Using design theory to characterize various forms of breakthrough R&D projects and their management: revisiting Manhattan & Polaris.

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USING DESIGN THEORY TO CHARACTERIZE VARIOUS FORMS OF BREAKTHROUGH R&D PROJECTS AND THEIR MANAGEMENT: REVISITING MANHATTAN & POLARIS.

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ABSTRACT

In this paper we propose to revisit two emblematic projects, Manhattan and Polaris, with the models developed by design theory. In particular we demonstrate, relying on recent advances in design theory, how these major projects, traditionally presented as radical innovations, are in fact quite different. We show that this explains the different managerial strategies of this two cases : whereas Polaris focuses on the control of the design process, Manhattan exhibit a very original strategy, characterized by the simultaneous exploration of different solutions, to manage unforeseeable uncertainties. We therefore hope to demonstrate the fruitfulness of the dialogue between design theory and project management.

Keywords: Design theory, project management, innovation

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1. Introduction

One of the most important evolutions in the field of project management in recent years is, in our view, the development of a form of "contingency theory" of project management. Indeed, the field of PM has long been dominated by a rational, instrumental approach which aims to reach a clearly defined goal within budget, time and quality constraints. This perspective is now widely criticized. In line with work on project classification (Wheelwright & Clark, 1992; Shenhar & Dvir, 2007) we believe that a distinction should be drawn between the various design situations to which different types of projects will be suited. However this raise an important problem namely to characterize the "nature of the problem" faced by a given project. This is where design theory could be extremely useful. Indeed, until now, the few research on this question rely on relatively general criteria:

- Wheelwright & Clark, in their famous 1992 paper, proposes to class project according to two criteria: the degree of product and process change. This leads them to distinguish between derivative, platform, breakthrough and research & advanced development projects. More recently,
- Shenhar & dvir (2007) have proposed the "diamond approach", in which for criteria are used to class projects : novelty, technology complexity and pace (NTCP). They thus show that traditional project management is suited to relatively simple project that develop incremental innovation.

However, in each case, notion such as "product change", "novelty", or "technology" can be criticized has being too vast. For example, the classification of technology from "low-tech" to "super high-tech" proposed by Shenhar & Dvir is quite hard to operationalize. Indeed "super high-tech" is defined as a case where "*projects are based on new technologies that do not* *exist at project initiation*” (p. 48). But this is a rather broad definition. It does not allow to really grasp the nature of the design situation confronted by a project. Furthermore it is a-theoretical, not grounded in any theory of the design process. This is where PM meets design theory.

In this paper we propose to use recent advances in design theory to discuss the relevance of the dominant model of project management and its limitations in situation of innovative design. Most recent design theories like C-K design theory (Hatchuel & Weil, 2009) are today powerful tools to follow complex design processes, from science products requiring intensive knowledge creation to creative design processes (see for creativity methods and C-K Reich & al., 2010; see for science-based products for instance (Gillier & al., 2010); see examples in Le Masson & al., 2010). Relying on the C-K theory of design we will revisit two landmark cases in the history of project management : the Polaris project (1956 – 1960) and Manhattan project (1942 – 1945). The first one is famous in the field of PM for its development of the PERT method of planning. Thus it exemplifies a case of “traditional” project management. The second one constitutes a perfect example of radical innovation, namely the atomic bomb. Although presented has the roots of modern project management (see for example Morris, 1997), this case has recently been reexamined by Lenfle (2008 & 2011) and Lenfle & Loch (2010). They demonstrate that this presentation is notoriously wrong. On the contrary, Manhattan exhibits very original managerial strategies that can be applied to exploratory projects that are more and more important in today’s innovation-based competition. The discrepancy between the two cases are interesting to study because, usually, they are both presented has radical innovations and there are difficulties to characterize the differences between them. In this paper we intend to demonstrate, using design theory, that they were in fact confronted to quite different situations. And this explains why the managerial strategies have been so different. In so doing we hope to demonstrate that bridging project management and design theory constitutes a fruitful research field for the coming years, in particular for new product development projects.

Fortunately, for our attempt the Manhattan and Polaris Projects have been extensively studied. We may therefore draw on a large amount of historical material that has so far not been used to study innovation management. Our objective is not to provide a comprehensive account of the cases or to summarize their unfolding (for Manhattan see Hewlett & Anderson, 1962 or Rhodes, 1986; for Polaris see Sapolsky, 1971 and Spinardi, 1994), but to focus on the design situation they confront. We will nonetheless include details that are critical for our argument. Given the information available, we consider that the point of "theoretical saturation," which Glaser and Strauss (1967) proposed as criterion to stop collecting data, has been attained. Our analysis may therefore lack empirical originality, but will hopefully triangulate the data in original ways.

2. C-K Design theory

2.1. Main features of C-K design theory for the analysis of projects

Since its inception, design theory has attempted to develop models of the designers' reasoning, as well as tools to organize and/or rationalize the design process (Simon, 1969; Pahl & Beitz, 1996; Suh, 1990). Marples' 1961 seminal paper includes a design tree of engineering design decisions, which helps understand the different options studied by designers working on nuclear reactor design. The same approach has been subsequently used by Clark to analyze the implications of innovation (Clark, 1985). Still these representations were based on a decision making process: the tree-shape described a search process in a complex decision space. In the last decades, it has been demonstrated that decision making models can not account for design processes, because the latter are not search processes because a design process precisely tend to regenerate the space of constraints and the space of design capacities (Dorst, 2006;, Hatchuel, 2002). In the last decades several design theories have been proposed, with the aim to account for more and more generative processes. General Design Theory (Reich, 1995; Yoshikawa, 1981), Axiomatic Design (Suh, 1990), Coupled Design process (Braha & Reich, 2003), Infused Design (Shai & Reich, 2004) or C-K Design Theory (Hatchuel & Weil, 2009) are formal theories that go beyond decision making theory and account for the processes that help to create new objects from known ones by expanding the initial space into a newer, broader one (for a comparison see Hatchuel & al., 2011).

