Exploration, project evaluation and design theory: a rereading of the Manhattan case.

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1. Introduction

There is a widespread agreement in the managerial literature that projects produce much more than they deliver (Shenhar & Dvir, 2007; Iansiti & Clark, 1994; Nonaka & Takeuchi, 1995). However, most of the literature focuses first and foremost on what projects deliver (new products, processes, services). The examination of projects' contributions to a firm's dynamic capabilities or knowledge base is frequently left for post-project review… and rarely done in practice (Brady & Davies, 2004). While unquestionably central, the focus on project deliverables can be misleading , especially in the case of projects with unforeseeable uncertainty (Loch & al., 2006). Project evaluation therefore deserves further research, for it raises important theoretical and managerial questions. The thorough evaluation of a project's contribution to the capability building process could help managers appreciate its strategic importance for firms, and avoid the problems of financial evaluation depicted for example by Baldwin & Clark (1994).

This question is fundamental in a competitive environment that leads firms to rely on projects to explore new fields. Research on innovation management demonstrates that projects with unforeseeable uncertainty rarely survive the resource allocation process in large organizations (Bower, 1970; Dougherty & Hardy, 1996; Christensen, 1997; Burgelman, 2003). Indeed, in this case, neither the goals nor the means to attain them are clearly defined from the outset, since "*little existing knowledge applies and the goal is to gain knowledge about an unfamiliar landscape*" (McGrath, 2001). We will call such projects *exploration* (or *exploratory*) projects (Lenfle, 2008a) – a notion close to that of *vanguard projects* proposed by Brady & Davies (2004). Their evaluation is an important topic in contemporary research on project management both from an operational and a strategic perspective. In the case of exploratory projects, the "rational" view of project management as the accomplishment of a clearly defined goal in a specified period of time, within given budget and quality requirements, has been shown to be oversimplified (Lenfle 2008a; McGrath, 2001; Loch & al, 2006). Contemporary research on project management has argued for a plurality in project management practices, according to the nature of the project (e.g. Shenhar & Dvir, 2007). It has also called for an alternative model which sees project management above all as an experimental learning process

(Loch & al. 2006), as a way to organize and structure search and exploration processes (Lenfle, 2008a&c; Adler & Obstfeld, 2008). This has raised the need to define a framework to evaluate a project's results, its successes and failures. In this article we wish to explore this issue by bridging project management and design literature. We believe research on design processes proposes models that could help managers to better understand how exploration projects function and what is at stake in them.

To deal with this question we will go back to history. Indeed one of the most famous cases of vanguard exploratory project is the Manhattan project that, during the Second World War, resulted in the development of the atomic bomb. This case is worth studying for at least two reasons. First, it led to a major breakthrough in the history of technology. Second, it demonstrates the complexity of project evaluation. The Manhattan case thus constitutes an exemplary case (Yin, 2003) that may provide interesting insights on the definition of new frameworks to evaluate project results.

This article is organized as follows. Section 1 discusses the literature on project evaluation. Section 2 summarizes the history of the Manhattan project, which is in turn analyzed in section 3. Section 4 examines the relevance of the proposed framework and identifies questions for future research.

2. Project evaluation: an analysis of the literature.

The question of project evaluation is central in the literature on project management, and has been treated from different perspectives.

The PM literature: from QCT to multidimensional strategic concept of success

The literature is dominated by the classical quality / cost / time framework, which emphasizes, first, the attainment of a clearly defined goal within specified constraints and, second, the enhancement of a firm's project capability in a PMM perspective (see for example Cleland & Ireland, 2002; Morris & Pinto, 2004; Kerzner, 2005). Here the main question is how to manage a project better rather the development of new competences. Such an approach, well adapted to development projects, hardly applies to exploratory situations in which neither the goal nor the way to reach it can be clearly defined from the beginning (Mc Grath & Mc Millan, 2000 & 2009; Loch & al., 2006; Lenfle, 2001 & 2008; Elmquist & Le Masson, 2009). Shenhar & Dvir (2007) proposed to enlarge the framework and define project success as a "*multidimensional strategic concept*" (2007, p. 24). They defined five groups of measures depending on the time scale under consideration: from QCT measures to evaluate short-term efficiency, to measures aimed at assessing a project's contribution to developing new competences useful in the future. While interesting, this framework is limited by the hypothesis that the constitution of new competencies is a by-product that can only be evaluated after the end of a project. Moreover research on post-project review demonstrates that, although such reviews are important to organizational learning, they are rarely carried out (von Zedtwitz, 2003). In contrast, as we argued in an earlier paper, the constitution of new knowledge or of unused concepts is one of the key results of exploration projects (Lenfle, 2008).

The evolutionary perspective: product and capability building

This is in line with the literature on new product development projects, which emphasizes the dual nature of project performance (Maidique & Zirger, 1985; Wheelwright & Clark, 1992; Nonaka & Takeuchi, 1995). For example, in Teece & Pisano's seminal issue of *Industrial and Corporate Change* on dynamic capabilities, Iansiti & Clark (1994) develop a framework that draws on the evolutionary theory of the firm (Penrose, 1959; Wernerfelt, 1984) and on Clark's work on new product development (Clark & Fujimoto, 1991). They observe that "e*ach development project draws on the knowledge from the existing capability base, processes the knowledge through a sequence of concept development and implementation activities, and produces outputs that are valuable to the organization. Product development activities create two types of output. First, they create new products. Second, they develop new knowledge bases which renew the firm's competence base*" (p. 567)[1]. They further demonstrate how different generations of projects at Nissan and NEC build on knowledge developed by their predecessors, thus greatly improving the firm's performance. In the same vein, Brady & Davies (2000 & 2004), studying the case of CoPS (complex products and services), propose that what they call *base-moving projects* constitute a powerful solution to enter new markets and develop new capabilities. In such an evolutionary perspective, projects are the firm's "*engine of renewal*" (Bowen & al, 1994) and performance measurement systems must integrate the capability-building dimension (see Loch & Kavadias, 2008). This works, however, do not provide operational tools to manage this dimension.

