deeply to influence the future of mankind*."

Niels Bohr to F.D. Roosevelt, June 1944

"*This project should not be considered simply in terms of military weapons, but as a new relationship of man to the universe*".

H.L. Stimson, Secretary of War, to the Interim Committee,
Washington D.C, may 31, 1945

"*Never in history has anyone embarking on an important undertaking had so little certainty about how to proceed as we had then.*

General Leslie R. Groves, Manhattan Project Director
(in Groves, *Now it can be told*, 1962, p. 72).

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1. Introduction[2]

The strategic role of new product development and innovation (Nonaka & Takeuchi, 1986, Wheelwright & Clark, 1992; Hamel & Prahalad, 1994, Brown & Eisenhardt, 1998; Doz & Kosonen, 2008) makes design performance a central concern of managers. Project management therefore appears to be an adequate solution to the integration problems raised by these activities. Adler (1989), for example, makes the project the main way to implement innovations. Work such as that of Clark & Fujimoto (1991) has thus helped make heavyweight project management a dominant organizational model. This is a major characteristic of American managerial literature. Indeed, the leading US manuals [typically Burgelman et al., 2004] cover in detail the way in which the innovation process is carried out, technology analysis tools, the development of industry, etc., but offer little insight into the organization appropriate to innovation. Indeed, this topic is approached either via the resource-based model (Hamel & Prahalad, 1994), from the perspective of functional policies or, when the question of integration is raised, via project management models. The article by Clark & Wheelwright (1992) on heavyweight project management is therefore the incontrovertible reference.

In this article, we wish to question this tendency to equate projects and innovation. This tendency can, in fact, appear surprising inasmuch as Clark & Fujimoto (1991) indicate that their research does not take into account the question of advanced engineering or basic research (p. 26). We therefore believe that it can lead to improper use of the project format to manage innovation. We feel that, in line with work on project classification (Wheelwright & Clark, 1992; Shenhar & Dvir, 2004 & 2007; Balachandra & Friar, 1997), 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've discussed the complex links between project management and innovation management literatures. Specifically we note the gap between a definition of project that underline novelty, and a mainstream literature which propose an instrumental view of Project Management (typically the PMI Body of

[2] This paper has been presented as a slideshow at the Workshop on « technological uncertainty in modernity » organized by. P. Fridenson (EHESS) and P. Scranton (Rutgers University), with the support of the Centre de Recherche Historique – EHESS, Abbaye des Vaux de Cernay, March 14-15, 2008.

Knowledge, see Duncan, 1996). While criticized in recent years[3] this "rational" view of project management as the accomplishment of a clearly defined goal in a specified period of time, within budget and quality requirements, remains dominant in most textbooks and discourses on project management. But we can wonder if this is adapted to innovation management. Actually innovation is first and foremost characterized by divergence and discovery (Van de Ven & al., 1999), and unforeseeable uncertainties which render the rational approach irrelevant (Loch & al., 2006). We thus argue for a model of project management that relies on specific principles adapted to situations of exploration where neither the goal, nor the way to reach it are known at the beginning.

In this paper we want to continue this discussion of the links between project and innovation with a different methodology. Instead of using contemporary materials we will go back to history by analyzing one of the most important project ever undertaken : the Manhattan Project. We decided to focus on this case for several reasons.

First because of its historical importance. The Manhattan Project remains one of the biggest project ever undertaken (it mobilized nearly 130 000 persons in 1945, the size of the automotive industry at this time) and had a major impact on the second World War and, more generally, International Relations. Indeed, the Manhattan Project has changed the unfolding of World War II, leading to the quick surrender of Japan on august 15, 1945[4]. Moreover it marked the beginning of the cold war and of the nuclear arm race between the US and the Soviet Union. Thus, as explains by N. Bohr after the project, it leads to "*a completely new situation, that cannot be resolved by war*" (N. Bohr, 1957, quoted in Rhodes, 1986).

The second reason is that the making of the Atomic Bomb represent unquestionably a major breakthrough in the history of technology. It exemplifies the power of "Big Science" i.e. the mobilization of important resources (human, financial, industrial) to

[3] See the Special Issue of the International Journal of Project Management on *Rethinking Project Management*, 2006, vol. 24 n°8

[4] There is of course some controversy on this point, some arguing that the bombing was not necessary and that Japan was ready to surrender. D. Eisenhower was among them explaining that "*...in [July] 1945... Secretary of War Stimson, visiting my headquarters in Germany, informed me that our government was preparing to drop an atomic bomb on Japan. I was one of those who felt that there were a number of cogent reasons to question the wisdom of such an act.* (…) *I had been conscious of a feeling of depression and so I voiced to him my grave misgivings, first on the basis of my belief that Japan was already defeated and that dropping the bomb was completely unnecessary, and secondly because I thought that our country should avoid shocking world opinion by the use of a weapon whose employment was, I thought, no longer mandatory as a measure to save American lives. It was my belief that Japan was, at that very moment, seeking some way to surrender with a minimum loss of 'face'.*" Dwight Eisenhower, *Mandate For Change*, p. 380. See also footnotes 7 below.

overcome major scientific and technical problems. As noted by Hoddeson & al. (1993) the managerial practices developed at the Los Alamos Laboratory have been widely developed after WWII in the US scientific and industrial community. Studying how this breakthrough happens may thus lead to important insights on the management of innovation.

Finally we focus on the Manhattan Project because of its place in the literature on project management. Indeed, the Manhattan Project is frequently quoted in this literature as reference, the proof of the power of projects. Gaddis (1959), in his seminal paper on the project manager, mention it for its incredible success and Morris (1994), in his history of Project Management, explains that the MP "*certainly displayed the principles of organization, planning and direction that typify the modern management of project.*"[5] (p. 18). More recently, Shenhar & Dvir (2007) stated that "*The Manhattan Project exhibited the principles of organization, planning, and direction that influenced the development of standard practices for managing projects*" (p.8). However, we will see that a careful analysis of the project does not confirm this views.

We thus believe that the Manhattan project constitute an exemplary case that may provide interesting insights for the management of project and innovation, a contemporary research question (see, Loch & al, 2006). More specifically, we want to show that this case illustrate the strength of this organization to manage radical innovations. But a closer look at the project, using, reveals that most of the best practices classical project management are ignored. On the contrary, as we will show, the Manhattan Project is typical of exploration projects management (e.g. Loch & al., 2006 ; Lenfle, 2001 & 2008).

Methodologically we believe that the Manhattan Project is particularly well suited to single-case study. As Yin (2003) explains, there is five rationales to use a single-case methodology :

1. it represents the critical case in testing a well-formulated theory;
2. the case represents an extreme case or unique case;
3. conversely the case is the representative or typical case;
4. it is the revelatory case
5. it gives the opportunity for longitudinal case study.

[5] Adding that "*it also displayed many of the problems, such as cost overruns and concurrency that have characterized defence project ever since*" (p. 18).

We will see that the Manhattan Project constitutes in some dimension an extreme case. But, at the same time, it provides useful insight on contemporary research questions on the management of highly uncertain and innovative project. Thus, as Siggelkow (2007) suggests we will thus use the case both to illustrate the challenges faced by this project and to discuss and extend existing theory on this question, specifically the framework proposed by Loch & al. (2006). This study is thus based on published sources since a vast empirical material is available on the history, organization and management of the Manhattan Project[6].

The paper is organized as follows. We will first briefly present the dominant model of project management and its limitation. The second section will present the origins and objectives of the Manhattan Project. The third section will be devoted to an analysis of the scientific and technical challenges the project has had to face. The strategy of the Project Director to manage uncertainty will thus be presented (§4). Section 5 will illustrate this strategy with several short cases from the project. Finally we will draw some conclusions concerning the management of exploration projects.

Before entering the Manhattan Project we want to underline that we don't want to enter here the debate on the necessity to use the bomb against Japan[7]. We are aware of the difficulty to study a project which finally lead to the death of tens of thousands of persons in the cities of Hiroshima and Nagasaki[8]. We fully agree with French novelist Albert Camus who wrote, two days after Hiroshima's bombing that, « *the mechanical civilization as reach its highest degree of savageness. We will have to chose, in a more or less distant future, between collective suicide and intelligent use of scientific conquests.* » (*Combat*, August, 8, 1945). This his another, frightening, dimension of this project.

2. The dominant model of project management

Project Management is now a well developed and structured field for practitioners and academics. Textbooks, journals, professional association exists that have led to the development and the formalization of a set of tools and concepts that are now widely

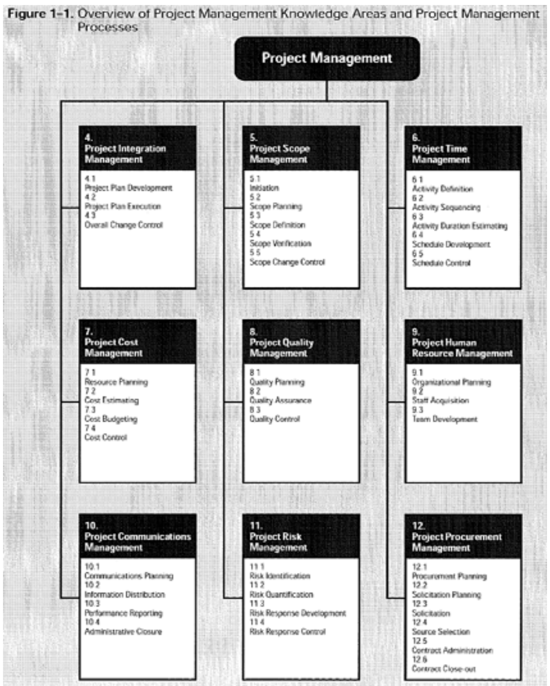
[6] See the "Manhattan Project" section in the references.

[7] For an introduction see the articles from S. Walker and G. Alperovitz, reproduced in Kelly, 2007. See also Malloy (2008) which provides a complete discussion of the decision to use the bomb against Japan.

used. Even if it has been criticized in recent years (Cicmil & al. 2006; Morris & al. 2006) the model developed and publicized by the US Project Management Institute (Duncan, 1996) remains dominant in most discourses and textbooks on project management.

In this perspective project management is a “rational” methodology whose aims is the accomplishment of a clearly defined goal in a specified period of time, within budget and according to requirements. Therefore this model is focused on project execution and the associated management techniques for planning, scheduling, controlling cost, etc (figure 1 and 9 p. 38). According to this “*instructionist*” strategy (Pich & al, 2002) the goal is to avoid uncertainty by defining ex-ante 1) a critical path, 2) a risk management plan to manage the foreseeable contingencies that the project may meet.

Figure 1. The project management body of knowledge (Duncan, 1996).



More specifically this model underlines the importance of “clear objectives” in project success. Project are more likely to be successful if the goal is clearly defined at the beginning. In this literature this appears to be a structuring dimension of projects. This point is worth noting since General Leslie Groves, the Manhattan Project Director,

[8] It is interesting to note that there was no consensus within the project on this awful question The opponents, lead by L. Szilard, argue for a demonstration of the power of the bomb to convince Japan to surrender.

identifies the existence of a clear objective as a key success factor of the project. He thus wrote in 1962, 17 years after the project completion (Groves, 1962, p. 414) :

> "*First we had a clearly defined, unmistakable, specific objectives. Although at first there was considerable doubt about wether we could attain this objective, there was never any doubt about what it was. Consequently the people in responsible positions were able to tailor their every action to its accomplishment*."

This constitute a key point of our thesis since in our view "build an atomic bomb" is not a clear objective. Indeed if this is a clear objective then almost everything ("go to Mars", "Design a flying car",…) becomes a clear objective. There is furthermore a contradiction in Groves statement since his account of the project shows the huge uncertainties the project has to face and the problems raised by atomic power. In this perspective the Trinity test in July 16, 1945, was literally a revelation for the team (see section 6.2.3. below). Thus it seems to us very questionable to argue that the goal of the Manhattan Project was "clearly" defined.

We will elaborate more on this point in the remaining of this paper but we can already note that the pre-existence of a "clear" objective is typical of the literature on project management. Most of this works emphasizes the need to clearly define the goal of the project at the beginning. In this perspective a typical "Development" project start with a "contract book" which specify the specifications of the product, business plan, schedules, deliverables, manufacturing plans, and so on (see Wheelwright & Clark, 1992, chap. 8). Even the most recent research on project management rely on this assumption. For example Shenhar & Dvir research [synthetized in Shenhar & Dvir, 2007], which is particularly interesting since it bridges the gap between PM and Innovation management literature by making extensive use of the latter, presuppose that there is always a defined objective, even potentially "new to the world", at the beginning of a project (see their definition of *breakthrough projects* in 2007, p. 67).