In this paper we choose to use C-K design theory, because the theory has already been successfully used in the study of innovation processes (see for instance Elmquist & Segrestin, 2007 ; Elmquist & Le Masson, 2009; see also Lenfle, 2012) where we have shown that the C-K theory provides a very useful framework to manage exploration projects and evaluate their outcomes.). Moreover it covers a large scope of design processes, from science based products to creative industrial design (Le Masson & al, 2011).

C-K design theory considers that a design process begins with a set of propositions that are considered as true (they are in the K-space, that contains all the propositions that are considered as true) and with one proposition that is neither true nor false (this is called, technically, a disjunction). This is one of the main advantages of C-K theory: it clarifies what is a starting point of a design process: it is a proposition that is not true yet – and it is impossible to prove from the beginning that it is impossible (eg non-marketable or unfeasible). For instance, in 1943, the proposition “there is an atomic bomb” is a concept: nobody can show an atomic bomb but nobody can prove that it is impossible to build an atomic bomb. A proposition that is neither true nor false can not be in the K-space (by definition of the K-space, see above): a proposition that is neither true nor false is called a Conception in C-K theory and is written in the C-space. The design process consists in using proposition known in K to refine and “expand” the proposition in C and to use the proposition in C to create new true proposition in K. In C-K theory design is a dual expansion process: it creates new concepts and new knowledge. The process goes on until the proposition in C is so refined, and the propositions in K are so enriched that finally a proposition in C becomes true: it is no more a concept, it becomes knowledge (technically this is called a disjunction).

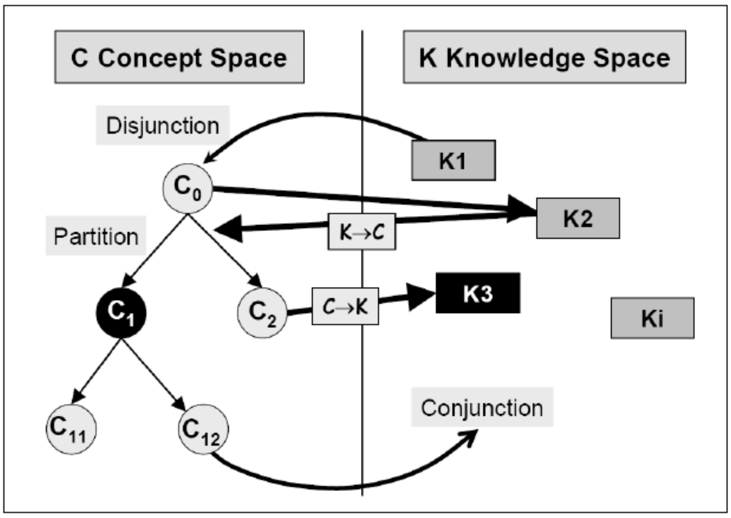
Let’s underline two critical properties of C-K design theory for the analysis of radical innovation process:

1. it helps to track the *evolution of concepts*, i.e. the reformulations, refinements and changes in the product concept all along the design process. C-K theory shows that, paradoxically, there is a strong order in the C-space ; we say paradoxically because the C-space appears as the space of creativity, imagination,chimeras,… a space that is often considered as irrational and chaotic; C-K theory confirms that in C the “truth logic” can not be applied (all propositions are neither true nor false) but there is still a logic that describes the rigorous refinements of an initial concept when new attributes are progressively added to it. Hence it helps to follow complex reasoning on objects that are still partially unknown.

2. It helps to follow the expansion in K: during the design process, new knowledge can be produced (following a research program for instance). This new knowledge accumulates in K

The generic structure of design reasoning is presented in Figure 1.

Figure 1.The generic pattern of design reasoning in the C-K design theory (Hatchuel & Weil, 2009).



2.2. C-K Design Theory and types of design reasoning

It can be proven that C-K design theory accounts for creative design reasoning (see for instance (Hatchuel & al., 2011). More recently (Hatchuel & al., 2013; Le Masson & al., 2013) it has been shown that the structure of the knowledge base has a very deep impact on the type of concepts and creative reasoning. Based on mathematical models, it can be shown that two contrasted situations are possible:

- if the knowledge base is modular or deterministic, then design will finally resume in a form of combinatorial process. The new concept can be designed by the pure combination of known components (actually it was not a concept). A knowledge base is said to be “modular” when a knowledge attribute can be added to a concept without any impact on the following design steps (at a certain step, A or A’ can be added to a concept C and then B can equally be added to C-A and C-A’). A knowledge base is deterministic when the addition of one attribute implies the addition of another one (A implies B).

- By contrast, if the knowledge base is non-modular and non-deterministic, then the design process will lead to the creation of an element that can not be “deducted” from the knowledge base, that is more than a combination of known elements. A knowledge base is non-modular and non-deterministic if, at any stage of the design process there are always two possibilities for additional attributes (C→ C-A *or* C→ C-A’) and it is impossible to add two attributes without any consequences (there will never be any B such that B can be added equally to C-A and C-A’). In this case it is said that the knowledge base follows the *splitting condition*.

These mathematical conditions actually correspond to two specific design situations; in engineering design situations, R&D department and design organization tend to favor situations that are non-splitting (modular architecture and deterministic laws); whereas it has been shown (Le Masson & al., 2013) that industrial designers are precisely educated in creating their own knowledge base that follows the splitting condition. It can be easily understood: engineering department favor stable situations where each object will appear as a combination of known pieces of knowledge whereas designers aims at creating novel, original, “new-to-the-world” objects.

Hence design theory provides us with a powerful theorem: if a knowledge base follows the splitting condition, then it will lead to design “new-to-the-world” objects. We will use this theorem to characterize our innovation projects.