The impact of the capital budgeting process

The idea of integrating the capability-building dimension was not entirely new, but rather part of a larger debate on capital budgeting systems and their impact on firm performance. Indeed, in the 1980's and early 90's, a heated debate on the American economic decline took place within the US academic and managerial community (Hayes & Abernathy, 1980). Kim Clark was an active player in it, bridging new product development and finance research (on this debate and Clark's work, see Lenfle & Baldwin, 2007). Together with C. Baldwin (Baldwin & Clark, 1992 & 1994), he argued that the budgeting procedures of large US corporations are unable to cope with the emergence of a new industrial paradigm. This new paradigm, the authors noted, required investment in new organizational capabilities, such as quality, speed, flexibility, or capacity to cannibalize leading to radical innovations. From a financial perspective, these capabilities were not simple investments, but "platforms" that in turn generated "options." (In finance, an option is "the right but not the obligation to take a particular action.") The benefits derived from them were thus necessarily complex and difficult to quantify. At the same time, opportunities to make such investments generally arose at the lower levels

[1] Maidique & Zirger (1985) also point this out – without, however, providing case studies and a theoretical framework as Iansiti & Clark do.

of the organization—on the factory floor, in engineering departments, and in the new product development groups. In these "non-strategic" areas, financial analysis was generally based on simple discounted cash flow calculations, which were not capable of recognizing option values. Baldwin and Clark thus brought to light a fundamental mismatch between, on the one hand, the nature of investment opportunities in manufacturing and new product development, and on the other, the methods U.S. corporations used to assess their financial worth (on this question see also Christensen, 1997). This mismatch had a *systematic and pernicious effect* on investment decisions: it caused managers to favour short-term profitability over the creation of capabilities and learning capacity. Awareness of such a situation, Baldwin and Clark argued, should lead firms to adopt "*a 'mixed' resource allocation system*" integrating the value of capability building and the need for companies "*to change their initial plans as new knowledge develops*" (Baldwin & Clark, 1992, p. 80-81).

The real-options framework

Baldwin & Clark belonged to an important stream of research that made a fundamental contribution to the question of project evaluation: the real-options perspective. The real-options framework was originally developed to overcome the limitations of financial tools, first of all the Net Present Value (NPV). As explained by Kester (1984), some types of investment defy the NPV logic because, rather than either/or choices, "*they are but the first link in a long chain of subsequent investment decisions. Future events often make it desirable to modify an initial project by expanding it or introducing a new production technology at some later date.*" (p. 155-156). This is true for R&D investment (Hamilton & Mitchell, 1990), as well as for all kinds of *platform investment* (Kogut & Kulatilaka, 1994 & 2001) that reinforce organizational capabilities. The main contribution of the real-option framework is to introduce flexibility in the resources allocation process: instead of having to evaluate ex-ante the costs and pay-off of a project through NPV, the project under consideration is seen as a knowledge acquisition process. At each step, it is evaluated and, given the available knowledge, a decision is taken to defer, abandon, expand, contract or switch (Trigeorgis, 1997). Such an approach helps renew project evaluation and management (e.g. Mc Grath, 2001; Mc Grath & Mc Millan, 2009) by showing that the more uncertain a project, the more important it is to delay commitments and maintain flexibility to change the course of action. However, given that the real-options framework assumes the possibility of evaluating a project's potential (i.e. its capacity to draw on an underlying asset), it might be difficult to use, especially in connection with discontinous innovation (Elmquist & Le Masson, 2009; Hooge, 2010). Huchzermeier & Loch (2001), for example, demonstrate that the value of flexibility depends on the structure of uncertainty resolution. They show that there are cases where flexibility is of no value, such as when the unfolding of projects does not gives enough information to reduce uncertainty or when a project is simply too far behind compared to its competitors.

Although the real-options perspective recognizes the necessity to consider a project as a process, it fails to provide a framework to represent the unfolding of the process. Indeed, most work in the area focuses on methods of evaluation at the go/no-go decision points (see Schwarz & Trigeorgis, 2004 for an overview) rather than on the process itself. However, the crucial element for evaluating exploration projects is to keep a trace of the process, since only that makes it possible to fully grasp their contribution to capability building.

Internal Corporate Venture and capability building

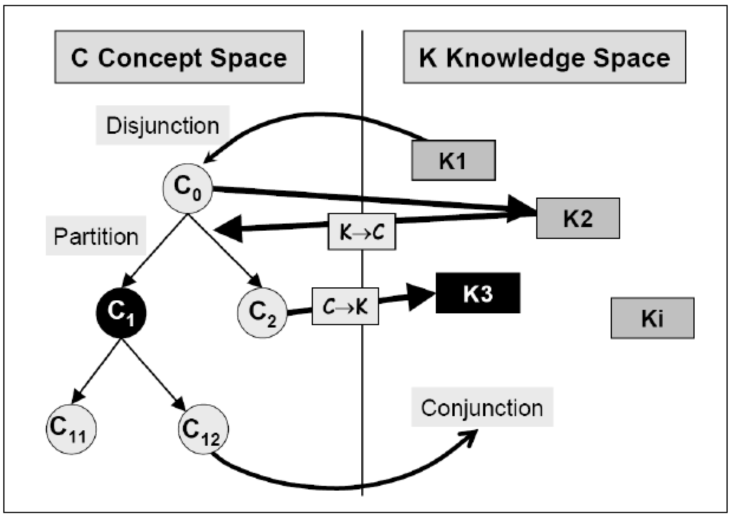
A first step in this direction comes from the evaluation of Internal Corporate Venture Projects. In a recent paper Keil, Mc Grath & Tukiainen (2009) connect the real options and capability perspectives to propose an enlarged evaluation framework for internal corporate venture. They rely on and extend the capability life-cyle proposed by Helfat & Peteraf (2003) to build a model in which, at each milestone, a new venture is evaluated by what it delivers and by the capability it builds. Thus, a venture can be a complete commercial failure, but create or transform organizational and/or individual capability. The authors propose to evaluate a venture's contributions according to three dimensions: outcomes, capability creation, and capability development and transformation. This combination of both the real-options and evolutionary frameworks generates interesting insights for evaluating exploration projects. It focuses on capability building at the firm or sector level over a long period of time (e.g. Helfat & Raubitschek, 2000). It does not, however, explain how capabilities are created (Nonaka & Takeuchi, 1995), nor is it able to keep track of the exploration process itself. For example, it does not explain how, in which context, and for which purpose capabilities are created. In our view, this is where design theory provides a significant contribution.