We think that it is not necessary to have a "clear objective" to define a project. There is of course always an objective in every project. Indeed, as this has been remarkably shown by Boutinet (1990) or Emirbayer and Mische (1998), one cannot imagine action without a goal. "Projectivity", to use Emirbayer and Mische concept, is thus consubstantial to action and, obviously, central in the project form. But we cannot limit projectivity to a "clear objective". It is more generally, as stated by Boutinet an ability to create, to throw oneself into the future, to plan to do something. The clarity or completeness of this goal is thus not always necessary to define a project. In some case the objective is complete and straightforward ("build a bridge from point A to point B using the proven technology X in Y month for Z $$$) whereas in other cases the goal is very fuzzy and evolve during the project.

Following the work of Abernathy & Clark (1985) projects can thus be classified based on their impact on the firm's technical capabilities and on its "market" capabilities. In this perspective, "Development" refers to a situation were the technical and market knowledge associated with the project are well-known. It is thus possible to write a complete "contract book". On the contrary, "Exploration[9]" refers to a situation were both have to be explored. In this latter case, the objective itself is, as we will see, unknown or at least partially indefinable, and the project enters an exploration process (March, 1991). The result of the project is then no more only a product, as we will see. We therefore agree with contemporary thinking on the management of innovation, defined as a two-fold process of exploration of knowledge and concepts[10] which then give rise to developments or research (Lenfle, 2001; Le Masson & al. 2006; Loch & al. 2006). In this perspective project are an important component of search processes (Loch & al, 2006; Adler & Obstfled, 2007 ; Lenfle, 2008).

3. Origins and objectives of the Manhattan Project

The Manhattan Project was a part (perhaps the biggest with the research on radars at MIT[11]) of the global mobilization of US Science during WWII. The military significance of atomic power was first brought to the front by a famous letter from A. Einstein to F. Roosevelt on August, 2, 1939. This does not lead to a project at first. It is nevertheless interesting since it shows that the Manhattan Project builds on 20 years of research in the new field of nuclear physics. Indeed, since the pioneering works of Ernest Rutherford (1919), nuclear physics experienced a burst of researches, at first mainly in Europe. This research accelerates in the thirties. The works from Chadwick (1932); Cockcroft & Walton (1932), F. & I. Joliot-Curie (1934), E. Fermi (1934 &

[9] We prefer "exploration" rather than innovation. This later terms cover a very large number of definition and does not always implies exploration and/or the development of completely new knowledge.
[10] We refer here to the C/K theory of design developed par Hatchuel & Weil [34 for an introduction].
[11] see Mindell, 2000 for an introduction.

1938); Hahn & Strassman (1938) and Meitner & Frisch (1938) laid the foundations on which the Manhattan Project would build.

However, until the beginning of WWII, the question remains mainly a research topics for academics. This changed with the creation of the National Defence Research Council lead by V. Bush, in 1940. The goal was to coordinate the different research programs in the US to prepare a possible war. In July 1941 the British MAUD report was send to the US. It synthesized the British researches on nuclear physics and conclude on the possibility of using nuclear fission to build an atomic bomb. This strengthen the US involvement in this field and leads to the creation of the Uranium (or S1) committee within the newly formed Office of Scientific Research and Development[12] to coordinate US efforts on the A Bomb on December 6 1941... the day before Pearl Harbour.

At this date there was researches in US universities (Chicago, Illinois, Columbia, California...) on this question but the overall effort was still loosely coordinated. Things began to change during the summer of 1942 when V. Bush and J. Conant decided to involve the Army Corps of Engineer to manage the project. Colonel Marshall was appointed to manage the entire program, a seminar was organized at Berkeley in July by R. Oppenheimer to discuss possible bomb designs and the project was code-named Manhattan Engineering District (MED) in august. However, Colonel Marshall did not succeed in accelerating the program. He was replaced in September 17, 1942 by General Leslie Groves, a member of the Army Corps of Engineers, who was a very experienced project manager[13].

His appointment marked the take off of the project. As Groves explains "*there were three basic military considerations involved in our work. First the Axis Powers could very easily soon be in an position to produce either plutonium or U-235, or both. There was no evidence to indicate that they were not striving to do so; therefore we had to assume that they were. To have concluded otherwise would have been foolhardly. Second, there was no known defense against the military use of nuclear weapons except*

[12] The OSRD was lead by V. Bush. It comprises the NDRC, now directed by J. Conant, Harvard President. Bush & Conant were decisive in the ramp-up of the Manhattan Project.

[13] Leslie Groves was a brilliant, though not very popular, officer in the Army Corps of Engineer. During is pre-war career he has managed dozens of projects in the US (including R&D projects). After Pearl Harbor, during the mobilization of the US army and industry for the war, he became the Deputy Chief of Construction in the Corps of Engineers. He thus oversaw the construction of cantonments, munitions plants, airfields and so on, including the Pentagon. This gave him an intimate knowledge of the strength and weaknesses of construction and engineering firms across the country. When he was appointed to lead

the fear of their counteremployement. Third, if we were successful in time, we would shorten the war and this save tens of thousands of american casualties" (Groves, 1962, p. ??)

However, the accomplishment of this objectives was far from obvious, even if Groves hierarchy told the contrary. He thus wrote in his memories : "*Later that morning I saw Styer at his office in the Pentagon. He confirmed my worst premonition by telling me that I will be placed in charge of the Army's part of the Atomic effort. He outlined my mission, painting a very rosy picture for me : " the basic research and development are done. You just have to take the rough design, put them into final shape, build some plants and organize an operating force and your job will be over and the war will be finished". Naturallly I was sceptical, but it took me several weeks to realize just how overoptimistic an outlook he had presented."* (Groves, 1962 p. ??). Indeed, as we will see, the basic research and development were not done.

4. A Scientific and Technical Everest

To understand the difficulties the project had to face we first have to dive a bit into nuclear physics and, second, to identify the main design problems raised by the making of an atomic bomb. We conclude by explaining their managerial implications

4.1. Nuclear physics for dummies

The Manhattan Project didn't start from scratch. As explained by H.D. Smyth in his report[14] : "*The principal facts about fission had been discovered and revealed to the scientific world. A chain reaction had not been obtained[15] but its possibility – at least in principle – was clear and several paths that might lead to it had been identified.*(p. 364)". But he precised immediately that : "*All such information 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*

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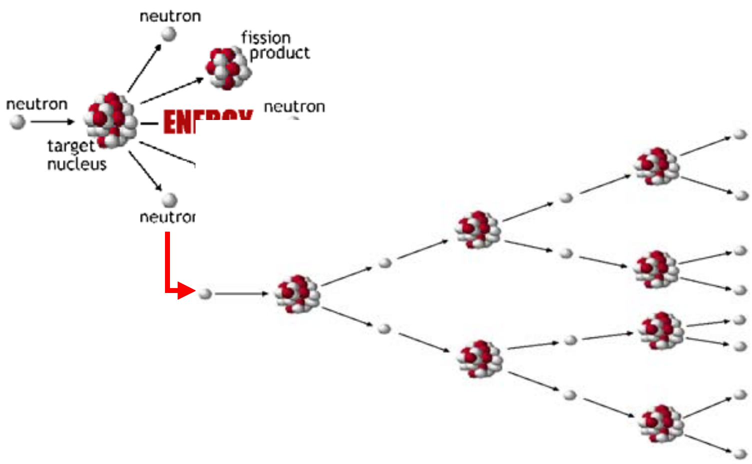
the MED he was thus able to chose the best firms and individuals. On Groves, a key figure of the Manhattan Project, see Norris (2002).

[14] Released just after Hiroshima, the Smyth report was written under by H.D. Smyth, a renowned physicist at Princeton and a consultant on the project, at the request of Leslie Groves. It thus represent the "official" history of the project.

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 is the following (figure 2). As demonstrated by Meitner & Frisch in 1938, when a neutron hit an atom of uranium this one splits in two parts, releasing energy and additional neutrons, that will split the two parts again and so on[16]. Thus scientifically some[17] of the major problems were to find 1) the critical mass of fissionable material needed to start and sustain a chain reaction, 2) the number of neutrons released at each step (the reproduction factor, k) knowing that they can be lost or absorbed by other materials.

Figure 2. The principle of nuclear chain reaction.



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

This was a true revolution since "*the most energetic chemical reactions* [known at this time] – *burning hydrogen with oxygen, for example – release about 5 electron volts per atom. Meitner calculated, and Frisch soon demonstrated by experiment, that a neutron moving at energies of only a few electron volts, bombarding an atom of uranium and bursting it, would release about 170 million electron volt per atom. 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, *The Los Alamos Primer*, 1992, p. xiii).

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[15] This will occurs two months after the beginning of the project in December 1942, under the direction of E. Fermi at the Unversi ty of Chicago.

[16] This chain reaction was only envisioned by Meitner & Frisch. Their contribution was to demonstrate the splitting process. They build on experiments by Hahn & Strassman, close colleagues of Lise Meitner.

[17] I insist on some, since there was thousands of problems during the course of the project

4.2. From theory to practice…

But this was only theoretical in 1942[18] and the Manhattan project faced two major problems :

- The production of fissionable materials
- The design of the bomb itself.

This has been complicated by the overwhelming importance of time. US government was afraid that Nazi germany build the bomb first. This leads the project steering comitee to decide, in November 1942, to skip the pilot phase and to go directly from research to full-scale production[19].

4.2.1. The production of fissionable materials

Two materials were identified at the beginning of the project to sustain a chain reaction. The first uranium 235 is a natural component of natural uranium (U238) but represent only 0,72% of its mass. The second, plutonium (Pu239) is a by-product of nuclear fission discovered by G. Seaborg in 1941, only a year before the start of the project. In both case, the production of fissionable materials raised huge scientific and technical problems :

– Separating U235 from U238 involves extremely complex processes, based on the slight differences of the atomic mass of the two isotopes (less than 1%). To perform this task 7 different methods had been identified in 1941 among which, as we will see, 3 will finally be used (Smyth, 1945).
– In the same way, producing plutonium involves the design and construction of nuclear reactors and the associated chemical separation plants. Twelve separation processes were studied at the Met Lab at the beginning of plant construction.

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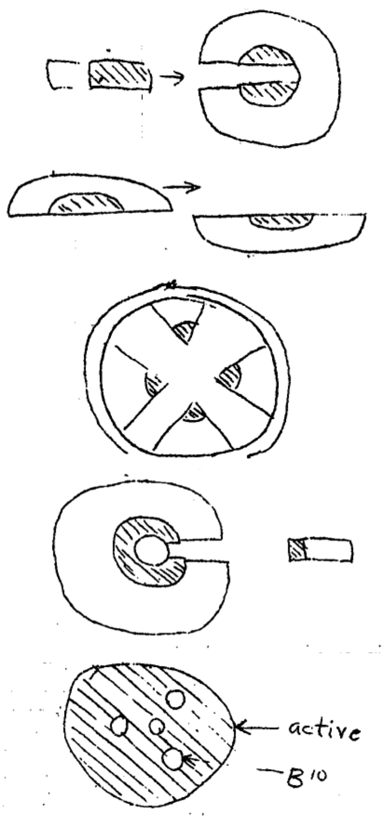
[18] The first self-sustained nuclear reaction was obtained by Enrico Fermi and his team at the University of Chicago on December 2, 1942.

[19] This decision had important consequences for the project. This is not the central topic of this paper. However it is interesting to keep this point in mind. Consider, for example, the following case that explains the difficulty of the so-called electromagnetic separation process (code-named Y-12) : « *Between October and mid-December [1943], Y-12 paid the price for being a new technology that had not been put through its paces in a pilot plant. Vacuum tanks in the first Alpha racetrack leaked and shimmied out of line due to magnetic pressure, welds failed, electrical circuits malfunctioned, and operators made frequent mistakes. Most seriously, the magnet coils shorted out because of rust and sediment in the cooling oil. (..) Alpha 2 fared little better when it started up in mid-January 1944. While all tanks operated at least for short periods, performance was sporadic and maintenance could not keep up with electrical failures and defective parts. Like its predecessor, Alpha 2 was a maintenance nightmare.* » Source : http://www.cfo.doe.gov/me70/manhattan/y-12_operation.htm

This were breakthrough innovations. This processes didn't exist before the project (plutonium production) or had never been used with radioactive materials (chemical separation). They supposed extremely tight requirements, involves radioactive (and thus very dangerous) materials and so on. And, above all, the available knowledge on both the production, metallurgy and chemistry of plutonium and Uranium separation was far from complete. Thus, discussing the research program of the Chicago Met Lab on plutonium for 1943, H. Smyth explains 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, p. ?). We think that this tension between theory and usefulness lies at the heart of the Manhattan project and is also a central characteristics of innovative project management.