3. The origins of the rational model of project management: the Polaris project.

3.1. Context

The Polaris project emerged in the US during the Eisenhower administration (1953 – 1961, a period during which 1) the launch of Sputnik (octobre 1957) lead to a fear of a “missile gap” with the USSR and 2) the miniaturization of thermonuclear weapons and the development of ballistic missiles converge to open the possibility of designing new types of weapons. This leads to the launch of huge projects to develop the first thermonuclear intercontinental ballistic missiles (ICBM) first by the US Air Force (Atlas / Titan project, 1954 – 1959) and then by the Navy (Polaris project, 1956 – 1960). Before developing the Polaris case it is useful to explains that the development of ICBM raises huge design problems that were both

- ***Technical*** : concerning components (liquid vs. solid propellants, guidance systems, warhead, reentry vehicle…) and « system integration » (vibrations, electromagnetic interferences, thermal control, interfaces management)
- ***Organizational*** : one need to coordinate and integrate the functionnally defined branches or bureaus, dozens of firms involved, conflict between armed services. Moreover, « *as a new technology, ballistic missile did not fit easily into the existing weapons acquisition structures* » (i.e. not a bomber, not a bomb, not a guided-missile (first called the *pilotless airplane)*…; Sapolsky, 2003.) and it hurts USAF strategy and « culture » i.e. reliance on manned strategic bombers (Beard, 1976)

To overcome this difficulty both the USAF and the Navy created dedicated organization with very strong political support and separate funding : the Western Development Division of the USAF and the Special Projects Office of the Navy.

3.2. Designing Polaris

The Polaris project was launched in 1956 by the Navy to develop the first submarine-launched ballistic missiles (SLBM) carrying thermonuclear warheads. These offensive weapons, almost impossible to track and attack, became a key element of nuclear deterrence. In spite of its reputation for introducing PERT, the Polaris project in reality was much more about strategic choices than about project management techniques. The U.S. Navy initiated the project in order to secure resources from the Pentagon, given that the newly created Air Force was appropriating most of the vast resources available for nuclear and strategic defense. A key purpose of the program was to "*get a share of the ballistic missile 'pie'*" (Spinardi, 1994, p. 25): Admiral Burke believed that "*The first service that demonstrates a capability for this is very likely to continue the project and others may very well drop out.*" (ibid, p. 26). The result was a clear prioritization of schedule over cost and specifications, and, in addition, a willingness to experiment and change the specifications over the course of the project. This is illustrated by the fact that the first two versions deployed (in July 1960 and late 1961) of the Polaris missile had only about half the originally desired range (of 1,500 miles) and explosive capacity (of one megaton).

What is interesting for our purpose is that, to gain funds, the specifications of Polaris were carefully differentiated from the competing Air Force systems, emphasizing the destruction of urban centers with limited accuracy required—as opposed to the Air Force's goals of destroying military targets, which required less power but more accuracy (ibid, p. 34).

The technical challenge was huge since nobody has ever designed a ballistic missile for submarine launch. In order to understand the challenge we have to dig a bit into the technical aspects of missile design. The first important point is that, at the time of Polaris, the architecture of a ballistic missile is largely given. It is composed (from top to bottom) by 1) a reentry vehicle carrying the warhead, 2) a guidance system, 3) propulsion and flight controls. Therefore in Polaris Polaris design

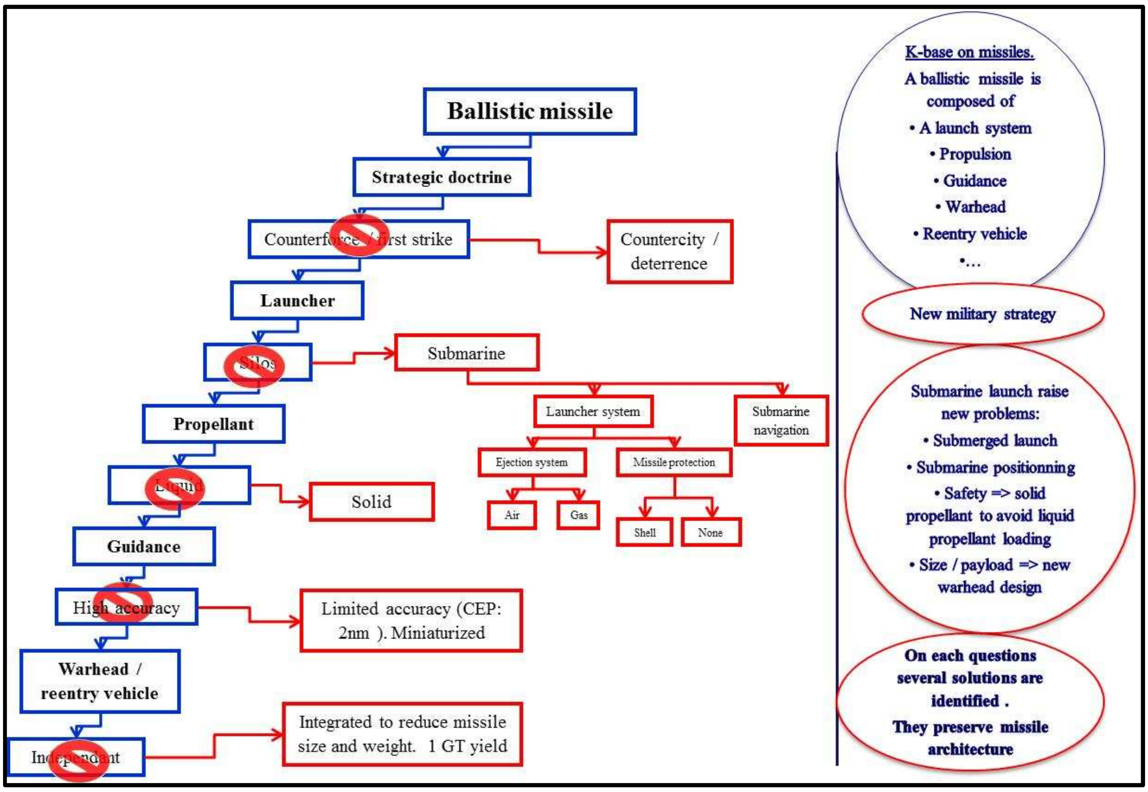
1. Innovation concern mainly subsystems and, first and foremost the W47 thermonuclear warhead which is probably the only radical innovation in Polaris[1].
2. The huge difficulty is the complexity of system integration in the missile itself (given the size constraints imposed by submarines), between the missile and the submarines, and with the required navigation/communation systems required to ensure an accurate positioning of the missile[2].