The contributions of design theory

Since its inception, design theory has attempted to develop models of the designers' thought, 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). Nevertheless, their focus on the evolution of a design leaves out the knowledge associated with particular decisions. This lacuna is filled by Hatchuel and Weil's C-K theory (see Hatchuel & Weil 2009 for a synthesis).

The C-K theory describes design reasoning as the interaction between two spaces, the concept space C and the knowledge space K. Design begins with an initial value concept, a proposition that is neither true nor false, i.e. undecidable in the K space (hence the notion of a "K-C disjunction"). The concept, let us say of a "flying boat," cannot initally be said feasible or unfeasible, marketable or not. The design process consists in refining and expanding it by adding attributes coming from the knowledge space (flying boats have sails or motors, hull, foils or wings, and so forth). The process can also lead to the production of new knowledge to be used in the design process, for example as result of an experiment conducted to understand the effect of the foils on the boat's behaviour. The initial concept set is thus partitioned into several subsets. The process unfolds until one refined concept is sufficiently specified to be considered as true by the designer. At that point, the concept becomes a piece of knowledge (hence the notion of a "C-K conjunction"). 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).



As we have shown in previous work, the C-K theory provides a very useful framework to manage exploration projects (Lenfle, 2008a&c ; Lenfle & Midler, 2010) and evaluate their outcomes. For example, following Le Masson & al. (2006), we identified four different results of exploration projects (Lenfle, 2008a&c):

1. Concepts that, after development, become commercial products.
2. Concepts that have been explored but not pursued due to lack of time or resources.
3. New knowledge that can be reused on other products (e.g. components, technical solutions, new applications).
4. New knowledge that has not been used during the exploration but can be useful for other products.

Similarly, in a recent paper, Elmquist & Le Masson (2009) demonstrate the relevance of C-K theory to assess the value of "failed" R&D projects.

In the next section, we will use the Manhattan project, in our view an archetype of vanguard exploratory project, to illustrate the strength of this approach.

3. Research method

This paper is part of a broader investigation aimed at revisiting the roots of project management, especially very innovative ones, to discuss the relevance of the dominant control-oriented view of PM (Lenfle, 2008; Lenfle & Loch, 2010; Lenfle, 2011). Surprisingly, historical materials, widely used by historians, sociologists of technology or economists, are rarely used by scholars working on project management or innovation. History, however, constitutes a powerful way to test the relevance of existing theory or to generate insights on contemporary questions (Kieser, 1994).

The Manhattan Project was an obvious candidate for two reasons. First, the making of the atomic bomb unquestionably represents a major breakthrough in the history of technology. It exemplifies the power of "Big Science," the mobilization of important human, financial, and industrial resources to overcome major scientific and technical problems. Studying how the breakthrough happened may provide insights into innovation management.

Second, the Manhattan Project occupies a particular place in the literature on project management. It is frequently presented as proof of the power of projects. Gaddis (1959), in a seminal paper, highlighted its incredible success, and Morris (1994) argued that the development of the atomic bomb "*certainly displayed the principles of organization, planning and direction that typify the modern management of projects*" (p. 18). More recently, Shenhar and Dvir (2007) wrote that "*[t]he Manhattan Project exhibited the principles of organization, planning, and direction that influenced the development of standard practices for managing projects*" (p.8). However, a careful analysis of the Project does not confirm these claims (Lenfle & Loch, 2010). Rather, it reveals that the Project leaders ignored most of the best practices in classical project management. Such a tension between the common perception of the Manhattan case and how the Project really developed is relevant for disentangling the strands that led to the current dominant view of project management, and therefore provides an opportunity to revise the concept of project management itself.

Fortunately for us, the Manhattan Project has been extensively studied, mainly by historians, and its relevance no longer needs to be proved. We were therefore able to draw on a large amount of historical material which, however, has not yet been used to study the management of exploration projects. It is not possible to provide a comprehensive account of the Manhattan project, nor is doing so our present goal. Instead, we have focused on a specific set of events likely to reveal the problems raised by the evaluation of exploration projects; Langley (1999) has characterized such a strategy as *bracketing events for theoretical purposes*. At the same time, we have included details of the events critical for conveying and illustrating our thesis that design theory may provide models to enlarge the current view of project evaluation. We relied both on the "official" history of the Manhattan Project (Smyth, 1945; Helwlett & Anderson, 1962; Groves, 1962; Jones, 1985), and on more recent work (especially Rhodes, 1986). We also drew from research about an individual (e.g. Bird & Sherwin, 2005) or a specific part of the Project (e.g. Hoddeson & al., 1993). Given the information available, we considered that the point of "theoretical saturation," which Glaser and Strauss (1967) proposed as criterion to stop collecting data, had been attained. Our analysis may therefore lack empirical originality, but will hopefully triangulate the data in original ways.

4. The Manhattan case[2].

4.1. Origins of the project

The Manhattan Project was part of the global mobilization of US science during WWII. A. Einstein's famous letter to President F. Roosevelt of August 2, 1939 highlighted the military significance of atomic power . It did not at first lead to a concrete project, and until 1942, the overall effort to develop an atomic bomb remained 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, and they really take off with the appointment of General Leslie Groves, a member of the Army Corps of Engineers and a very experienced project manager (see Norris, 2002), on September 17, 1942.