4.2.2. Alternative bomb designs

The team face the same situation concerning the design of an atomic bomb. In a seminar organized by R. Oppenheimer at Berkely in july 1942, scientist met to discuss alternative bomb designs (figures on the right, from Serber, 1992). Thus a number of alternative fission bomb assembly design were envisioned : the gun method (at top), the implosion method (center), the autocatalytic method, and so on. In the end, only the "gun" method and a more complicated variation of the "implosion" design would be used but, as we will see, the path to it was not straightforward. Furthermore the Berkeley discussion was theoretical, no prototypes were build nor experiments undertaken. Whether, for example, a "gun" design works for uranium and plutonium, or an "implosion" device was feasible, remains to be proved.



4.3. Managerial implications

This situation as fundamental managerial implications. The most important is that the entire project was 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 & Loch, 2004, p. 1334). This means that the team faces a situation where events can occurs that are outside is knowledge. This cannot be more clearly explained than by Groves statement that "*the whole endeavour was founded on possibilities rather than probabilities. Of theory there was a great deal, of proven knowledge, not much*" (Groves, 1962, p. 19). Therefore the team cannot plan or prepare for them. In contemporary terms, Project Risk Management[20] is no longer efficient since nobody can anticipate the risks (see Loch & al., 2006 for an excellent discussion of this question).

In this case nobody can predict the unfolding of the project and Groves quickly realized the implications of this situation. First he recognized the impossibility to buind a reliable plan of the project. As he explained (July 9, 1942) : « *Out of this meeting a tentative construction program emerged. It called for the starting of the construction of the plutonium reactor piles by October 1, 1942; of the centrifuge process by January 1, 1943; of the gaseous diffusion process by March 1, 1943; of the electro-magnetic separation process by November 1, 1942. It soon became apparent that this 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). And actually the schedules will becomes reliable only at the end of 1944.

But this raise a more fundamental problem. As the last sentence indicate the available knowledge at the beginning of the project was largely inexistent. Let's follow Groves again, this time at the end of a meeting with scientists at the University of Chicago on October, 5, 1942, soon after his nomination : "*As the meeting was drawing to a close, I 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 accurante 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*

[20] That did not exist as an established methodology in 1942.

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 is imply was not possible then to arrive at a more precise answer.” (Groves, 1962, p. 40) He thus conclude : “*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.).

It his thus clear that the “rational” model of project management is irrelevant in this situation. Given the unavoidable unforeseeable uncertainties it is impossible to design a Work Breakdown Structure, to define a planning, to estimates costs, to anticipate risks… which constitutes the building blocks of the traditional/PMI approach of project management. The question thus becomes : how did they do ?

5. Managing the unknown[21] : concurrent exploration and engineering.

Considering unforeseeable uncertainties, Groves and the Steering Committee adopted a very innovative strategy. First they decided to explore and implement simultaneously the different solutions, both for the production of fissionable materials and for bomb design. Secondly, given the utmost importance of time, they proceeded concurrently, doing fundamental research, designing and building the plant at the same time. If Groves has already used concurrent engineering in past projects, it was the first time that it 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). For example Thayer (1996) shows that DuPont pushed this strategy to “*its ultimate extreme*” (p. 42) in the management of the Hanford Project that leads to the production of plutonium. They decided, following Groves decision, “*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*" (ibid. p. 41). This was a breakthrough in their managerial practices, even if they already practice concurrent engineering, since "*DuPont normal, peacetime commercial practice was to hold off on construction until final design has reached 60% completion*" (ibid)[22].

Shortening the project 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) [23]. For Hewlett & Anderson (1962) "*Groves wanted speed. 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*." (p. 181).

To clarify the meaning of this strategy, we've used the published sources (a timeline of the project is available in Kelly, 2007, we've completed it with Smyth, 1945; Hewlett & Anderson, 1962; Gosling, 1999 and Rhodes, 1986, when necessary) to reconstruct the project organization and unfolding. For each we've tried to indicate start of operations and the start of production (SoP, also black points in figure 5). Figure 3 & 4 summarizes the organization of the project and figure 5 its unfolding. Figure 3 & 5 are the most interesting for our purpose. What is striking to note is the simultaneity of the different tasks :

- Uranium separation, plutonium production and bomb design proceed concurrently;
- For Uranium separation two different methods are used in parallel, and a third one has been added late in the project (September 1944, we will come back to this later);
- The Los Alamos laboratory explore several different methods at the same time. They first focus on the "gun" design but, as we will see, they have to switch to "implosion" design in july 1944.

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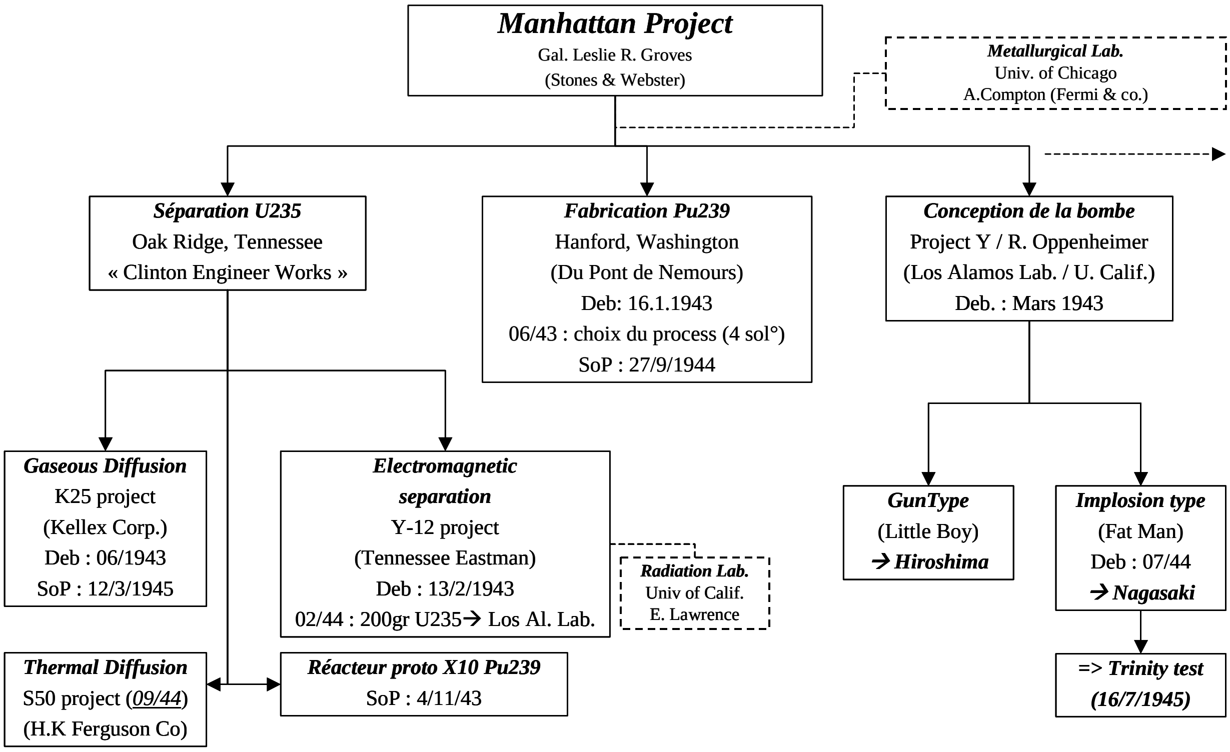
[21] This title is borrowed from Loch & al. 2006

[22] See Thayer for a description of how DuPont engineers include uncertainty in their design of the separation plant. The same strategy was at work in Gaseous Diffusion where engineers design the process to start the plant as the separation tanks became available, and progressively add new ones.

[23] According to Thayer (1996), had the Hanford project proceeded according to the traditional rational/sequential rather than the concurrent method, the first plutonium would not have been ready to test and use until may 1948, almost three years after its actual completion … and not before 12 years under rational peacetime practices !!! (Thayer, 1996, p. 45-46). Instead in took DuPont 23 months to ship the first product, which is incredible given the novelty and the complexity of the process.

Figure 3 : organization of the project (part 1)[24]



[24] Organizationally the Manhattan Project relies on classical processes in large engineering project. Each part of the project was subcontracted to one or several contractor (usually one to build and one to operate the plant, except at Hanford where DuPont managed design, construction and operation) under a cost plus fixed fee contract. The plants were thus government-owned / contractor-operated. Each time Groves "*put together an integrated group of architectural, engineering and construction firms working in concert with the army area office*" (Norris, 2002, p. 200). Groves closely oversees and monitors the project from Washington D.C. with a small staff of officers, through the local area offices he put in place for each contract, and through frequent site visits. On Groves management practices see Groves (1962), and Norris (2002). One has to realize that the magnitude of the work was without precedent, notwithstanding its scientific newness. For example at Hanford only "*the numbers were staggering : 540 buildings, more than 600 miles of roads 158 miles of railroad track, vast quantities of water, concrete, lumber, steel, and pipe. Approximately 132 000 people were hired over the period – eight times the number that had build the Grand Coulee Dam, and almost as many as has worked on the Panama Canal. Peak employment occurred in June 1944 at fifty-one thousand*" (Norris, 2002, p. 221). For a detailed and very interesting analysis of management practices of the Hanford project see Thayer, 1996.

Figure 4 : organization of the project (part 2)[25]

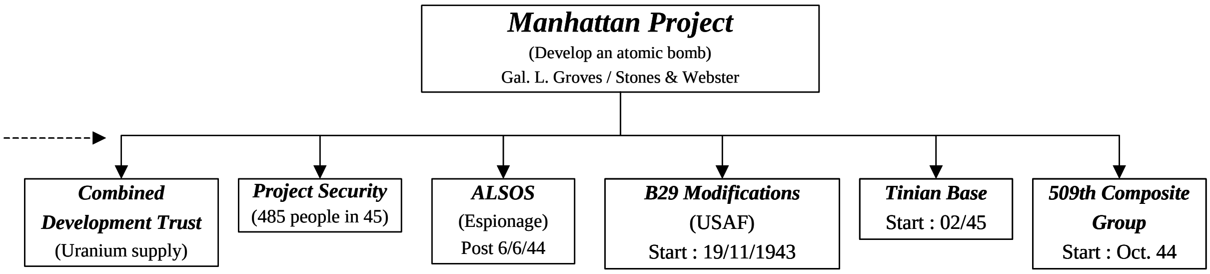
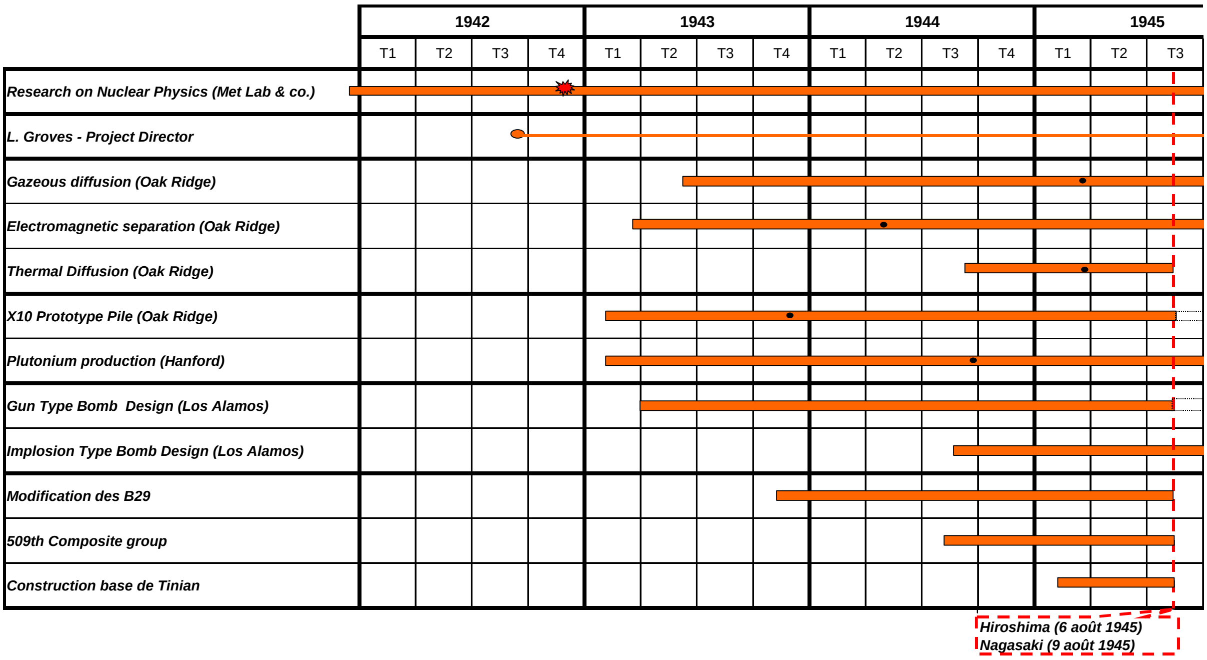


Figure 5 : planning of the Manhattan Project



The rationale behind this strategy was straightforward. Given technical and scientific unforeseeable uncertainties the simultaneous pursuit of different solutions increase the

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[25] This figure show the huge scope of the project. Indeed it became quickly obvious that the project will have to design and deliver the bomb. This activities are sometimes ignored in the account of the project. This is probably because this activities are less innovative that those of figure 3. However, they were also crucial for the final success of the project. Specifically, the delivery of the bomb (known as Project Alberta) involves important task such as B29 modifications, construction of the Tinian Air Base in the Pacific and, most important, the training of the crew. This last dimension was very important for at least tow reasons 1) the final assembly of the bombs is very complicated and has led to important engineering works to simplify this task, 2) the dropping of an atomic bomb involves specific skills to avoid the destruction of the plane itself. As Groves summarizes : « *From the problems of reactor design to the health of fish in the Columbia River and the condition of women's shoes covers a considerable range of problems, and obviously they were not of equal importance. But they all mattered in the job we were trying to do.* » (Groves, 1962, p. 93).

likelihood of success. As explained by Hoddeson & 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*". We would like to add that this also considerably enrich the exploration process, as we will see later.