However the available knowledge base with solid enough to allow the project team to identify ex-ante different technical solutions. Sapolsky is very clear on this question when he explains (1972, p. 136-137) that " *if breakthrough means a substantial and unanticipated advance in state-of-the-art, there were, is is true, no technological breakthrough (…) [in] FBM subsystems. In every subsystems, the trend of technology could be identified at the initiation of the program and remained essentially unchanged during its duration. In every subsystem, progress came through a multitude of small steps and not through dramatic leaps. (…)"* and he confirms that " *(…) The technical challenge and breakthrough in the FBM program was the early development of the system itself. (…) To build a system that involved interdependant progress in a dozen of technologies was, however, unprecedented. Such a*

[1] In order to reduce the size and the weight of the warhead, engineers and scientists to integrate the reentry vehicle and the warhead which becomes a single unit. This require close cooperation between the Laboratory and the Navy, establishing a new way of doing business for both
[2] One has to remember that the first satellite-based localization system, Transit, has been designed for Polaris.

system represents a susbtantial and historically unanticipated advance in the arts of planning and program management ». Therefore, if we apply the C/K framework to the Polaris case, we obtain the following depiction which emphasizes that Polaris's design strategy was to differentiate it from USAF ICBM

Figure 2 : Polaris design as differentiation from USAF ICBM



This (simplified) representation of Polaris's design strategy demonstrates that

1. Polaris builds on previous projects
2. The conceptual evolution is important (from silo-based ICBM targeted at military forces to submarine-launched deterrence weapon targeted at cities)
3. But, since Polaris builds on previous projects, the knowledge base is very rich at the beginning : the architecture is given, for each components several solutions are

identified and competences are available within the Navy, contractors like Lockheed and universities.

Therefore the residual uncertainties are not that big. Let's underline the two critical reasons: on the one hand there is a large reuse of existing components and solutions; and the other hand the project benefits from strong independences, since some components will be changed without strong impact on the others. For instance the main uncertainties concern the warhead design, which is largely independent from the rest of the missile, the underwater launch system and the solid propellant propulsion..

Let's come now to the characterization of the knowledge base of the Polaris project: it appears that the available competences finally build a knowledge base that, with respect to the initial concept, was actually modular and deterministic. It does not follow the splitting condition. Using the theorem mentioned in part 2.2, we can conclude that Polaris project was actually a combinatorial project. Of course it tested a combination that had never been tried before but the combination was finally (quasi-) predictable with the available knowledge.

3.3. Managerial implications

This representation of the design problem helps to understand the management of Polaris. Indeed, since the design process is foreseeable (notwithstanding the inevitable surprises), the main challenge is to control its unfolding given cost / time / quality constraints. We can now understand the end of Sapolsky's sentence : "*Such a system represents a susbtantial and historically unanticipated advance in the arts of planning and program management*" (see above). Indeed the challenge is to control the design of an incredibly complex system. This leads the Polaris to rely on two managerial innovations.

The first and, unquestionably, the most important is the creation of a dedicated organization, the Special Projects Office (SPO; see Sapolsky, 1972). This allows the project to overcome the usual bureaucratic war between the different departments of the Navy.

Furthermore the organization of the SPO mirrored the architecture of the missile. It was organized by subsystems (SP 22 : launcher / SP 23 : guidance & fire control / SP 24 : Navigation / etc.) and it combines

1. A very tight centralization of system integration : the SPO define the goals, architectures, interfaces and control the budget ;
2. High delegation of the work on subsystems. Contractors were give a very high degree of autonomy within the guidelines set by the SPO. There were always several contractors in competition in the design process to maintain pressure and ensure the existence of back-up solution (see table Sapol1 in chapter 4 of Sapolsky).

According to Sapolsky, the existence of the SPO and its managerial approach is the key success factor of the Polaris project.

The second managerial innovation, the most famous if not the most efficient, is the PERT approach to scheduling. In popular account of the project, the success of Polaris, is associated with the development of the PERT method which, after the project, will become almost synonymous with project management. Sapolsky has demonstrated that this was a myth (chapter 5) and our purpose here is not to discuss this question. We are interested in uncovering what the principles of PERT reveals from the management of Polaris. To do this we refer to the paper published in 1959 by Malcolm & al (who were working for the SPO), which is the first appearance of PERT in the literature. And their starting point is very interesting. As they explain: « *A schedule for the system development was at hand, encompassing thousands of activities years into the future* ». In other words, in 1959 (3 years into the project) most of the design work was done and the challenge was to monitor work progression in a context of very tight schedule. Therefore, they explain "*The PERT team felt that the most important requirement for project evaluation at SPO was the provision of* *detailed, well-considered estimates of the time constraints on future activities*." The hypothesis they make are revealing of the huge K-base of the project :

- An ordered sequence of events to be achieved constitute a valid model of the program
- Activities could be determined
- Activities are conditionned by identifiable product performance requirements and ressource applications.
- Ressources are known and technical performance expected is specified

Consequently, "*an approach dealing only with the time variable was selected*". And, in fact, to the extent that system and components were already specified, the main uncertainty was task duration. The problem is thus one of decision under uncertainty, a question that could be handled through operation research methods that were in favor during the sixties through institutions like the RAND corporation (see Marschak & al., 1967 or, for an historical approach, Hounshell, 2000; Erickson & al., 2013). But we now know the necessary conditions to rely on this method: the existing K-base and its structure allows an (almost) complete definition of the system from the start[3].

4. The Manhattan case and the management of innovative design situation

We can now turn to the Manhattan case which another landmark project in the field of project management. Recent research demonstrated that that statements saying tha Manhattan is the roots of modern project management are false (Lenfle, 2008; Lenfle & Loch, 2010). On the contrary Manhattan exemplifies the case of a project confronted with radical innovation and the associated unforeseeable uncertainties (or unknown unknowns). The interesting question is thus to analyze how they succeed in designing such an innovation so quickly. Here again

[3] This is obvious in a Navy study of 1956-57 which gives almost the final characteristics of Polaris (see The China Laker, vol. 9, n°4, fall 2003)

we will not describe the unfolding of the project (see Gosling, 1999 or Lenfle, 2008 for an overview). Instead we will focus on the design strategy of the project.