4.2. A Scientific and Technical Everest

The main goal of the Manhattan project was to design an atomic bomb before Germany, and to use it to end the war. However, the pursuit of these goals was far from obvious since the basic research and development had not been done. To understand the difficulties the project had to face we need to introduce a bit of nuclear physics and identify the main design problems raised by the making of an atomic bomb.

4.2.1. Nuclear physics for dummies

The Manhattan Project did not start from scratch. As physicist H.D. Smyth explained in his report released in August 1945, just after the atomic bombing of Hiroshima and Nagasaki, "*The principal facts about fission had been discovered and revealed to the scientific world. A chain reaction had not been obtained but its possibility – at least in principle – was clear and several paths that might lead to it had been identified*. (p. 364)". But he immediately specified that "*[a]ll such information was generally available; but it was very incomplete. There were many gaps and many inaccuracies. (…) Although the fundamental principles were clear, the theory was full of unverified assumptions, and calculations were hard to make. (…) 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 hits an atom of uranium, the atom splits into two, releasing energy and additional neutrons; this begins a self-propagating chain whereby neutrons again split the two resulting parts. Two of the major problems involved in understanding and marshalling this process consisted of finding the critical mass of fissionable material needed to start and sustain a chain reaction, and of measuring the

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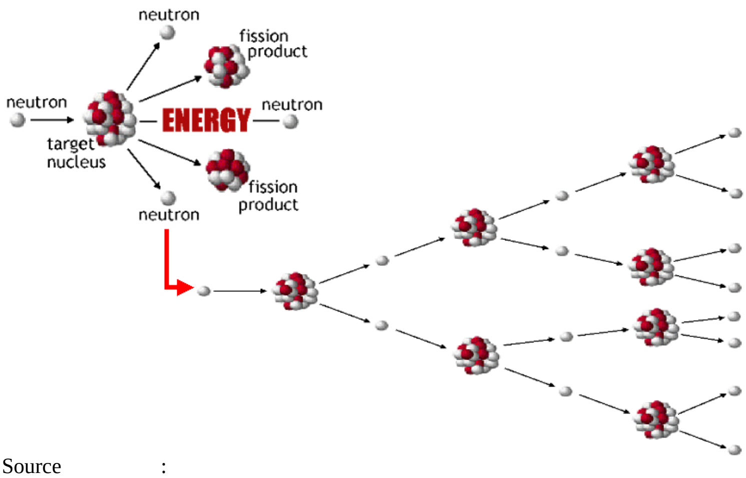
[2] This section draws heavily on Lenfle (2008b), which provides a more detailed description of the case.

number of neutrons released at each step (the reproduction factor, k), knowing that they can be lost or absorbed by other materials.

Figure2. The principle of nuclear chain reaction.



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

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

4.2.2. From theory to practice…

The first self-sustaining nuclear reaction was obtained in December of 1942 by Enrico Fermi and his team. After that, the Manhattan project faced two major problems: the production of fissionable materials and the design of the bomb itself.

A. The production of fissionable materials

At the beginning of the project, two materials were identified as being able to sustain a chain reaction. The first, uranium (U) 235, is a natural component of U238, but represents 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 cases, the production of fissionable materials raised huge scientific and technical problems:

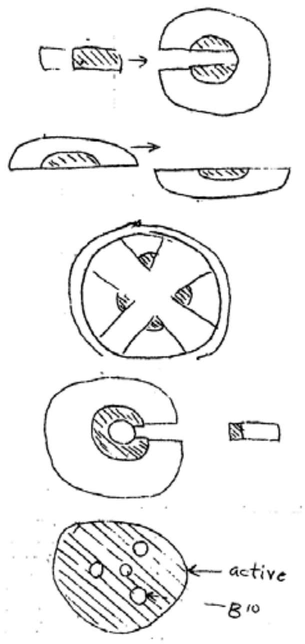
- Separating U235 from U238 involves extremely complex processes, based on the slight difference (less than 1%) in the atomic mass of the two isotopes. To perform this task seven different methods had been identified in 1941 among which, as we will see, three will finally be used (Smyth, 1945).
- In 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.

These were breakthrough innovations. Some of the processes did not exist before the project (plutonium production) or had never been used with radioactive materials (chemical separation), and the available knowledge on 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 explained that "*[m]any 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).

B. Alternative bomb designs

The team faced the same situation concerning the design of an atomic bomb. In a seminar organized by R. Oppenheimer at Berkeley in July 1942, scientists met to discuss several possibilities (figure 3 from Serber, 1992). A number of alternative designs were envisioned: the gun method (at top), the implosion method (center), the autocatalytic method, and others. However, the Berkeley discussion was theoretical, with no prototypes built or experiments undertaken. It remained to be determined, for example, whether a "gun" design would work for uranium and plutonium, or whether an "implosion" device was feasible.

Figure 3. Alternative bomb designs at the Berkeley seminar (July, 1942)



4.3. Managing the unknown[3]: parallel strategy and concurrent engineering.

Such a situation had fundamental managerial implications. The most important was that the entire project was first and foremost characterized by unforeseeable uncertainties (Loch & al, 2006). 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). At the beginning of the project, the necessary knowledge was largely nonexistent. Thus, recalling a meeting with scientists at the University of Chicago in October 1942, Groves wrote that "*[t]here 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*" (Groves, 1962).

Considering unforeseeable uncertainties, Groves and the Steering Committee (most notably V. Bush and J.B. Conant) decided to explore and implement simultaneously the different solutions, both for the production of fissionable materials and for bomb design. Moreover, given the utmost importance of time, they chose to proceed concurrently, doing fundamental research, designing, and building the plant at the same time. Groves had already used concurrent engineering in past projects, but it was the first time the strategy was extended to fundamental research (see Thayer, 1996, on the management of the Hanford Project by DuPont). 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*" (Groves, 1962, p. 253). Figures 4 and 5, respectively, trace the project managerial strategy and unfolding on the basis of published sources (see references).