6. Case studies in the management of radical innovation

We believe that this overview of the project, while necessary to understand the global managerial strategy, is insufficient to comprehend the processes at work at a more micro level. Thus to enrich our understanding of the innovative project management, we have to dive deep into the project. We decided to focus here on four cases studies that exemplifies the management of radical innovation[26].

6.1. Surprises in the production of fissionable materials

6.1.1. The canning problem in plutonium production (experimentation).

The "canning" problem[27] was probably, with the "barrier" problem in the gaseous diffusion separation process, one of the hardest challenge of the Manhattan Project. It arise in the context of plutonium production. Indeed to produce plutonium in a nuclear reactor, the raw material is uranium which is used to sustain the nuclear chain reaction. However it is impossible to use uranium directly in the pile, it has to be "canned" to be protected from the cooling water[28]. And here lies the challenge : the problem of sealing the uranium slugs into protective metal jackets raised huge technical problems and was of crucial importance since the failure of a single can might require the shut-down of an entire pile.

[26] The chosen cases are the most famous of the Manhattan Project, related by all the historians of the project.

[27] Taken from Smyth, 1945.

[28] Or air in some case, such as the X10 prototype reactor at Oak Ridge. Note that this constitute a notable exception to the decision to skip the pilot plant phase (see footnote 19 and section 4.3.). The X10 prototype reactor served as a bench for the Hanford production reactors, even if they proceed simultaneously and if the cooling technology was not the same.

The question is particularly difficult since the metal sheath should protect uranium from water corrosion, keep fission product out of the water, transmit heat from the uranium to the water and not absorb too many neutrons. This has never been done before, since plutonium has only been discovered in 1941, and has never been produced on an industrial scale. Without going into the details, Smyth explains that « *Attempts to meet the requirements involved experimental work on electroplating processes, corrosion-resistant alloys of uranium, hot-dipping processes, cementation-coating processes and mechanical jacketing or canning processes* » (8.53. In Smyth, 1945). To overcome this problem, the engineers from Dupont, its contractors and the Scientist have explored several methods simultaneously ("*two years and a half of trial and error*" wrote Rhodes, p. 598). At one point, during the winter of 1944 the different groups working on this problem "*seemed to be learning more and more about less and less. They were amassing data but not developing a process*" (Hewlett & Anderson, 1962, p. 224). Indeed the process was excessively complex, comprising a huge number of steps and parameters each of which susceptible to cause failures. Therefore, "*the Hanford operating crew considered it an accomplishment to can three or four slugs per day, even when working on double shifts. In the first two weeks [march, 1944] they succeeded in canning a total of thirty-six slugs, and none of these look acceptable*". (Hewlett & Anderson, 1962, p. 225). At the end, only the massive effort of the Du Pont engineers leads to a solution in late august 1944, only days before the start of the first Pile[29].

6.1.2. Barrier design in gaseous diffusion separation (experimentation).

The Manhattan Project encounters similar difficulties with the gaseous diffusion separation process. It 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 becomes a real nightmare for the Kellex Corp which was responsible for the design and construction of the K25 plant[30]. As Smyth writes in his report, the barrier in the gaseous diffusion process "*must have almost no holes*

[29] For a detailed account of the slug crisis see Hewlett & Anderson (1962, chap. 6).
[30] Houdaille-Herschey Corp, was the contractor responsible for producing the barrier. Whereas Union Carbide was the operating contractor.

which are appreciably arger than 0,001 micron, but must have billions of holes of this size or smaller. These holes mus not enlarge or plug up as the result of direct corrosion or dust coming from elsewhere in the system. The barrier must be able to withstand a pressure of a "head" of one atmosphere. It must be amenable to manufacture in large quantities and with uniform quality" (Smyth, 1945, 10.14, p. 432). It is thus not a surprise that the design of this barrier raised huge problems that requires both theoretical and experimental studies. Here again researchers and engineers adopted a parallel strategy exploring several solutions simultaneously. The process was unknown and so complex that "*one slight variation in any step could completely alter the separation property of the product*" (Hewlett & Anderson, 1962, p. 126). After huge 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 appears and Groves, as usual, decided to continue with the Norris-Adler design but to develop the second-type as insurance against failure. This was a good option since the second type proved to be much more promising than the Norris-Adler design which encounter huge technical problems. Thus, in a move typical of the Manhattan Project management, Groves decided "*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 & Anderson, p. 137). This leads to "*rip out all the carefully designed machinery in the Norris-Adler Plant and install the new process*" (ibid. p. 138), a very risky decision[31]. But, at the same time, the research continues on the Norris-Adler solution. This finally leads to a suitable barrier. And the separation of uranium by gaseous diffusion at Oak Ridge started on January 20, 1945, almost two years later than the initial plan (March, 1943, see p. 15)[32].

6.1.3. Xenon poisoning in Hanford's B Pile (overdesign).

September 1944 marked a crucial step in the project : the production reactor at Hanford were ready to start production. But, immediately after the start of production, an unknown phenomenon appeared : the reactivity of the pile decreased slowly, the pile died in a few hours, came back to life again, start another decline, etc.

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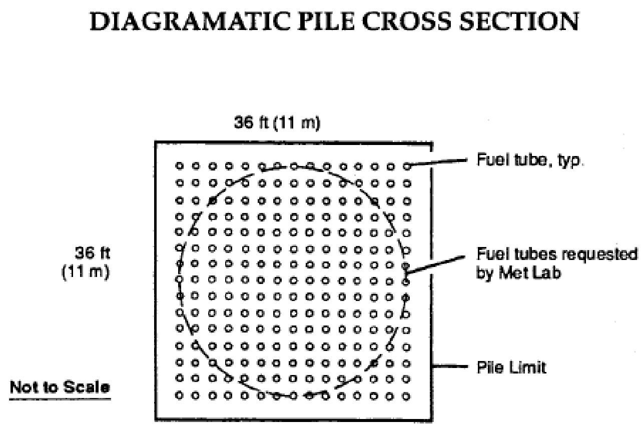
[31] The British delegation on the Gaseous Diffusion plant considered that "*If the Americans met their schedule, it would be something of a miraculous achievement*" (quoted in Hewlett & Anderson, p. 138).
[32] For a detailed account of the barrier design see Hewlett & Anderson (1962, chap. 5).

This was completely unexpected. J. Wheeler, a Princeton physicist that works as a consultant for Dupont on the project, and E. Fermi soon suspected a poisoning problem. They were right : as a result of chain reaction Xenon, a fission product, was produced. It absorbed neutrons, stopped the Pile, quickly disappeared… until the pile starts again. Mobilizing all their resources they quickly validated the phenomena on Oak Ridge prototype pile and proposed solution. Fortunately Dupont, advised by Wheeler and backed by Groves, had deliberately overdesigned the pile, in case of… As Rhodes explains "*If Du Pont had built the Hanford production reactors to Egune Wigner original specifications, which were elegantly economical, all three piles would have require complete rebuilding now. Fortunately Wheeler had fretted about fission-product poisoning. After the massive wooden shield blocks that form the front and rear faces of the piles had been pressed, a year previously, he had advised the chemical company to increase the count of uranium channels for a margin of safety. Wigner's 1500 channels were arranged cylindrically; the corners of the cubical graphite stacks could accommodate another 504. That necessitated drilling out the shield blocks, which delayed construction and added millions to the cost. Du Pont had accepted the delay and drilled the extra channels. They were in place now when they were needed, although not yet connected to the water supply*" (Rhodes, 1986, pp. 559-560 and figure 6 below).

Figure 6. The design of the Hanford reactor (from Thayer, 1996, p. 10)



Uranium was thus added to the pile to overcome the Xenon poisoning effect. The pile went critical on December, 28, 1944, three month after the discovery of a problem that, according to A. Compton, the Met Lab Director, "*led to a fundamentally new discovery in regarding neutron properties of matter*" (in Rhodes, 1986, p. 559).

6.1.4. The thermal Diffusion process (flexibility).

The recurring problems encountered with gaseous diffusion and electromagnetic separation processes lead to a crisis in spring 1944. 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 conducted by Ph. Abelson on the thermal diffusion separation process for the Navy, J. Oppenheimer, Director of the Los Alamos Laboratory, suggested to Groves in april 1944 1) to use the research on thermal diffusion process and 2) to combine the different separation process instead of using them separately. "*Dr Oppenheimer suddenly told me that we have made a terrible scientific blunder*" Groves testified after the war, "*I think he was right. It is one of the things that I regret the most in the whole course of the operation. We had failed to consider thermal diffusion as a portion of the process as a whole*" (in Rhodes, 1986, p. 533). The leaders of the Manhattan project thus realized that the different process can be combined instead of viewing them as competing horses in a race.

On this basis Groves acted very quickly. He appointed a comittee to survey Abelson's work and, in june 1944 contracted with the engineering firm HK Ferguson to build a thermal diffusion plant relying on the K25[33] power plant for electricity supply. They had 90 days to build "*twenty-one duplicates*" (Hewlett & Anderson, 1962, p. 296) of the Navy experimental plant. The production started in early 1945.

6.2. The paths to the A Bomb

6.2.1. Alternative Bomb design at Los Alamos

In march 1943, the building of the Los Alamos Laboratory began on a mesa in San Jose desert, New Mexico. Lead by physicist Robert Oppenheimer, the laboratory was the central node of the Manhattan Project network. Its goal was "*to produce a practical military weapon*[34] *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[35]).

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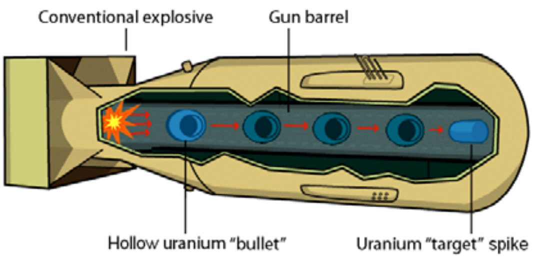
[33] K25 is the code name for the gaseous separation plant at Oak ridge.
[34] Underlined in the original text.
[35] *The Los Alamos Primer* is actually in reprint of the course given by R. Serber to the physicist arriving at Los Alamos in april 1943. The objective was to give them a state of the art in nuclear physics and bomb design.

The goal thus seems straightforward but, like the production of fissionable materials, several design might be possible[36]. Since the beginning of project Y at Los Alamos, three of them were under study :

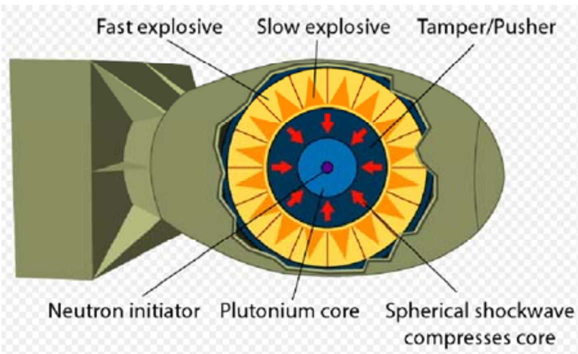
1. The Gun design. This solution is build on years of experience on bomb design. The principle is apparently simple : a piece of fissionable material is thrown to another piece by means of traditional explosives (Figure 7). They then become critical starting the chain reaction. This design has been used in the "Little Boy" bomb dropped on Hiroshima on August 6, 1945.