4.1. Designing the Bomb

Scientifically, the Manhattan Project was based on the principle of the self-sustained nuclear chain-reaction which will be demonstrated by Enrico Fermi in December 1942, 3 months after the beginning of the project. However going from a crude prototype pile at the University of Chicago to a working nuclear weapon will be a harsh innovation journey. More precisely the project faced two major problems: the production of fissionable materials, and the design of the bomb itself. These problems were aggravated by time pressure. Indeed, the US government feared that Nazi Germany would build the bomb first; therefore, by November 1942 already, it had been decided to skip the pilot phase and move directly from research to full-scale production.

4.1.1. The problem : production of fissionable materials and bomb design

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

- Separating U235 from U238 involves extremely complex processes, based on the slight differences in the atomic mass of the two isotopes (less than 1%). Seven different separation methods were identified in 1941; as we shall see, three of them would finally be used [14].
- Plutonium production involves the design and construction of nuclear reactors and the associated chemical separation plants. Twelve separation processes were

studied at the University of Chicago “Met Lab” at the beginning of plant construction.

These were breakthrough innovations. The processes either did not exist before the project (plutonium production) or had never been used with radioactive materials (chemical separation). They entailed extremely tight requirements, and involved radioactive (and therefore very dangerous) materials. Above all, the available knowledge about the production, metallurgy and chemistry of plutonium and uranium separation was far from complete. Thus, commenting on the 1943 Met Lab plutonium research program, Smyth observed that “*Many of the topics listed are not specific research problems such as might be solved by a small team of scientists working for a few months but are whole fields of investigation that might be studied with profit for years.* [So] *it was necessary to pick the specific problems that were likely to give the most immediately useful results but at the same time it was desirable to try to uncover general principles*” [14]. In modern terms, they were confronted to a (highly) generative design space. The more they progress, the more they are likely to face new problems and solutions.

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

4.1.2. Managerial implications

Such a situation had fundamental managerial implications. The most important one was that the entire project was first and foremost characterized by unforeseeable uncertainties. General Leslie Groves,the project director, quickly realized the implications of such a situation. First, he recognized the impossibility of establishing a reliable plan of the project. A "*tentative construction program*" had emerged out of the Berkeley seminar. But "*[i]t soon became apparent that these target dates were wholly unrealistic, for basic research had not yet progressed to the point where work on even the most general design criteria could be started*" (ibid, p. 15).

In short, the required knowledge was largely non-existent at the outset of the project. At the end of a meeting with scientists at the University of Chicago on October 5, 1942, soon after his nomination as Project director, Groves "*asked the question that is always of uppermost in the mind of an engineer: with respect to the amount of fissionable material needed for each bomb, how accurate did they think their estimate was? I expected a reply of "within twenty-five or fifty percent," and would not have been surprised at an even greater percentage, but I was horrified when they quite blandly replied that they thought it was correct within a factor of ten. This meant, for example, that if they estimated that we would need on hundred pounds of plutonium for a bomb, the correct amount could be anywhere from ten to one thousand pounds. Most important of all, it completely destroyed any thought of reasonable planning for the production plants of fissionable materials. My position could well be compared with that of a caterer who is told he must be prepared to serve anywhere between ten and a thousand guests. But after extensive discussion of this point, I concluded that it simply was not possible then to arrive at a more precise answer*" (ibid, p. 40). He thus concluded: "*While I had known that we were proceeding in the dark, this conversation brought it home to me with the impact of a pile driver. There was simply no ready solution to* *the problem we faced, except to hope that the factor of error would prove to be not quite so fantastic*" (ibid.).

It is thus clear that the project faces a design situation that is completely different from Polaris. The K-base is largely non-existent, there is no existing industrial base and, therefore, nobody can predict the unfolding of the project. One can even question its manageability. This is where the design strategy plays a central role since the question is not to control a complex but predictable design process (Polaris), but to manage the unknown.

4.1.3. Design strategy

This is where the design strategy plays a central role. We can roughly summarize as follows : given the available K-base, nobody knows what is feasible in terms of fissionable material [mt] and ignition mechanism [im]. Several solutions are identified (see Serber, 1943) but it's impossible to anticipate which one will work. Moreover there are probably incompatibilities in K-space, i.e. not all the [mt;im] combination will work. Contrary to the previous case, there are strong interdependencies, the choice of one alternative leads to redesign the rest of the project. We recognize here the two features of the splitting condition: with regard to the initial concept, the knowledge base is non deterministic and non modular. Here again, using the splitting condition theorem, we can deduce that this implies that the concept will lead "out-of-the-box", beyond the pure combination of available components[4].

In a such a situation, it is necessary to go out of the box while meeting all the "constraints" or requirements of the initial concepts. It implies a strong effort of knowledge creation on each of the constraints. Since no modularity can be expected, it is necessary to explore a large set of alternatives. It hence enlightens the the design approach of L. Groves and the steering