It is striking to note the simultaneity of the different tasks:

- Uranium separation, plutonium production and bomb design proceeded concurrently;
- Two different methods were used in parallel for uranium separation, a third one was added late in the project (September 1944, we return below to this point), and the Los Alamos laboratory explored several methods at the same time. They first focused on the "gun" design but, as we will see, switched to "implosion" in July 1944. A third group led by Edward Teller, with much smaller resources, began work on the "super," a thermonuclear weapon[4].

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

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

[4] It was clear for Oppenheimer that this third design was too radical an innovation to be ready for use during the war. However, given its potential, theoretical work on it was conducted at Los Alamos during the entire project.

Figure 4: organization of the project [5]

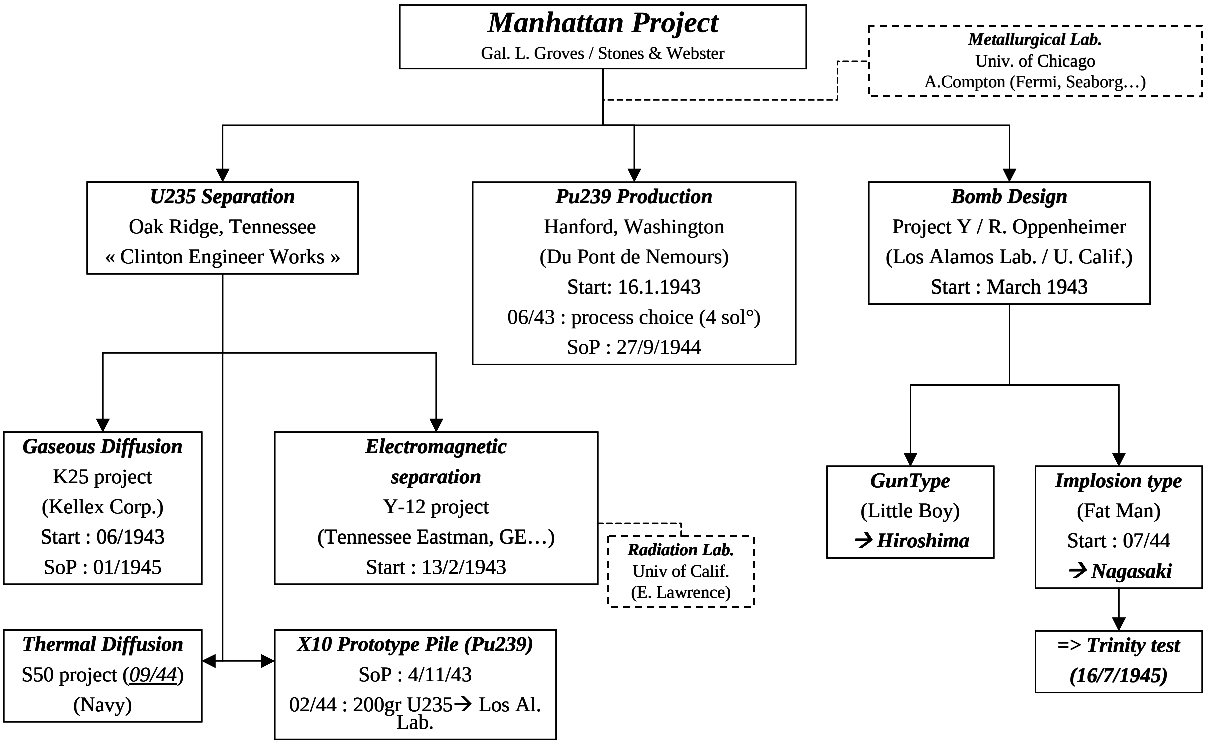
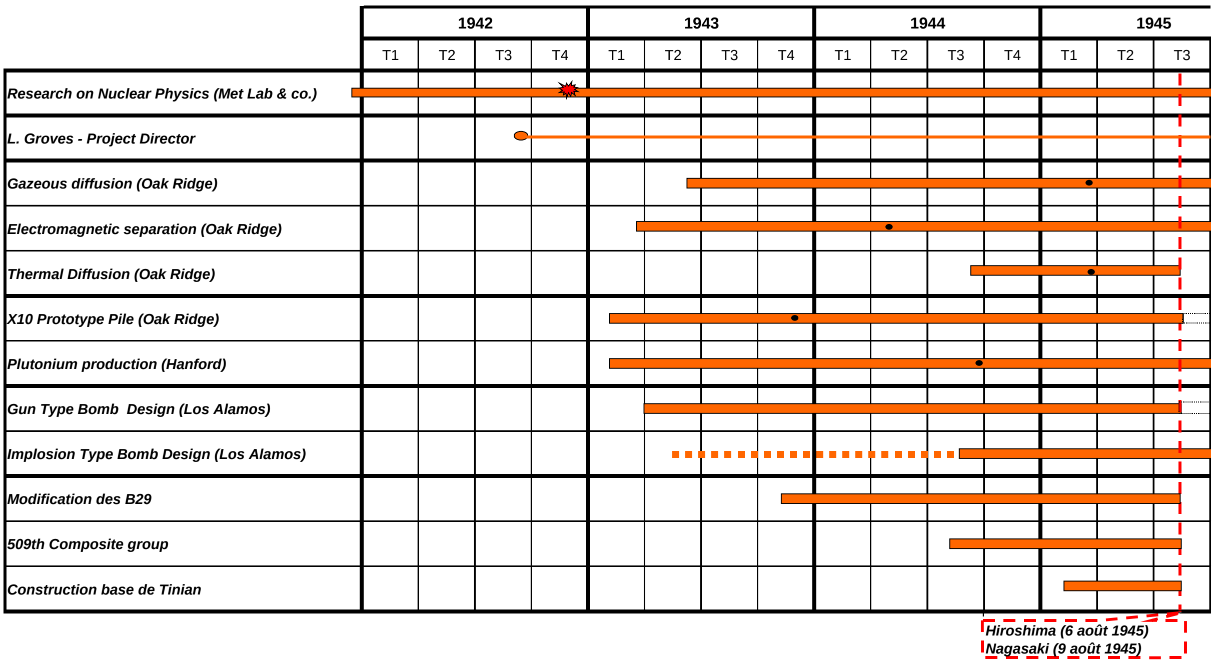


Figure 5: planning of the Manhattan Project



[5] This figure describes the "scientific" part of the Manhattan Project. A complete description is available in Lenfle (2008b).