Figure 7. Gun type fission bomb



2. The Implosion design constitutes a breakthrough innovation in weapon design. In this case, conventional explosives are placed around a plutonium core. When they detonate, they blow inward, the core collapse and thus become critical leading to an explosive chain reaction (Figure 8). This design was used in the "Fat man" bomb dropped on Nagasaki on August 9, 1945.

Figure 8. Implosion type fission bomb



[36] Indeed at the beginning of Los Alamos there was some debates on the type of weapons to be developed. Some were arguing 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 this weapons continues at Los Alamos, until at least February 1945. See Malloy (2008, p. 59-60).

3. The "Super", suggested by E. Teller and E. Fermi, was another radical innovation. Indeed it does not rely on fission but on nuclear fusion. In this design a fission bomb helps to start a fusion reaction in deuterium or Tritium leading theoretically to a much more powerful explosion than with fission bombs. However, the theoretical foundations of such a weapon, based on the analysis of the functioning of stars, were less solid than the fission designs.

These different paths to an atomic bomb had not the same priorities at Los Alamos. Given the current state of knowledge on weapons and its supposed robustness, the "Gun" design was the first priority. Even if the use of this solution with fissionable materials raised important scientific and engineering questions on interior ballistics, the shape of the uranium and plutonium parts, the explosives to be used, detonation, and so on, it was believed that the gun solution could be used for 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.

However, Oppenheimer and Groves decided at the beginning of project Y that they cannot 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 back up for the plutonium gun but, as they soon discovered, it can also be a much more efficient assembly method than the crude "gun" design. A third group, smaller and with much lower resources, was also assigned to work on the "Super". It was clear for Oppenheimer and its colleagues at the beginning of the project that this third design was a too radical innovation to be ready to use during this war. However its potential was so high that theoretical work on this solution was conducted at Los Alamos during the entire project (in part due to the obsession of E. Teller with this design).

We thus find at Los Alamos the same managerial philosophy than in 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.

6.2.2. 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 exhibit a “spontaneous fission” rate much higher than uranium. This means that the two parts of the gun has to get together at very high speed. Otherwise the chain reaction starts before the two parts collides (and thus reach the critical mass) and the bomb “fizzles” (i.e. pre-detonate and does not explode).

While identified at the beginning of the project this spontaneous fission phenomenon was not mastered by the scientist because plutonium was a completely new material. So measuring and analyzing spontaneous fission was an important part of the work at Los Alamos (under the supervision of E. Segré). This was particularly difficult since the scientist had to find the methods and tools to analyze this phenomenon, at a time when plutonium was available in submicroscopic quantities. The problem turn to a crisis when Los Alamos received the first reactor-produced[37] samples of plutonium in april 1944. They exhibit a spontaneous fission rate five times higher than the sample they already had, which were produced with another process (the Berkeley Cyclotron). Research on this question continues until July but the results were desperately the same. The conclusion was clear to Groves, Oppenheimer and their colleagues : the plutonium gun would never worked. This lead to a crisis at Los Alamos. The entire plutonium path to the bomb (and the millions of dollars already spend) can indeed be cancelled… at a time when the separation of U235 encountered huge technical difficulties.

To overcome this crisis Oppenheimer completely reorganized the laboratory. In july 1944, the design of the gun was well advanced and, even if engineering questions remains important, under control, at least for uranium[38]. Furthermore the research and experiments on implosion had leads to important findings (particularly, J. Von Neumann suggestions during the fall of 1943). So, in two weeks, Oppenheimer redeploy the resources of Los Alamos and gives it he first priority. Now the entire lab was

[37] From the X10 air-cooled prototype reactor at Oak ridge.
[38] “*It was fortunate that the greatest problems of guns, nuclear physics and chemistry could all be solved during the first year of project Y, because the spontaneous fission crises required an all-out focus on implosion during the second year”* (Hoddeson & al., 1993, p. 411).

focused on this question to save the plutonium path[39]. Two new divisions were created that borrowed people from the previous divisions[40] :

- the Gadget (G) Division, lead by R. Bacher, was to investigate implosion experimentally and eventually design a bomb.
- The Explosives (X) Division lead by G. Kistiakowsky, was devoted to design the high explosives components of the implosion bomb and develop methods of detonating them.

This constitutes an important change since the organization moved from one organized primarily around scientific and engineering tasks, to one where "*the organizing principle was whether work applied to implosion or the gun program*" (Hoddeson & al, 1993, p. 247). In other words the work was more and more organized around projects[41]. The goal of the reorganization 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 challenge was huge. Even if the research and experiments has produced crucial insight, some were questioning the possibility of an implosion design. The most difficult problem was symmetry : to ensure the start of the chain reaction, the inward collapse of the plutonium core must be absolutely symmetric. This has never been done before and explosives were not design for this purpose. Furthermore since this was a breakthrough innovation, the available knowledge was almost inexistent. Los Alamos had thus to explore simultaneously the hydrodynamics of implosion, the design of explosives 'lens" around the core, the design of the initiator that will release the neutrons necessary to start the chain reaction (see figure 7), the

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[39] However, in accordance with his managerial philosophy, Oppenheimer insisted not to completely abandon the plutonium gun. In a letter to Groves dated July, 18, 1944 he explained that "*Since the results outlined above* [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.*" (quoted in Hoddeson & al., p. 243).

[40] Details are available in Hawkins, 1961chap. 9 and Hoddeson & al. 1993, chap. 14.

[41] This illustrates the flexibility of Los Alamos which was in itself a fascinating organization "*whose structure was by nature ephemeral; experiments and responsibilities changed overnight as priorities that the war gave to the project changes*" (Hoddeson & al, 1993, p. 247. See also Thorpe & Shapin on the *normative uncertainty* at Los Alamos and the role of Oppenheimer in its functioning). The laboratory experienced a sequence of reorganization during the war moving from an academic-like laboratory to a huge scientific-industrial complex (it employs almost 9000 person at its peak). As times goes on the laboratory 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 1994 and throughout 1945 (see Hewlett & Anderson, 1962, p. 313-315; Hoddeson &al, 1993 chap 14 to 16 ; Thorpe & Shapin, 2000).

electronics to coordinate the detonators around the bomb, and so on… and to keep in mind that they must design a practical weapons[42]. For each question the scientist and engineers of the lab used multiples and overlapping approaches to enrich their understanding of the phenomenon at work, increase the likelihood of success and save time. For example seven experimental diagnostics were used to understand the physics and engineering problems of implosions. (Hoddeson & al., 1993) . They also relies heavily on small scale models and numerical analysis to run the necessary experiments[43].

This huge scientific and engineering finally lead to a radical innovation in weapon design : the implosion bomb[44]. However, uncertainties were so great on this new device that Groves finally, but reluctantly, approves Oppenheimer request to test the bomb, despites the huge cost of such an experiment.

6.2.3. The Trinity test (july 16, 1945) and the use of the Atomic bomb.

The Trinity test, organized in July 16, 1945 marked the dawn of the nuclear age. This day, 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.

What is interesting to us is the reactions to the test since it questioned the clarity of the goal. The most famous reaction was Oppenheimer's. In an interview on the BBC in 1965 he commented : « *We knew the world would not be the same. A few people laughed, a few people cried, most people were silent. I remembered the line from the Hindu scripture, the Bhagavad-Gita. (…) 'Now, I am become Death, the destroyer of worlds.' I suppose we all thought that one way or another* ». Reading Rhodes (1986, chap. 18) or Groves (1962, appendix VIII p. 433) it is striking to note the people's reaction to the test. It was literally a revelation[45]. They suddenly realized the true power of atomic bombs and their revolutionary nature.

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[42] A detailed account of the entire implosion program is found in Hoddeson & al. 1993.

[43] Ten IBM calculators were installed at Los Alamos and used on implosion studies.

[44] The design was frozen very late, probably on February 28, 1945. Oppenheimer then created the "cowpuncher committee" to oversee the final phase of the work on implosion (see Hoddeson & al, 1993, chap. 15 & 16).

[45] « *No one who had witnessed the test was in a frame of mind to discuss anything. The reaction to success was simply to great. It was not only that we had achieved success with the bomb; but that everyone – scientists, military officers and engineers – realized that we had been personal participants in, and eyewitnesses to, a major milestone in the world's history and had a sobering appreciation of what the results of our work would be"* (Groves, 1962, p. 288).

As Groves explains in his account of the project : "*With the war end, or about to end, many of our people began to discuss the future consequences of our work. The thoughts that they expressed were not particularly new, but until then, there had been little time to spent on nonessential conversation. Since 1939, they had been busy. Now they all realized for the first time that atomic energy was a fact and not a theory and they realized too, that a nuclear war could never be fought on this earth without bringing disaster to all mankind. This had been immediately evident to everyone who witnessed the Trinity test. (…) we had solved the problem of ending the war, but in so doing we have raised many unknowns*" (Groves, 1962, p. 354). He then quotes R. Oppenheimer who, after receiving the Certificate of Appreciation from the Secretary of War for the work accomplished at Los Alamos, said on October 16, 1945 : "*Today that pride must be tempered with profound concern. If atomic bombs are to be added as new weapons to the arsenals of warring world, or to the arsenals of nations preparing for war, the time will come when mankind will curse the names of Los Alamos and Hiroshima. The peoples of this world must unit or they will perish. This war that has ravaged so much of the earth has written this words. The atomic bomb has spelled them out for all men to understand. Other men have spoken them, in other times, of other wars, of other weapons. They have not prevailed. They are misled by a false sense of human history who hold that they will not prevail today. It is not for us to believe that. By our works we are committed to a world united, before this common peril, in law, and in humanity"* (ibid, p. 355).

In fact concerns about the military, diplomatic and moral significance of the atomic bomb have been a major subject of debate among the leaders (V. Bush, J. Conant, H. Stimson, R. Oppenheimer…) of the Manhattan Project since its inception. All were aware of that the A-Bomb was not just another new weapon. This raise fierce debates among the project and the US government. Indeed, far away from the official history of the bomb use[46], the existence of an atomic weapon raise complex question such as : "*how should the bomb be integrated into American wartime diplomacy? What role should the Soviet Union play in the future development and control of atomic energy ? was it legitimate to use the bomb again cities and civilians ? Should Japan be*

[46] This official history was stated in an essay ("The decision to use the atomic bomb", *Harper's Magazine*, February, 1947) signed by H. Stimson but in fact co-authored by several members of the project. It explains that the bomb was primarily used to avoid a bloody invasion of Japan and shorten the war and thus saving thousands of American and Japanese lives.

offered a warning ? Were there opportunities to hasten the end of the war, perhaps before either the bomb or an invasion was necessary ?" (Malloy, 2008, p. 163). In *Atomic Tragedy*, Sean Malloy remarkably demonstrate that given the inherent complexity of this questions, the pressure to end the war, the technical decision already made[47] and the momentum gained by the 2 billion dollars Manhattan Project, this questions were never really solved[48]. H. Truman and his advisers (including Oppenheimer) finally choose a strategy of immediate use on primarily civilian targets without warning and without any consultation of allies or plans for international control. As Oppenheimer observed in November 1945, "*the pattern of the use of atomic weapons was set at Hiroshima. They are the weapons of aggression, of surprise, and of terror. If they are ever used again, it may well be by thousands or by ten of thousands*" (in Malloy, 2008, p. 187). That the atomic bomb was not just another weapons was clearly stated by Secretary of War H. Stimson in a memorandum addressed to the President on September 11, 1945 in which he recognize that this was indeed a complete change in international relations that cannot fit into "old concepts" [49]. The Manhattan Project had thus produced the most important revolution in weapons since war exist[50].

Therefore we cannot agree that the objective was clear. The true implications of the A Bomb appeared in the course of the project. It is thus quite rare to see a project team write reports on "the political and social problems of the A Bomb"[51] or raise question such as "will the bomb ignite the atmosphere". This tension was at his

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[47] The decision to stop the design of underwater weapons and to focus on a bomb considerably reduce the range of the available choices. Indeed, given the need for high altitude bombing and the resulting low accuracy of the bomb, cities became the primary targets of the bomb. See Malloy, 2008. On the larger question of the interplay between technical and political decisions see the classic from MacKenzie (1990).

[48] It is very interesting to note that the difficulty to define a military strategy concerning nuclear weapons persisted after world war II. On this question see Rosenberg (1983).