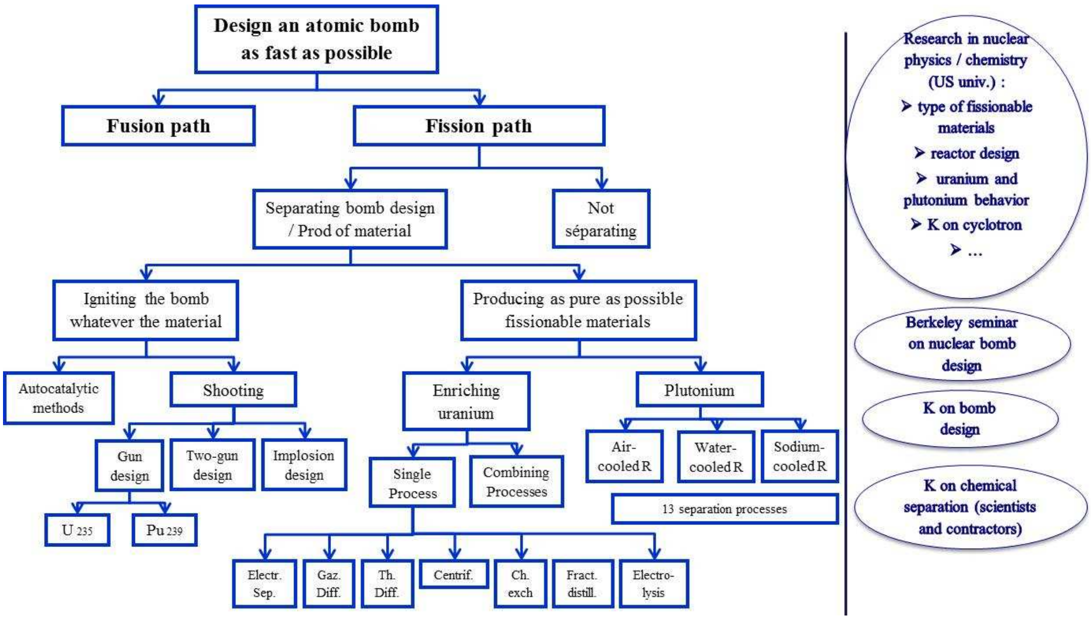
[4] Note that some design theories are very close to an extended combinatorics, like General Design Theory (GDT) or Axiomatic Design (AD). These theories are actually sufficiant to describe projects that don't meet the splitting condition. Polaris project might have been described using GDT or AD. As soon as a project knowledge base does meet the splitting condition, itwill be necessary to rely on design theories that are more generative, like Coupled Design Process (CDP), Infused Design (ID) or C-K. This confirms that we were right to choose C-K theory to compare and characterize Polaris and Manhattan.

committee. Indeed, as shown in the figure below, they will make two fundamental design decisions :

1. The separation of material production and bomb design. The idea was on one hand to explore different ignition mechanism working "*in one or more of the materials known to show nuclear fission*" (Serber, 1943, p. 1) and, on the other hand, to produce as pure as possible fissionable materials. The goal was to avoid exploration of *predefined* couples of [mt; im] that would prove to be dead ends;
2. Because of unforeseeable uncertainties and the utmost importance of time, they decided to explore and implement simultaneously the different solutions, both for the production of fissionable materials and for bomb design (see Lenfle, 2011 for a detailed analysis of the parallel approach in the Manhattan case).

The fundamental goal of this strategy was to build a large K-base to be able to design different weapons given what will be discovered. The Figure 3 summarizes the possible solutions envisioned by the project team. In the remaining of the paper, we will use it to describe the evolution of the design process of the atomic bomb. This will help to understand how this strategy explains the final success of a project that, otherwise, could have been a complete failure.

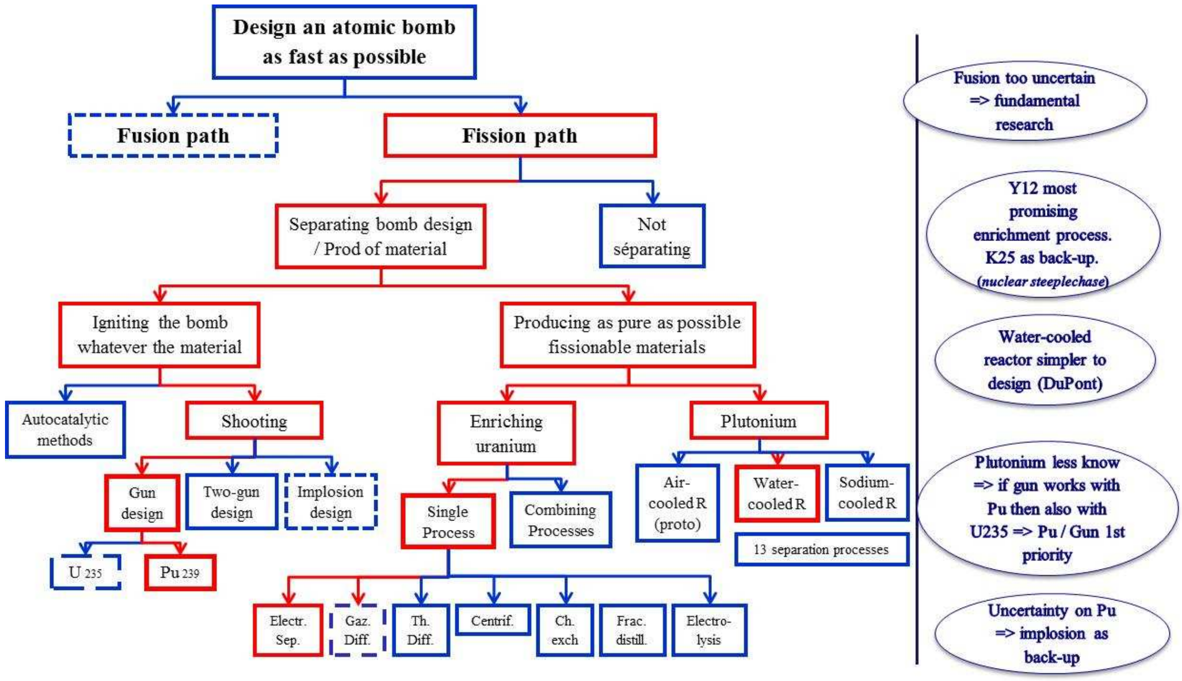
Figure 3 : a complex and generative design space (end of 1942)



Given what the available knowledge in september 1942 their first strategy (figure 4, preferred choices are in red, back-up in blue dotted lines) was

1. To favor fusion over fission which, although clearly envisioned, was too uncertain to be of any utility during this war ;
2. Concerning the production of fissionable material to focus on electromagnetic separation (code named Y12) with gaseous diffusion (K25) as a back-up
3. Concerning bomb design to favor the gun method, which seems more robust, and to use it with plutonium, less known at this time. It was supposed that if the gun design works with plutonium, it will also work with uranium. However, given the unknowns, implosion was studied, by a smaller team, as a back-up.
4. Concerning plutonium production, DuPont chooses a water-cooled reactor, simpler to design.