4.4. The paths to the A Bomb

Since our goal is not to provide an extensive description of the Manhattan case (see Lenfle, 2008, for a synthesis) we summarize here the events in the crucial period from Spring of 1944 to the end of the Project. By Spring 1944 none of the methods for producing enriched uranium had achieved sufficient accretion rates, and the "gun" design for the bomb had turned out to be unsuitable for plutonium, which exhibited a much higher "spontaneous fission" rate than anticipated. The Project had maneuvered itself into a dead end, with a fissionable material (plutonium) without a bomb design, and a bomb design (the "gun") without a workable fissionable material (uranium 235). The parallel approach describe above offered the means to overcome the crisis:

- For the production of fissionable materials, a breakthrough took place when it was discovered that a process discarded at the beginning of the project, thermal diffusion, could provide slightly enriched uranium, which would then feed the gaseous diffusion and electromagnetic processes for further enrichment. The parallel processes were unexpectedly combined into a composite process that finally, in March 1945, achieved the desired performance (Lenfle, 2011).
- For bomb design, a second group of scientists had worked on an implosion design as a back up. When it became clear in the Spring of 1944 that the gun approach did not work for plutonium, the implosion design became first priority. Still, unprecedented challenges had to be overcome because the implosion had to be perfectly symmetrical in order to achieve a chain reaction. This demanded mastery of the hydrodynamics of implosions – a new, uncharted field (see Hoddeson & al., 1993 for a detailed history of the implosion program).

The herculean scientific and engineering efforts finally led to a radical innovation in weapon design: the implosion bomb. The design was frozen very late, probably on February 28, 1945. Oppenheimer then created the "cowpuncher committee" to oversee the final phase [6, chap. 15 and 16]. Yet, the remaining uncertainties concerning the new device were so great that Groves, finally but reluctantly given the considerable cost of such an experiment, 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; the bombing of Hiroshima (with the uranium/gun design) and Nagasaki (with the plutonium/implosion design) followed three weeks later.

5. Analysis

How can we evaluate the results of this type of project?[6] The first approach, in the QCT tradition, seems straightforward even if there were no clear requirements at the

[6] We deliberately leave aside the very complex ethical debates that surround the Manhattan project. Therefore we don't discuss here the necessity to drop the bomb on Japan (see Malloy, 2008, for a recent synthesis) or the long-term effects of the building of a nuclear industry (e.g. environmental impact of

beginning of the Project (except that of building an atomic bomb as fast as possible): in two-and-a-half years, at the cost of 2 billion 1945 dollars, scientists and engineers succeeded in developing one of the most radical technological innovations of the twentieth century. This in itself is an extraordinary performance. However, limiting to it the results of the Manhattan Project is misleading and misses its contributions as a vanguard exploratory project.

Indeed, one of the most important challenges of that kind of projects is to explore a very complex design space (see Loch & al, 2006). In the language of C-K theory, the design space explored by the Manhattan Project was "generative" in nature, i.e. it was in perpetual expansion (Hatchuel, 2002). The more you explore it, the more options you discover. This means that the more the project advances toward its goal, the more it discovered new paths, new solutions, new problems, potential applications, and so on. H. Smyth highlighted this feature, in connection with the Met Lab research program, remarked 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"* (Smyth, 1945, p. 409). The logic of convergence predominant in the QCT framework is unsuitable for understanding such a situation. The challenge here is to find ways of dealing with the "divergence" (Van de Ven & al., 1999) characteristic of an expanding design space. The fact that certain projects produce much more knowledge than they need and can use has important managerial implications. The goal of such projects is to define what will be launched first, and what will come next. They thus build the foundations for "lineages" of products (; Le Masson & al, 2006) – in this case from gun fission to thermonuclear weapons. This is why, in a previous paper (Lenfle, 2008a), we emphasized the dual nature of such projects' performance, which encompasses both "products" and knowledge (see section 2, page 9). The knowledge dimension, generally considered as a project by-product that becomes important only after project completion (see Lenfle, 2008a for a discussion), is in fact central *while the project is being carried out*, and implies the constant adjustment of project objectives. The Manhattan case also illustrates this.

First consider the obvious result, the atomic bomb. The final result is not what was expected at the outset since the first design, the "gun" weapon, was unsuitable for use with plutonium. The project thus switched to the implosion design. So the result was not one, but two completely different bombs. But there was more than this.

As mentioned above, a third design was studied by the project: the "super". Even if it was quickly given a lower priority, research on it never stopped at Los Alamos. Furthermore, the project generated an extremely rich knowledge base in various fields, which would later expand and can be considered the cradle of the nuclear industry (military at first, but also probably civilian).

It is interesting here to follow the analysis of Hoddeson & al (1993, p. 416). As they note, "*the application of the Los Alamos at the nuclear weapons laboratory was direct*

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nuclear wastes, societal cost of the ensuing arms race, etc.). These are of course important questions but they are beyond the scope of the present article.