[49] "*If the atomic bomb were merely another though more devastating military weapon to be assimilated into our pattern of international relations, it would be one thing. We could then follow the old custom of secrecy and nationalistic military superiority relying on international caution to prescribe the future use of the weapon as we did with gas. But I think the bomb instead constitutes merely a first step in a new control by man over the forces of nature too revolutionary and dangerous to fit into the old concepts. I think it really caps the climax of the age between man's growing technical power for destructiveness and his psychological power of self-control and group-control his moral power.*" (Available online at http://www.nuclearfiles.org/menu/library/correspondence/stimson-henry/corr_stimson_1945-09-11.htm). This radical shift in international relations has been later formalized by T. Schelling in his famous *Strategy of Conflict* (1960).

[50] In this perspective it is interesting to note the persistence of "old concepts" in the debates of the Target Committee. Indeed they begin by discussing the opportunity to bomb plants or arsenals. However it becomes rapidly clear that atomic weapons were designed to destroy cities, not plants. It thus decided after its final meeting on may, 28, 1945 "*to endeavour to place the first gadget in center of selected city*" (see Malloy, 2008, chap. 5). My thanks to D. Whitney for this insight.

[51] Known as the Franck report (june, 1945).

maximum during the discussions on whether or not use the bomb against Japan (see Rhodes, 1986, chap 19 and Malloy, 2008).

7. Discussion. Toward a Project Management of Exploration

The Manhattan Project is unquestionably one the greatest scientific and technical achievements ever realized (and one of the most terrifying, see Rhodes, 1986, chap. 19 on the absolute horror of nuclear bombing). In less than three years, it succeed in designing a revolutionary innovation, starting with mostly theoretical knowledge and ending with the construction of an entire industry.

Furthermore he outlined an original and innovative managerial model which echoes contemporary research on the management of innovation and, more precisely, exploration i.e. situations were neither the goal nor the way to reach it are known (see Lenfle, 2008 for an introduction). In this last section, we want to discuss the nature and foundations of this model in the light of contemporary research on project management.

7.1. The power of projects

The first lessons of the Manhattan Project is obviously the power of project management. The ability of projects to leverage existing organizations to reach apparently unreachable objectives explains the success of the venture. As Groves explains "*The project made a maximum use of already existing agencies, facilities and services – governmental, industrial and academic. Since our objective was finite, we did not design our organization to operate in perpetuity. Consequently, our people were able to devote themselves exclusively to the task at hand, and had no reason to engage in independent empire building*" (p. 414). The setting up of a project gives to the thousands of persons on the project an objective, a framework for action : " *there was a positive, clear-cut, unquestioned direction of the project at all levels. Authority was invariably delegated with responsibility, and this delegation was absolute and without reservation. Only in this way could the many apparently autonomous organizations working on the many apparently independent tasks be pulled together to achieve our final objective*" (Groves, 1962, p. 414).

The existence of dedicated teams and leaders, fully backed by the US government[52], was of the utmost importance. Leslie Groves and Robert Oppenheimer, at the general level[53], coordinates the entire network of the Manhattan Project and relies on hundreds of smaller dedicated teams that together formed a very complex and flexible network[54]. As we have seen the ability of the project to reconfigure its resources was fundamental to cope with uncertainty. The reorganization of the Los Alamos laboratory to design an implosion weapons in august 1944 or the decision to add the thermal diffusion process to separate uranium are the most emblematic cases of the flexibility of projects to manage unforeseeable uncertainties.

The objective very probably couldn't have been achieved without the setting up of a project. The Manhattan Project thus constitutes another example of the power of dedicated and autonomous project teams to manage radical innovations (known in the literature as "tiger teams" (Wheelwright & Clark, 1992) or "skunkworks" (Rich, 1994). Even the ambidextrous model insist on the autonomy of exploration units see Tushman & O'Reilly, 1996[55]). Indeed the strategic role of a dedicated and empowered project management structure seems to be a common feature of this huge and highly uncertain projects. If we limit ourselves to postwar military projects, this organization has been fundamental in the success of strategic nuclear weapons projects like Atlas (the Western Development Division of the USAF under the direction of General B. Schriever; see Hughes, 1998 and Johnson, 2002) and Polaris (the Navy's Special Projects Office directed by Admiral Rabborn; see Sapolski, 1972).

This demonstrate the efficiency of projects to manage exploration and radical innovations. But at the same time, the model of project management implicit in the Manhattan Project is, as we will see, far away from « best practices » of project management publicized by the Project Management Institute. We want to insist on three

[52] L. Groves quickly obtained the highest priority rating for the project. He reported directly to a Top Policy Group composed by H. Stimson, Secretary of War, G. Marshall, Joint Chief of Staff, V. Bush and J.B. Conant.

[53] There was actually different project managers for the different part of the project. To name a few : Crawford Greenewalt managed the Du Pont part of the project and was definitely one of the key personnel of the Manhattan Project (see Hounshell & Smith, 1988); E. Lawrence was the leader of the electromagnetic separation process; P. Keith leads the gaseous-diffusion effort, and so on. Each played a crucial role in the overall success of the project.

[54] This was complicated by the strategy of compartmentalization adopted by L. Groves for security reasons. In this organization very few people have a global understanding of the project. It is interesting to note that this politics didn't apply at Los Alamos where R. Oppenheimer, on the contrary insist on intensive communication and collaboration between the different departments of the lab.

[55] With the exception that this models refers to project management organizations within firms; which is not the case of the Manhattan Project.

point that formed the basis of a project management model adapted to radical innovation management : the central role of experimentation, parallel strategies and the problem of "expansion" management.

7.2. Experimentation and parallel strategies in the management of exploration

The existence of unforeseeable uncertainties is probably the most important characteristics of the Manhattan Project. This is typical of innovation management which is, as Van de Ven & al. (1999), a *learning by discovery* process. In a context where neither the goal nor the actions to be taken are known, Van de Ven & al. demonstrate that "*an expanded definition of learning* [is needed] *that examines not only how action-outcomes relationships develop but also how prerequisite knowledge of alternative actions, outcomes, and context emerges. This expanded definition distinguishes learning by discovery from learning by testing. In particular learning by discovery* (…) *is an expanding process of discovering possible action alternatives, outcome preferences, and contextual settings.*" (p. 81). The difficulty is thus not only to adapt to events but to discover what is to be learned. In situations of exploration it is sometimes very difficult to interpret the results. People can be trapped in a situation where the are "*learning more and more about less and less"* as in the canning problem depicted in section 6.1.1[56].

The way to manage this situations is thus a central concern for researchers on project and innovation management. We think that the case of the Manhattan project provides very interesting examples and insights that echoes contemporary research on this question. Indeed how to cope with unforeseeable uncertainties and proceed in the dark is a central concern for firms in fast-paced and ever-changing contemporary competitive environments (Brown & Eisenhardt, 1998; Doz & Kosonen, 2008). Parallel strategies and experimentation are the main strategies we want to develop in the following sections.

7.2.1. Experimentation

The central role of experimentation is a striking features of the Manhattan Project. Uncertainty indeed raise an important problem for action : what to do ? where

[56] Van de Ven & al. explained that "*a central problem in managing the innovation journey is determining whether and how to continue a developmental effort in the absence of concrete performance information*" (p. 67).

to begin ? Studies on innovation and design management (Eisenhardt & Tabrizi, 1995; Lynn & al. 1996; Brown & Eisenhardt, 1997; Van de Ven & al., 1999; Lenfle, 2001; Thomke, 2003; Le Masson & al. 2006; Loch & al, 2006) underscore the need for action in the case of unforeseeable uncertainties, which will allow problems and solutions to be discovered.

The story of the Manhattan Project is thus characterized by its reliance on experimentation to discover and (try to) solve problems. In every part of the project experimentation plays a central role : in the gaseous diffusion barrier design, to solve the canning problem, to understand the dynamics of implosion, and so on. Hoddeson & al (1993) analysis of the practices of engineers and scientists at Los Alamos illustrates this point. They particularly underlines the role time pressure in the definition of an "*empirical problem-solving methodology based on systematic trial and error rather than thorough analysis. Traditional analytic methods were simply too slow. Among the particular techniques that the Los Alamos physicist and chemist used frequently, in combination with more traditional scientific ones, were the Edison approach of trying, in the absence of good theoretical guidance, one after another materials; (…) overlapping approaches in which multiple approaches were taken simultaneously to a specific problem in recognition that any one could be incomplete and uncertain by itself but that together the might be used to build up a consistent picture; the small-scale model study to save time and precious materials; (…) and numerical analysis now for the first time extensively done by computing machines* (p. 9-10[57]). The case of implosion weapons design, where scientists and engineers have used simultaneously seven experimental diagnostics to understand the physics and engineering problems of implosions, is typical of this probe and learn methodology.

When people don't know what to do and/or expect experimentation is the sole solution and the project becomes a probe and learn process (Lynn & al. 1996). This is why Leslie Groves emphasizes several times that "*a wrong decision that brought quick results was better than no decision at all*"(in Hewlett & Anderson, 1962, p. 181). Sketching out a plan of action must therefore be seen as a set of temporary hypothesis on the design space to be explored, allowing the learning process to begin. In this context, the design of the experiments that will prove or disprove the initial hypotheses occupies a crucial place in the management of the project firstly to create knowledge

[57] Illustration of this methods is available in Hoddeson, specifically the chapters on the implosion program.

and, secondly, because they constitute a key coordination element, inasmuch as no other timescale is applicable, unlike with development projects.

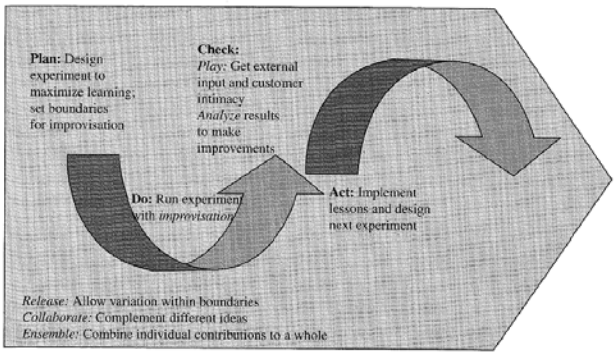
Experimentation, which has recently received more attention from scholars (Thomke, 2003), is thus central in the management of exploration projects. The managerial consequence is straightforward : since the team progressively discover problems and solution, it is impossible to establish a reliable plan or work breakdown structure at the beginning. “Probe and learn” means “plan, organize and continuously replan and reorganize” the project. The case of the Manhattan Project thus underlines the need to develop an model of project management adapted to innovation. We thus agree with contemporary thinking on project management that emphasizes an adaptive view of project management proposed by Shenhar & Dvir (2007 see figure 9 below) and the central role of learning in projects[58] (Loch & al. 2006). In this perspective the basic process of project is a Plan / Do / Check / Act cycle “*embedded in a process of a stream of learning events*” (Loch & al. 2006, p. 118 and figure 10 below). The criteria used to manage the project then switch from the traditional time/cost/quality index to the number of experiments carried by the team and the knowledge gained from them (see Thomke, 2003 on *enlightened experimentation*). The increasing returns of iterations becomes a central criteria to evaluate the project “progress” (Lenfle, 2001 & 2008).

Figure 9. The adaptive approach to project management (Shenhar & Dvir, 2007).

From traditional to adaptive project management		
Approach	**Traditional PM**	**Adaptive PM**
Project goal	Getting the job done on time, on budget, and within requirements	Getting business results, meeting multiple criteria
Project plan	A collection of activities that are executed as planned	An organization and a process to achieve the expected goals and business results
Planning	Plan once at project initiation	Plan at outset and replan when needed
Managerial approach	Rigid, focused on initial plan	Flexible, changing, adaptive
Project work	Predictable, certain, linear and simple	Unpredictable, uncertain, nonlinear, complex
Environment effect	Minimal, detached after the project is launched	Affects the project throughout its execution
Project control	Identify deviations from plan, and put things back on track	Identify changes in the environments, and adjust the plans accordingly
Distinction	All projects are the same	Projects differ
Management style	One size fits all	Adaptive approach, one size does *not* fit all

[58] A learning strategy emphasizes the iterative modification of the project goals as new informations emerges from experiments conducted sequentially.

Figure 10. Projects as experimental learning process (Loch & al., 2006, p. 119)



Design principles of the process:
- Recognize failure as a learning opportunity
- Experiment as early as possible
- Organize for frequent and rapid experimentation
- Integrate multiple experiment technologies

Figure 5.6 An experimental learning process

7.2.2. Parallel strategies

The other important specificity of the Manhattan Project, its use of parallel strategies to manage uncertainty, is probably clearer if we keep this view of project as experimental processes in mind. Abernathy & Rosenbloom, in an old study of R&D projects (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*" [59]. The benefits of this 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 & Rosenbloom distinguish this approach from the sequential approach i.e. "*commitment to the best evident approach, taking up other possibilities only if the first proves unsuccessful*"[60].