Figure 4 : First design strategy space (sept. 1942 – spring 1944)



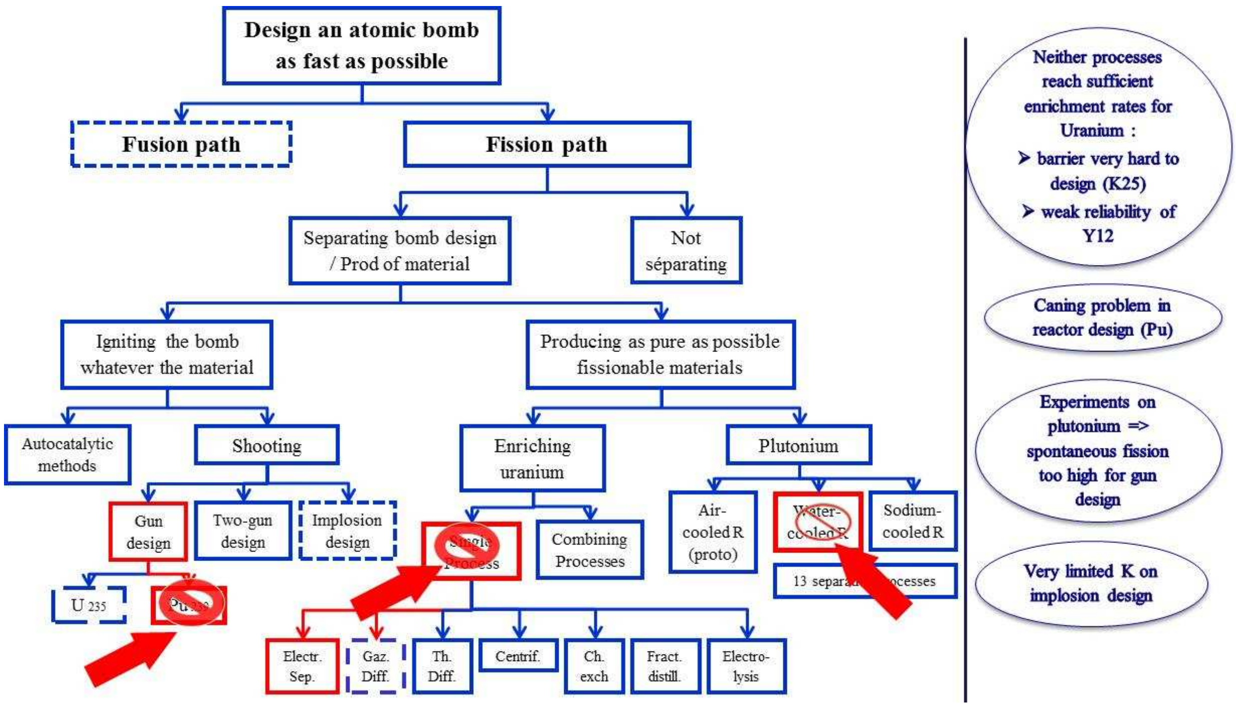
However, the unforeseeable uncertainties soon manifested and, in the spring of 1944, the project leaders, first and foremost, Groves and Oppenheimer, realized that the project had maneuvered itself into a dead end. Indeed

1. None of the uranium enrichment methods succeeded in producing sufficiently enriched uranium: the cyclotrons for electromagnetic separation were a "*maintenance nightmare*" and the gaseous diffusion process raised seemingly unsolvable design problems (see Lenfle, 2011 for a synthesis).

2. The production of plutonium looks more promising but "Canning" the uranium slots to protect them from water also raised huge problems.

3. And, to worsen the picture, the "gun" design proved to be unsuitable for plutonium (this episode, known as the "spontaneous fission crisis", is describe in detail in Hoddeson & al., 1993)

Figure 5 : The spring of 1944 crisis



Therefore at this date they had a fissionable material (plutonium) without a bomb design, and a bomb design (the "gun") without a workable fissionable material (uranium 235). This is where the chosen design strategy revealed its relevance. The building of a large K-base, and the decision to explore different solutions simultaneously allow the team (figure 6)

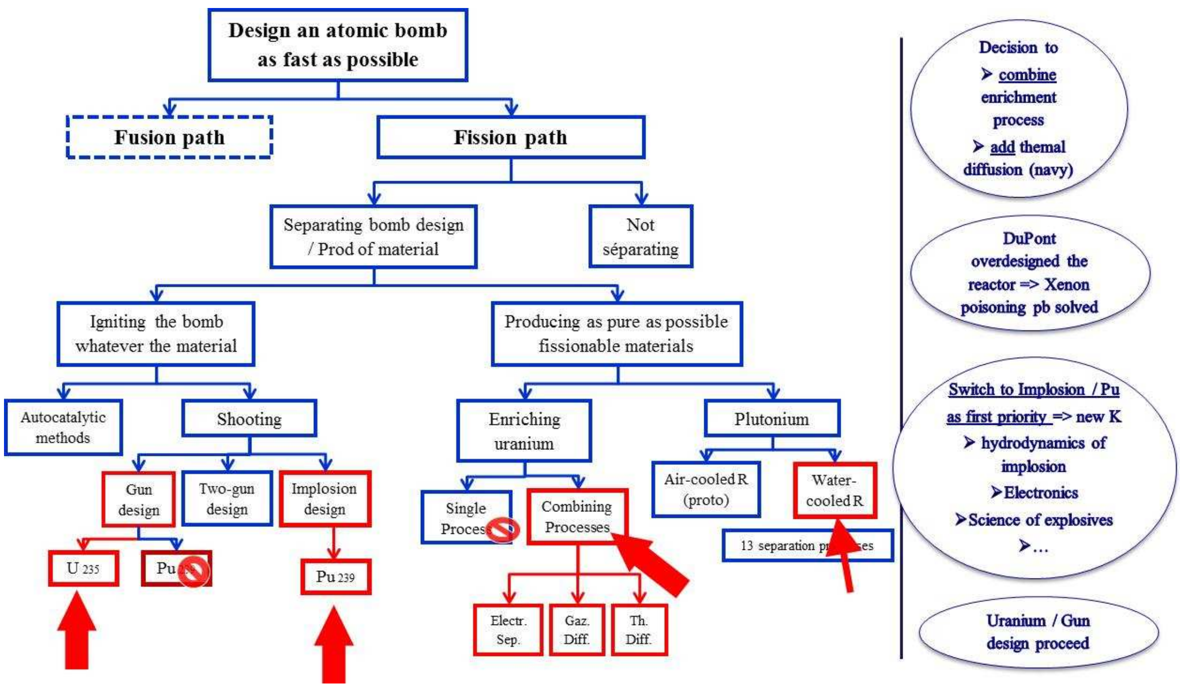
1. To switch from the plutonium gun to the implosion design as first priority (but gun design continued for U235) even if many people doubt that this could be designed ;