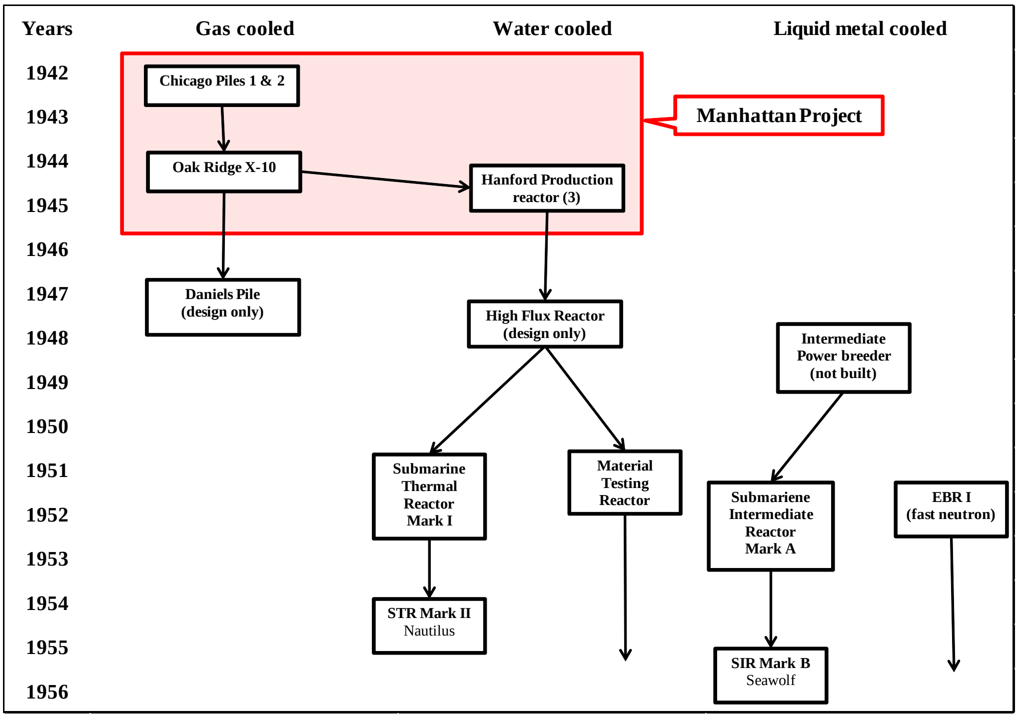
and massive". Rosenberg (1983) shows that "*[t]he 'nominal' 20 kiloton yield of the Mark 3 bomb* [an evolution of the Fat Man design] *was multiplied by 25 times between 1948 and 1952. These included advances in design, composition, stability, and power of the high explosives used to detonate a fission core, and improvements in mechanics, structure and composition of the fissile pit itself* [i.e. the plutonium core][7]". But as they also demonstrate, the Manhattan Project's technological contributions covered "*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 properties of plutonium metal were outlined, the correct formula for uranium hybrid was identified, fundamental properties of many explosives were discovered*,[etc.]. *(…) Transfer of information from the MIT Radiation Laboratory enabled Los Alamos to refine the development of amplifiers, scaling circuits, and multidiscriminators. (…) 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. (…) 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.* (…) *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*" (Hoddeson & al., 1993, p. 416-417). Another example of the post war follow-up of the Manhattan project is provided by Hewlett & Duncan in their history of the development of the Nuclear Navy (1974). They show how the reactors designed during Manhattan provided the basis for post-war submarines (figure 6 below).

Crucial here is the fact that the Manhattan Project leaders, specifically R. Oppenheimer at Los Alamos, deliberately decided to pursue research on long-term questions with no direct relevance for the current goals. As Hewlett & Anderson noted, "*under the circumstances, it was remarkable that they were able to spend any time on projects that look to the future*" (1962, p. 627). In our view, however, this is not a by-product, but an absolute necessity when a team explores a generative design space. The plurality of time horizons and the duality of short-term goals and long-term research to prepare the next steps lies at the heart of exploration project management (Lenfle, 2008a). Therefore, instead of focusing narrowly on the most urgent objectives, which obviously remain a top priority, the project manager has to engage *simultaneously* in preparing the subsequent steps. As illustrated by the Manhattan case, this involves fundamental research or second-order solutions (e.g. underwater weapons, the "super"), and increases the probability of long-term success through the creation of new capabilities upon which

[7] The use of numbers (Mark X, etc.) illustrates in itself the lineage concept, each new generation building on knowledge generated by its predecessors.

lineages of products will build. It is in this perspective striking to notice how L. Groves organized the transfer of knowledge after the Project's completion.

Figure 6: evolution of post-war reactor concepts (Hewlett & Duncan, 1974, p. 66)



For example, immediately after the war a course was organized at Oak Ridge, "*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 (see Hewlett & Duncan, 1974). In the terms of the Brady-Davies model (2004), what we see here is more than a *base-moving project*. We should rather talk of *base-creation*, since the Manhattan project literally lays the foundations of the US nuclear industry as far as competencies, plant, and designs are concerned.

6. Discussion and further research

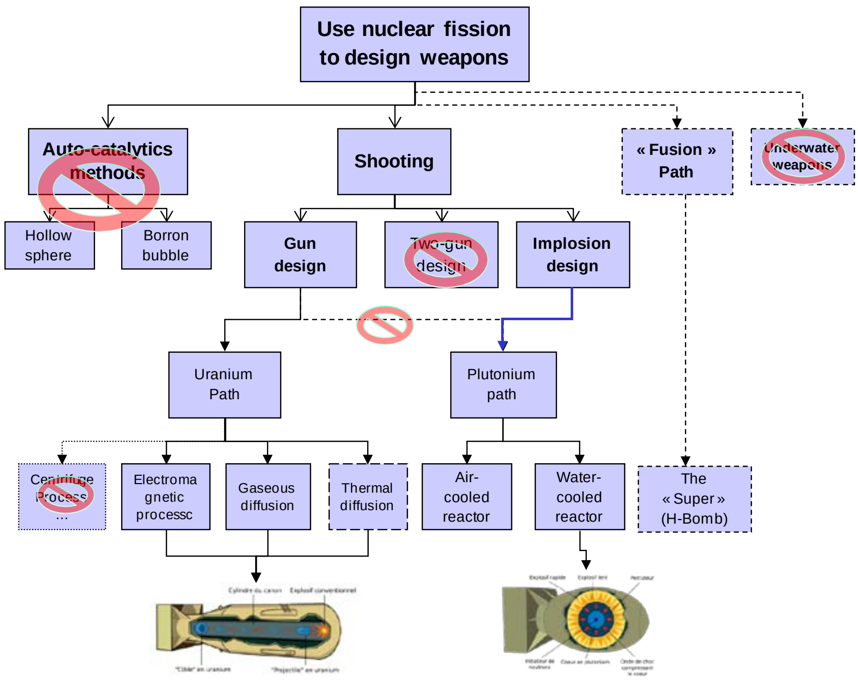
6.1. C-K design theory and project evaluation

The dialogue between project management and design theory opens the possibility of new frameworks to evaluate and manage exploratory projects. To fully grasp the potential contribution of such a dialogue we need to keep in mind that the goal of such projects is, as stated by McGrath, "*to gain knowledge about an unfamiliar landscape*". We think that this is exactly what design theory can do. We have used the C-K theory to summarize the main contributions of the Manhattan project (figure 7 below). What have we learned from it?