[59] In the literature there may be a problem on the definitions of the terms. Sometimes the word "concurrency" is used. But this can be confusing. For example, Johnson defined "concurrency" as a method in which engineers « *develop components in parallel with each other, and then integrate them into systems* [e.g. aircraft, missiles] » (2000, p. 96). Later he insist on the strategy, developed by B. Schriever (who proposed the word "concurrency") for the Atlas Project, that consists in developing separate technical solutions, for the same component, to cope with uncertainties. We think that Johnson melt two different concepts. Indeed what he defined as concurrency is now called "concurrent engineering" (see Nevins & Whitney, 1989) and we can perfectly imagine concurrent engineering without parallel strategy.

[60] In the remaining of their paper they identify two different parallel strategy differing by the project phase where they are used. What they called the *parallel synthesis strategy* is found in the earlier phase of

The parallel approach thus increased the probability of success. Indeed, if one of the chosen path appears unfeasible, another one is available. Contemporary research on project management, specifically the work from C. Loch and colleagues (Pich & al., 2002; Somer & Loch, 2004; Loch & al. (2006), refers to this strategy as *selectionism*. In this approach, given unforeseeable uncertainties and/or complexity[61], it is impossible to predict the unfolding of the project. The best strategy may thus be to try different approaches simultaneously and to see *ex-post* which one works best. Groves decision to explore and implement simultaneously plutonium and uranium separation processes and, in this later case, to try two and later three different approaches constitutes an illustration of this strategy. So is Groves/Oppenheimer decision to do research on alternative bomb designs. The obvious reward of this approach lies in its power to manage the unexpected. The switch from gun design to implosion design to overcome the plutonium spontaneous fission crisis, and the late combination of the different uranium separation processes, constitute perfect example of this strategy that ultimately allows the Manhattan Project to succeed. It keeps options opens[62].

7.2.3. Discussion

What is very interesting in Loch & al. works is their framework to analyze which strategy is best suited for a project confronted with unforeseeable uncertainties. They begin by identifying four different strategies for the project according to the use of learning and/or selectionist strategies (figure 11 below).

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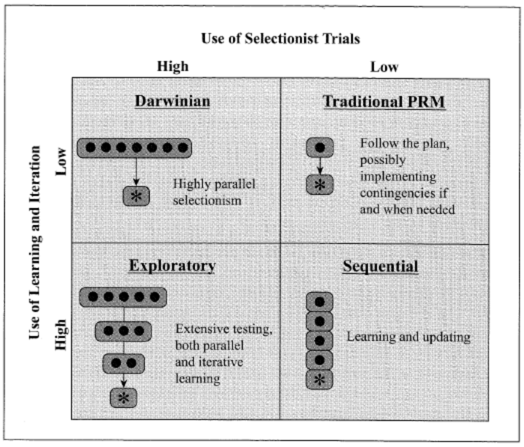
a program. The goal, at this stage, is to enrich the learning process when confronted to important uncertainties. It is a means "*of gaining information and maintaining options so that the best path may be selected for subsequent development*" (p. B-487). On the contrary the *parallel engineering strategy* occurs in a later stage of the development process. The goal is then 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 delay (in spending, reputation and opportunity costs). This is the case studied in their paper.

[61] Defined here as interactions among the different dimensions/components of the project. Discuter plus finement les travaux de Loch.

[62] Overdesign is probably part of this strategy since it allows the team to adapt to the unexpected. The Xenon poisoning case illustrates this situation.

Figure 10. Four basic scenarios of learning and selectionism (Loch & al, 2006, p. 146)



They thus develop a framework to help the project managers to choose among the four 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 compared to parallel trials. The result is presented in figure 12 below. It shows 1) that selectionist and learning strategies can be combined in what they called exploratory strategies, 2) that selectionist strategy are best suited when the complexity of the project is high and the cost of delay and learning are high compared to the cost of parallel trials (the *Darwinian selection* box).

Figure 12 : Value comparison of learning and selectionism with complexity and relative costs differences (Loch & al. 2006, p. 154).

	Relative Cost	
	Learning and delay more expensive	Parallel trials more expensive
High complexity (many interactions)	Darwinian selection *ex Post* Selection	Sequential learning or reduce complexity
Low complexity (few interactions)	Exploratory *Early* selection and learning	Sequential learning

Furthermore, in their analysis of selectionism, Loch & al. emphasizes the need

- To organize communication among the different teams leading the parallel alternatives

– To choose the trials that leads to robust results i.e. "*those that emerges from different trials and hold under a variety of conditions*" (p. 136). The sooner options are selected the best it is since cost are lower at the beginning of a project.
– To leverage the benefits from non-selected outcomes by exploiting the knowledge they have created.

Their goal is to ensure the commitment of the resources to the chosen options i.e. the one that emerge as the best given the unfolding of the project and its environment.

This framework is useful to characterize and discuss the strategy followed by the Manhattan Project. Furthermore, as we will see, this case, offers interesting insights on the management of parallel and learning strategies.

First, as we have shown, the project uses an exploratory strategy that combines extensive experimentation and the use of both parallel and iterative learning strategies. But, at the same time, it is closest to the *Darwinian selection* box of Loch's framework. It was unquestionably a project of very high complexity since the available knowledge on the process and the "product" was mostly theoretical at the beginning of the project. Furthermore the different processes involved many interacting parameters. On the other side the costs of parallel trials were obviously very high. However, given the wartime context, the utmost importance of delay[63] and the almost unlimited resources available, the Manhattan Project adopt a massive parallel strategy. Groves strategy was clear from the beginning "*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 & Anderson, p. 181). Thus the Manhattan Project confirms that, in case of unforeseeable uncertainties, urgency and with important resources, a project does not always have to chose between different options.

There is indeed two hypothesis in the selectionist framework. Firstly the goal is to select the best option and secondly, information is available to select the right solution. The ideal case is provided by Japanese consumer electronics company which, in the 90's, launch several products on the market and select ex-post the best i.e. the one with the better commercial results. In this situation the choice is based on "perfect" information. However in most case the choices are based on partial information. Loch &

[63] What Shenhar & Dvir (2007) called "Blitz projects".

al. shown that, in this case, early selection in complex projects may lead to an increase of project complexity[64] and of the associated costs and delay. They then favored sequential learning (figure 12).

The Manhattan mostly confirm this framework. When there is no way to chose between solutions given the radically innovative nature of the projects and the lack of knowledge, the project can adopt a parallel strategy i.e. decide to pursue all the options to their completion and to postpone the choice. Thus, in the Manhattan case, the choices occurs after the bombing of Hiroshima and Nagasaki. L. Groves then made important decisions : he decided to abandon the gun design (inefficient compared to implosion[65]), to close the Thermal Diffusion plant (too costly), to partially stopped the electromagnetic diffusion plant (for cost and low efficiency reasons). He thus favored the implosion design, plutonium and the enrichment of uranium with gaseous diffusion. But does it means that the Manhattan project was purely Darwinian ?

First we cannot say that the choices done by Groves were based on complete information. The available knowledge on atomic energy remains incomplete, even after the Manhattan Project completion. So the choices were not definitive. They were dictated by the available knowledge… and the need to reduce the spending quickly with war's end and probable congressional hearings[66].

Secondly, it could be difficult to distinguish clearly between selectionism and experimentation. As we have said, the Manhattan Projects uses both strategy at all levels of the project from the resolution of scientific and technical problems to the global managerial strategy. In this case each trials uses sequential learning and multiple approaches… and there was parallel trials. This is a much more complex case than, for example, Japanese electronics mentioned earlier. In this latter case there was many trials but each trial was closer to a development project.

Furthermore, what is interesting in the Manhattan case is its combination of different options, an infringement of the *Darwinian* logic. The decision to use Thermal diffusion provides a useful example of the late addition of a new trial and of the combination of the different trials to reach the objectives of the project. We thus see

[64] *"In the case of early section, where only partial information is revealed, (…) selectionist trials will not be favored in complex projects. (…) the reason is that making wrong assumptions about an unknown project influence "disturbs" a complex project more than a simple project. Through the many interactions in a complex project, the error in one influence factor has wider repercussions and degrade the quality of the selection choice."* (Loch & al. 2006, p. 151).

[65] Even if the power of the two bombs was similar, the implosion device consumes less fissionable materials and its "yield" (the percentage of material used during the explosion) was better.

1. that even in the case of high complexity and high cost of delay, learning and selectionism can be used simultaneously (a case of exploratory strategy *without* early selection) ;
2. that combination of the different trials to reach the project goals can be an interesting options to reach the project objectives. In this perspective the different trials can be considered as different experiments;

This underlines the fundamental importance of the management of the different trials. When using this strategy the management of the project have to be aware of the progression of the different path and stay ready to change their order of priority[67], add new options, combine the trials, reopen previously closed solutions (remember footnote 38 p. 27). Flexibility[68], defined here as the ability of projects to reconfigure its resources, constitutes a key success factors and must be a central concern for managers. The functioning of Los Alamos and its reorganization during the summer of 1944 provides a perfect illustration of the management of exploration projects. Oppenheimer's redeployment of resources between implosion and gun designs demonstrate that the pursuit of parallel trials does not mean that they are "independent". This constitutes an important challenge for project managers (see Loch & al. 2006 on this questions).

Thus in situation of urgency and unforeseeable uncertainty it could be very interesting to keep all the options open until the end of the project. However we think that in this situation of breakthrough innovation another process is at stakes. Indeed the goal is not so much the successful completion of the project that its ability to give birth to an entire range of new products, some speaks of lineages (Le Masson, 2001), based on the accumulated knowledge. The completion of the project in thus only a first step in a broader knowledge-creating or dynamic capability building process (Nonaka, 1994; Iansiti & Clark, 1994). We now turn to this question.

[66] During the war the project was completely secret, even for congressmen.
[67] This is actually what happens on the Manhattan project since the electromagnetic separation was first perceived as the best choice but was finally ovetaken by gaseous diffusion.
[68] or Agility based on resource fluidity to use Doz & Kosonen concept, 2008.

7.3. Managing expansion.

There is in our view another reward to the parallel strategy : it considerably increases the richness of the exploration process and thus of learning. What, in Abernathy & Rosenbloom framework, constitute an *indirect benefit* of the parallel strategy is, in our view, a central characteristics of innovation management. Indeed, one of the most important challenge of this type of projects is to explore a very complex design space (see Hachuel & al, 2006; Loch & al, 2006; Baldwin, 2007 on Design spaces). In this perspective a strategy focused on a few design parameters restrict its ability to understand the processes at stake. Whereas a strategy that explores simultaneously the design space allows the team to progressively build a global understanding of the field[69]. This will allow the project to succeed, but more generally this is the only way to build knowledge and define a strategy on the emerging field. In our view this represent a fundamental shift in the philosophy of project management since delivery, while remaining an important objective, is not the only objective of project management.

Indeed one of the most important specificity of the Manhattan Project lies probably in its "generative" nature. There is indeed a fundamental difference between a Development Project and what we observe here. In a Development situation, as already explains, the knowledge base associated with the project is clearly defined at the beginning. The main objective of the firm is thus to organize the convergence of the project toward its target, usually defined in quality, cost and lead time.

The unfolding is completely different in the Manhattan case. Indeed the more the project progress toward its goal, the more it discovered new path, new solutions, new problems, potential applications and so on. Of course it converge to its final goal, in this case the design and delivery of the atomic bomb. But, in our view, the result was much more complicated than that.

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[69] This has been identified in the classic paper of Marples on *The Decisions of Engineering Design* (1961). Page 64 he explains that "t*his methods* [i.e. 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 attempted in order to get, through comparison of subproblems, a clear picture of the "real nature" of the problem*." Note that Marples was studying nuclear reactor design.

Let's first consider the obvious result, the atomic bomb. The final result is not what was expected at the beginning since the first design, the "gun" weapon was unsuitable to use with plutonium. The project thus switched to the implosion design. So result was not one but two completely different bombs. But there was more than this. As we explains, a third design was studied by the project : the "super". Even if it was quickly given a lower priority, research on this question never stopped at Los Alamos. A similar process was at work in the uranium separation part of the project : electromagnetic separation was the first, apparently most promising solution. Simultaneously the work on gaseous diffusion started. Both goes into a crises in spring 1944. Then comes the thermal diffusion process and the combination of the processes.

If we use the language of contemporary design theory (Hatchuel & Weil, 2003) the design space explored by the Manhattan Project was "generative" in nature i.e. in perpetual expansion (Hatchuel, 2002). The more you explore it, the more options you discover[70]. This clearly never happens in a Development project and has important managerial implications.

First the teams face an risk of overload i.e. they have to chose between the different options. The Manhattan project decide not to choose among the different options, even if it assigned quickly a lower priority to the "super", given its largely theoretical nature at his time.