2. To add a new separation process for uranium enrichment and to combine the different processes in order to reach the desired level of enrichment. Combination of uranium enrichment processes (see Lenfle, 2011 on this decision) ;

3. To adapt a strategy of intense experimentation on the “canning” problem in plutonium production

In terms of design theory, there occurs here a fascinating phenomena: the initial knowledge base met the splitting conditions; now it has been so enriched during the exploration process, that step by step it becomes non-splitting: modules and deterministic rules have been created. And at this stage of the process, it becomes possible to combine pieces and components to get a new “modular” solution. As soon as the knowledge base appears as (most likely) modular it becomes possible to stop exploration and knowledge creation and to come back to a combinatorial process. Hence the surprising speed of the final design phase.

Figure 6 : Escapes (summer 1944 – august 1945)



This flexibility, allowed by the design strategy, explains the final "success" of the project that, at the end, proceeded at incredible speed. The implosion design was frozen very late, probably on February 28, 1945. Oppenheimer then created the "cowpuncher committee" to oversee the final phase (see Hoddeson & al, chap. 15 and 16). Yet the remaining uncertainties around the new device were so great that Groves, finally but reluctantly, and despite the considerable cost it would entail, approved Oppenheimer's request to test the bomb. The Trinity test marked the dawn of the nuclear age. On July 16, 1945, the Manhattan Project tested, in a remote area of the New Mexico desert, the implosion bomb. The test was a success. The "gadget", as it was nicknamed, exploded with an estimated power of 20,000 tons of TNT and the bombing of Hiroshima and Nagasaki followed three weeks later.

5. Discussion and further research

In this paper we have tried to bridge the literature on project management with recent advances in design theory. What can we learn from this first attempt and particularly from the comparison of the two cases.

First it demonstrates the power of design theory to overcome the limitations of traditional typologies of innovation. Indeed, both Polaris and Manhattan are traditionally presented as examples of radical innovations. However our analysis demonstrates that the problem is more complex than that. Of course both were innovations, but we show that Polaris benefit from a large K-base and can rely on an industrial network of contractors already active in the field of missile design. Therefore, as pointed out by Sapolsky "*In every subsystems, the trend of technology could be identified at the initiation of the program and remained essentially unchanged during its duration. In every subsystem, progress came through a multitude of small steps and not through dramatic leaps*." There were risks in Polaris but few unforeseeable uncertainties. The knowledge base was basically structured in a non-splitting way, meaning that it was fundamentally modular and deterministic. On the contrary the Manhattan Project was plagued by unknown unknowns and has no industrial base to rely on. More precisely the analysis with C-K theory reveals that in Manhattan case, the initial knowledge actually correspond to the splitting condition: any new attributes had critical consequence and there was never one single and self-evident alternative. As predicted by the splitting condition theorem, Polaris design strategy was quite straightforward, whereas Manhattan had to adopt a much more original approach to manage the unknown and learn. As Groves said: *"the whole endeavour was founded on possibilities rather than probabilities. Of theory there was a great deal, of proven knowledge, not much"* (1962, p. 19) . In so doing we show how design theory is more precise than the traditional typology of innovations to understand what happens in projects.

This, and this is our second contribution, leads to explicitly link the design situation and strategy to the management of the project. It contributes to the on-going effort to excavate the roots of project management techniques (Lenfle & Loch, 2010; Soderlünd & Lenfle, 2013). More precisely it demonstrates that the "rational" approach to PM, with its emphasis on control, is viable when the team benefits from a K-base and a concept that allows to 1) define the problem and 2) identify the different solutions beforehand. This is largely the case of Polaris. On the contrary in situation of innovative design, when unknown unknowns exist in K-space and/or C-space, then traditional PM techniques become completely irrelevant. This cannot be more clearly stated than by General Groves insistence on the decision "*almost at the very beginning that we have to abandon completely all normal orderly procedures in the development of the production plants* » (Groves, 1962, p. 72). And our analysis of Manhattan with the C/K theory demonstrates the necessity to adopt new managerial approaches based on the construction on a large K-base in order to *design* the necessary flexibility. Moreover we discover one key feature of the success of Manhattan: the team did not only learn but the knowledge created actually led *to build a knowledge base that,* *this time, was non-splitting*. We better understand the very smart strategy of Groves, to explore all the extreme combinations of alternatives, in the hope to create new pieces of knowledge that could be considered as modules or deterministic rules.

No doubt that this dialogue between project management and design theory constitutes an important avenue for future research on the management of exploratory project. Indeed it could help to new strategies of project management that take into account advances in design theory. We think in particular to the notion of expansion (Hatchuel & Weil 2009) and expandable rationality (Hatchuel, 2002) that, in our view, reopen a field that, for too long, has think of projects as convergence processes. This is already in progress. Lenfle (2012), for example, how C-K should lead us to rethink the evaluation of project which produces much more than what they deliver. Design theory helps to formalize the "much more" in terms of C & K. Therefore, more generally we think that design theory offers new way to represent / discuss / manage the exploration process.

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