First, this figureconstitutes a first mapping of the design space traveled by the project. It brings to light the conceptual and knowledge dimensions of the different paths explored. It explains how they reached their goal: in the Manhattan case, by producing two different bomb designs relying on different production processes, and leading to the constitution of broad capabilities in nuclear design ("knowledge space" in figure 7 above).

Figure 7: C-K and the "results" of the Manhattan Project

Concept Space



Knowledge space

- Nuclear chemistry
- Nuclear physics
- Nuclear engineering
- Metallurgy
- Sciences of explosives
- Weapon design (implosion…)
- Electronics & microelectronics (computers, scientific instruments…)
- …

Hoddeson & al., 1993, p. 416

Note of this graph: We rely on available sources to map the project journey (especially Serber, 1943, who describes the different bomb design envisioned at the beginning of the Manhattan Project). This is of course an oversimplification. We have only represented here the main paths explored by the project. As explained by Smyth (1945), each part of the project opened entirely new fields of investigation. It is thus impossible to display the entire Manhattan Project with one graph; different graphs would be required to represent design processes within the different parts of the Project.

But it also shows the paths not taken or abandoned along the way. Thus, on the conceptual side, Figure 7 shows that:

– Some uranium separation processes, such as centrifuge separation, were discarded early on, but could be used in the future[8]
– Some processes, such as thermal diffusion, were discarded but used when the project faced major problems
– The design of other types of weapons, such as atomic torpedoes, was studied, but did not give rise to development
– Research on fusion was begun even if in 1945 it remained a remote possibility

The same type of analysis could be applied to the knowledge side of the project (see for example chapter 20 in Hoddeson & al.).

[8] Centrifuge separation is now (with gaseous diffusion) the main industrial process to enrich uranium.

Such an approach is, in our view, very useful to evaluate and manage exploration projects since it provides a representation of what has been learned and of the array of choices for the future. As Clark (1985) points out, project managers can now decide to go down the hierarchy to refine existing concepts, go up and re-open previously frozen parameters in order to depart from existing approaches, or do both. On the knowledge side, C-K theory helps to identify what has been learned and what is to be learned, thus defining questions for future research programs (for example on fusion or on the metallurgy of plutonium). Design theory thus offers the opportunity to assess and discuss a project's current and future trajectory. It thereby enhances the real options framework, and could help renew project evaluation and management techniques. By representing what a project produces in concept and knowledge – i.e. beyond its immediate deliverables – it completes the evolutionary perspective as well as Shenhar & Dvir's "preparation for the future" dimension of project success.

6.2. The road ahead

Several important questions remain open and deserve further research. High on the agenda is the study of the implementation of the model we sketched in ongoing exploration projects. Here we have used ex-post historical data. However, applying the model to real ongoing projects is another story, and there is very little work on the implementation of C-K theory in organizations. In a study of exploration projects in the case of automotive telematics services (Lenfle & Midler, 2010) we relied on the C-K framework to explore new design spaces. Elmquist & Le Masson (2009) in the case of transportation services, or Le Masson & al (2011) for semi-conductors, have provided other illustrations of C-K method implementation. Among the questions that deserve further research are: Which problems prompted the implementation of C-K in ongoing projects? Who is using the method (project members, stakeholders)? For which purpose (e.g. to evaluate a project at milestones and/or to enhance collective reflection-in-action; Schön, 1983)? What could be the role of management scholars in this process? At first sight we can envision three different uses of this model:

1. As this article has hopefully demonstrated, the C-K framework is very fruitful to analyze ex-post a project's journey in terms of products / concepts / knowledge. Therefore it can provide the basis for post-projects reviews, which are often neglected;
2. It could also be used during a project to represent a current strategy and discuss the project's future. This is what Felk (2011) performed at ST Microelectronics, though without explaining the specific challenges of implementing C-K. In such contexts, the method may enhance collective reflection-in-action (Schön, 1983) and debate at different levels while the project is still under way (what Van de Ven & al., 1999, call *decision-making by objection*)
3. Finally, the method could probably be used at the evaluation stage prior to getting funding in order to present a chosen design strategy and discuss its relevance.

No doubt these uses of the C-K framework raise particular challenges, since the framework is easier to use ex-post than with partial information before a steering committee. So the actual use of C-K theory in organizations deserves further research.

At a more general level, the issues we raised allow us to revisit how project management deals with exploration, and to consider projects as first and foremost ways of organizing the exploration of emerging innovation fields. This entails a major shift in project management methodology, since it is no longer possible to define ex-ante a specific goal (or only very broadly, as in "design an atomic bomb") and the means to reach it. Projects thus became highly uncertain, and are to be characterized as reflexive probe and learn processes. We have here tried to demonstrate the fruitfulness of an approach that bridges project management and design theory. Their articulation may help us design new tools to represent, and thus manage, innovation processes. In particular, project evaluation should explicitly take into account the knowledge produced in the framework of exploratory projects (Iansiti & Clark, 1994; Engwall, 2003; Keil & al., 2009), as well as the fundamentally *discovery-driven* nature of the latter (Mc Grath & McMillan, 2009).

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