This underlines the second important specificity of this kind of project. They are producing much more knowledge than they need and use. In this perspective their goal is to define what will be launched first, and what will comes next. They are thus building the foundations for "lineages" of weapons (Chapel, 1997; Le Masson & al, 2006) : in this case from gun fission to thermonuclear weapons. In other words, following here Le Masson, Weil & Hatchuel [40], we can identify four different results for this projects :

1. Concepts that, after development, becomes commercial products
2. Concepts that have been explored but adjourned due to lack of time or resources
3. New knowledge that has been used during the exploration and can be re-used on other products (e.g. components, technical solutions, new uses, and so on)

[70] Remember Smyth statements 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*".

4. New knowledge that has not been used during the exploration but can be useful for other products.

We think this represent an important shift in the philosophy of project management since the knowledge management dimension becomes central. In this perspective convergence, which is the usual goal of project management, is not the single objective. Indeed, we have to include the fundamentally diverging nature of the innovation process (Van de Ven, 1999) in the management of exploration project. This is why, in a previous paper (Lenfle, 2008) we underline the dual nature of the performance of this projects which encompass both “products” and knowledge. This knowledge dimension, generally considered as a by-product of project, that becomes important only after the project completion (see Lenfle, 2008 for a discussion), is central *while the project is being carried out*. It leads to the constant adjustment of project objectives and is the foundations for the creation of lineages of project (see Lenfle & Midler, 2003 & Lenfle, 2008 for a contemporary case) whereas in Development situations projects are frequently “one shot”.

The Manhattan Project provides a perfect example of this process since it creates an incredible knowledge base in various fields that will expand in the following years. It can be considered the womb of the nuclear industry (military first[71] but also probably civilian, even if we don’t know any work on this question[72]). It is very interesting here to follow the analysis of Hoddeson & al (1993, p. 416). As they explains “*the application of the Los Alamos at the nuclear weapons laboratory was direct and massive*”. Rosenberg (1983) thus shows that “*the weapons in the American stockpile had grown increasingly sophisticated and powerful in the last years of the Truman Administration. The ‘nominal’ 20 kiloton yield of the Mark 3 bomb* [an evolution of the Fat Man design] *was multiplied by 25 times between 1948 and 1952. These included advances in design, composition, stability, and power of the high explosives used to detonate a fission core, and improvements in mechanics, structure and composition of the fissile pit itself* [i.e. the plutonium core]. *The pit improvements included development of ‘composite’ U-235 and plutonium cores, and exploitation of the*

[71] A course was organized at Oak Ridge immediately after the war, *“where the engineers of some of the bigger companies, as well as some military officers, could be trained in what might be termed the practical end of atomic engineering*” (Groves, 1962, p. 387). H. G. Rickover, the future “father” of nuclear submarines, was among them. Similarly the Chemical Engineering Department of MIT created a course on Atomic engineering in 1946.

[72] Groves explains in his book that after the war he made the isotopes produced by the MED available for research in Medicine, Biology, Agriculture, and so on. (1962, p. 386).

levitation design concept which utilized an air space to allow the detonation shock wave generated by the high explosives assembly to gather momentum before imploding the core, resulting in higher yields and increased efficiency int eh use of fissionable material. These developments led to the November 1952 test of the Mark 18 Super Alloy (U-235) bomb which yielded 500 kilotons[73]*".*

But there was much more than new nuclear weapons. Indeed Hoddeson & al (1993, p. 416) demonstrated that in fact "*technological contributions cover the full range of science and technology, from chemistry, physics and the science of explosives to the revolutions in electronics and microelectronics. For example, the basic property of plutonium metal were outlined, the correct formula for uranium hybride was identified, fundamental properties of many explosives were discovered,*[etc.]. *New phenomena were uncovered, such as low-energy resonance in U235. New problems were identified, for example, the need to understand more about the fission process, especially at higher energies. Transfer of information from the MIT Radiation Laboratory enabled Los Alamos to refine the development of amplifiers, scaling circuits, and multidiscriminators. Although the Rad Lab deserves credit for many electronics advances, such as decreasing the response time in electronics from milliseconds to microseconds, Los Alamos, helped turn the new electronics technology into a science.*

To help transmit this science to a wider community, Los Alamos wartime researchers M Sands and W. Elmore wrote Electronics: Experimental Technics, *which became a landmark text, not only for experimental physicists but also for chemists, biologists and medical professionals. The new electronics extended the range of research. For example,* (…) *accelerator advances, such as improved time-of-flight equipment and monoenergetic neutron sources, improved the experimental capability of accelerators. Immediately after completion of the atomic bomb work, E. McMillan achieved a milestone in accelerator history with his invention of the principle of phase stability, a development without which more powerful postwar larger circular accelerators could not have been built.*

The potential of the computer for solving highly complex problems (e.g. those of hydrodynamics [of implosion]) *was greatly expanded by the Theoretical Division group responsible for the IBMs; and several Los Alamos theorists, most prominently N. Metropolis, figured in the development of postwar computers. Some of the materials*

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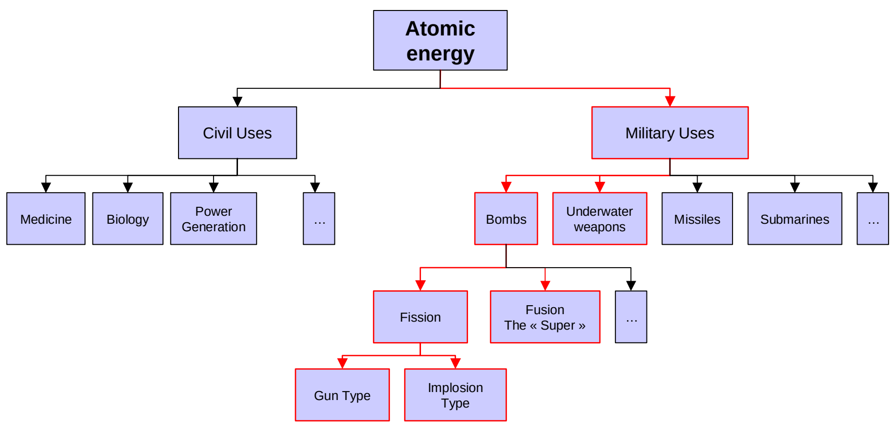
[73] Note that the use of numbers (Mark X) illustrate in itself the lineage concept, each new generation building on the knowledge of its predecessors.

made available at Los Alamos and other parts of the MED advanced postwar in unexpected ways. (…) For example, the development of pure isotopes at Los Alamos made possible the crucial discovery in the 1950s by E. Maxwell and B. Serin of the "isotope effect" in superconductors. This discovery set J. Barden on the path to the development of microscopic theory of superconductivity (…).

Each of these important impacts on postwar research tells its own story about the degree to which technical work at Los Alamos during World War II helped shape the course of modern science" (p. 416-417).

The Manhattan Projects opens the uses of atomic energy by focusing logically on military applications. A simple design tree (Marples, 1961; Hatchuel & Weil, 2002) is useful to show, very partially, the possible uses of atomic energy and the trajectory of the Manhattan Project (figure 12 below).

Figure 12. The trajectory of the Manhattan Project (red lines) [74]



What is important to us is that the Manhattan Project leaders, specifically R. Oppenheimer at Los Alamos, have deliberately decided to pursue research on long term questions with no direct applications for the current goals. As Hewlett & Anderson noted, "*under the circumstances, it was remarkable that they were able to spend any*

[74] It could be very interesting to use the C/K theory framework (Hatchuel & Weil, 2003) to display the corresponding knowledge developed during the project. For example the decision to design bombs, underwater weapons or missiles depends on the military strategy of the states (see MacKenzie on the missile case). The choice between implosion and gun designs is linked as we have seen to a huge bdoy of

time on projects that look to the future[75]" (1962, p. 627). But, in our view, this is not a by-product but an absolute necessity when a team explores a generative design space. We think that this plurality of time horizons and this duality between short-term goals and long-term research to prepare the next steps lies at the heart of exploration project management (Lenfle, 2008). Therefore instead of focusing narrowly on the most urgent objective (build a pratical military weapon in this case), which remains obviously a first priority, the project manager has to *simultaneously* prepare the next steps. As illustrated by the Manhattan case, this involves works on fundamental research or second-order solutions (e.g. underwater weapons, the "super", and so on). This increases the probability of long-term success through the building of new capabilities upon which lineages of products will be build (see Lenfle, 2008).

8. Conclusion

The Manhattan Project constitutes a fascinating case for anyone interested in project and innovation management. The analysis of the managerial strategy adopted by its leaders demonstrate the power of projects to reach extraordinarily difficult goals. However the management of the Manhattan Project is very different from the dominant model of project management represented, for example, by the PMI. Indeed, given the unforeseeable uncertainties it faced, the project cannot rely on an instructionist or rational strategy. Instead they combined parallel strategy and experimentation to explore the field, learn and reach their objectives. They therefore foreshadow contemporary thinking on the management of innovative projects.

There was of course some important specificity that limit the generality of the case. If we leave aside the moral issues and the impact of the atomic bomb on International Relations, the most important relates to resources and impetus. Indeed, given the wartime context and the fear that the Nazis get the bomb first, speed was of the essence. The Manhattan Project thus benefited for the full support of Franklin D. Roosevelt and its administration. This gave us access to almost unlimited resources. Furthermore, for the same reasons the project benefited for the mobilisation of the entire US industry (Du

knowledge on nuclear physics, metallurgy, chemistry, electronics and so on. We leave this to future works.

Pont, Union Carbide, General Electric, Chrysler, Westinghouse, Tennessee Eastman and many others participate in the project) and science (E. Fermi, J. Franck, E. Lawrence, A. Compton, J. Chadwick, N. Bohr, E. Wigner, H. Urey were all Nobel Prize winners). It was thus possible to implement a massive parallel strategy without worrying on spendings[76]. If the project needed a new plant, they build it as quick as possible no matter if it finally proved to be useless. Furthermore, given the wartime context, they benefited from a completely dedicated and motivated workforce at all levels. Overtime was the rule on the project with people working 10 to 20 hours a day, seven days a week.

If this situation is quite rare in real business situation (what Shenhar & Dvir called "blitz" projects conducted in situations of urgency), the Manhattan Project nevertheless remains an important case to understand the problems that exploration raised for project management. It thus contribute to the foundations of a model of exploration project management that builds on experimentation, parallel strategies, iterative learning and the constant adjustement of objectives (Lenfle, 2001 & 2008; Loch & al, 2006; Shenhar & Dvir, 2007). A model that also emphasize the needs to enlarge the scope of project to encompass the building of lineages of products and the development of the firm dynamic capabilities. In this perspective we can wonder if new approaches like numerical simulation and fast-prototyping (Thomke, 2003, Loch & al, 2006) allows low-costs experimentation and thus the widespread use of parallel strategies and experimentation.

Finally, theoretically, this case shows that the fundamental tension between exploitation and exploration, first analysed by J. March, applies to project management. We can therefore distinguish between two different views of projects that are complementary since new ideas are supposed, at least theoretically, to move smoothly from exploration to exploitation/development.

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[75] Thermonuclear weapons (the "Super") are is the most famous examples of this preparation of the future.

[76] « Judge William P. Lipkin, then a finance officer with the rank of captain, recently told me that he remembers vividly what happened when he once questioned a rather sizable MED [i.e. Manhattan Project] voucher that passed over his desk for payment. His superiori told him firmly, "You will forget that you know anything about it. Just forget that you spoke to me about it. Just pay the MED bills and discuss the matter with no one"." (In Groves, 1962, p?). This allow the MED for example, to borrow silver from the US treasury stocks to build the magnet needed to complete the Y12 plant.

In the exploitation perspective the role of the project is to organize the convergence to a predefined objective within a given set of constraints (time, budget, quality). Projects mainly exploit existing competences. The PMI or instrumental view of the project and the work from Clark & Fujimoto falls within this approach.

In the second perspective, projects are a way of organizing the exploration of emerging innovation fields. But entering exploration entails a fundamental shift in project management methodology, with the risk of applying the exploitation framework to exploration. As shown by the present work and other recent research (Lenfle, 2001 & 2008; Loch & al, 2006; Shenhar & Dvir, 2007) in exploration situations it is no more possible to define ex-ante the goal and the means to reach it. Projects thus became highly uncertain and reflexive probe and learn processes. In this perspective projects are first and foremost a way to explore and learn. They became a fundamental component of *search* processes (Adler & Obstfeld, 2007). This should lead us to revisit the fundamental nature of projects which are not only a set of management tools but more generally a way to construct the future and to break with past routines (Adler & Obstfeld, 2007; Boutinet, 1990; Emirbayer & Mische). We hope that this historical detour may help to build this alternative model of project management.

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