When Project Management Meets Design Theory: Revisiting the Manhattan and Polaris Projects to Characterize 'Radical Innovation' and its Managerial Implications

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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 C/K theory, how these major projects, traditionally presented as radical innovations, are in fact quite different. In particular we show that the structure of the knowledge base (splitting or non-splitting) has major consequences. 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 discuss the implications of this result for design theory and project management.

Introduction

In this paper, we propose using recent advances in design theory to discuss the relevance of the dominant model of project management (PM) and its limitations in situations of innovative design. We thus propose revisiting two landmark cases in the history of PM: the Polaris Project (1956–1960) and the Manhattan Project (1942–1945). The first project is famous in the PM field for its development of the program evaluation and review technique (PERT), a planning method. Thus, it exemplifies a case of 'traditional' PM. The second project is a perfect example of a radical innovation, namely, the atomic bomb. Although presented as the root of modern PM (see, e.g., Morris, 1997), this case has recently been re-examined by Lenfle (2008, 2011) and Lenfle and Loch (2010), who demonstrate that this presentation is notoriously wrong. On the contrary, the Manhattan Project exhibits very original managerial strategies, referred to as *selectionism* by Loch, DeMeyer and Pich (2006), which can be applied in the exploratory projects that are increasingly important for today's innovation-based competition (Lenfle, 2011). It is interesting to study the discrepancies between the two cases not only because these projects are usually presented as radical innovations but also because some general analytical frameworks fail to distinguish between them.[1] Indeed, in terms of technology readiness level (TRL), it seems at first glance that both projects had to go from very low to very high TRL and that, in Brian Arthur's terms, they used newly discovered phenomena and required complex combinations of existing techniques (Arthur, 2009). However, as we will see, there were fundamental differences between the design problems they faced. This, we believe, explains why the two projects do not rely on the same PM models; one relies on PERT, while the other clearly does not. Thus, there seems to be a

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'hidden' contingent variable that characterizes both the innovation issues and the PM model adapted to these issues.

In this paper, we use design theory to characterize the differences in the designs of the Polaris Project and the Manhattan Project. Indeed, recent theories, such as concept-knowledge (C-K) design theory (Hatchuel & Weil, 2009), constitute powerful tools to follow complex design processes in situations ranging from science projects requiring intensive knowledge creation to creative design processes (for creativity methods and C-K design, see Reich et al., 2012; for science-based products, see, e.g., Gillier et al., 2010; see examples in Le Masson, Weil & Hatchuel, 2010). Based on C-K design theory, this paper characterizes the design problems that the two projects encountered and explains why their managerial strategies and PM models were so different. In so doing, we hope to demonstrate that bridging PM and design theory constitutes a fruitful research field for the future, particularly for new product development projects.

The paper is organized as follows. The next section states the theoretical problem by reviewing the literature on PM and design theory. The Polaris Project and the Manhattan Project are presented and discussed in the subsequent two sections. We then present the main results, and the final section discusses the managerial implications of this research.

Literature Review

Toward a Contingency Theory of PM

One of the most important recent evolutions in the PM field is the development of a form of 'contingency theory' of PM. Indeed, the PM field has long been dominated by a rational, instrumental approach that aims to achieve a clearly defined goal within budget, time and quality constraints (PMI, 2013). This perspective is now widely criticized. In particular, it is well established that this model fails when confronted by innovations that render its hypothesis (i.e., the ability to identify a clear objective, to plan the work, etc.) irrelevant (Loch, DeMeyer & Pich, 2006; Lenfle, 2008; Sehti & Iqbal, 2008; Davies, 2013). This failure leads several authors to critique the 'one-size-fits-all' approach underlying the dominant model (Shenhar & Dvir, 2007). These authors propose to differentiate the management of projects according to the problems that each project faces. The influential work of Shenhar and Dvir distinguishes between 'traditional' and 'adaptive' PM, the latter being better suited for innovation (see also Davies, 2013). In line with this work on project classification, we believe that a distinction should be drawn between the various design situations for which different types of projects will be well suited. However, this approach highlights an important problem, namely how to characterize the 'nature of the problem' faced by a given project. The prior research on this question relies on relatively general criteria:

- In their famous 1992 paper, Wheelwright and Clark propose classifying projects according to two criteria: the degree of product change and the degree of process change. This classification leads them to distinguish among derivative, platform, breakthrough and research, and advanced development projects. The more innovative the project, the more autonomous the project team should be.
- More recently, Shenhar and Dvir (2007) have proposed the 'diamond approach', in which four criteria are used to classify projects: novelty, technology, complexity and pace (NTCP). Therefore, they not only show that 'traditional' PM is compatible with relatively simple projects that develop incremental innovation but also discuss the implications of the 'adaptive' model for more radical innovations.

In each case, however, categories such as 'product change', 'novelty' and 'technology' can be criticized as too broad. For example, the technology classification is on a continuum from 'low-tech' to 'super high-tech', as proposed by Shenhar and Dvir, which is very difficult to operationalize. Indeed 'super high-tech' is defined as a case in which '*projects are based on new technologies that do not exist at project initiation*' (p. 48). However, this definition is relatively broad. Theoretically, it is grounded in Abernathy and Clark's (1985) landmark typology of innovations, which relies on the notion of market/technology novelty, with more novel innovations being competence-destroying. This typology highlights the issue of knowledge renewal (acquisition, creation) during the project. Later works provide more detailed elements related to the variety of knowledge evolutions involved in innovation. In particular, Henderson and Clark's (1990) introduces the famous distinction between architectural and modular innovation; they show that the issue is not simply the 'quantity' of knowledge but also its 'structure'. In the case of architectural innovation, the relationship between the components (and the skills related to these components) is changed (which also requires a form of knowledge creation); in the case of modular innovation, there might also be a difficult knowledge challenge in one module, but the relationship between that module and the rest of the object remains unchanged. This distinction leads to an important research stream on the problems raised by architectural innovations and the power of modularity (Baldwin & Clark, 2000; McCormack, Rusnak & Baldwin, 2012). These works show the competitive advantage of owning a particular architecture that can become a platform leader (Gawer, 2009) and enable an increase in some forms of agility based on modular evolutions (Sanchez, 1995; Sanchez & Mahoney, 1996). This research echoes works on firm strategy and organization that claim that a firm's identity is created through its 'combinative capabilities' (Kogut & Zander, 1992), i.e., the architecture that the firm masters and the 'modular' combinations that it can build using these architectures to create a new product. These works show that the question of architecture and its corresponding combinative capabilities is central to radical innovation.

Thus, the literature invites us not only to consider that an innovative project must create knowledge, but also to distinguish between projects that maintain architectures and projects that renew both architectures and combinative capabilities. However, the literature does not resolve the question of the *emergence* of the architecture and the creation of *new* combinative capabilities. Henderson and Clark (1990) have shown that incumbent firms are often unable to manage architectural changes. Later works have shown why incumbent firms face difficulties in this regard. For instance, Kogut and Zander (1992) argue that combinative capabilities are the firm's genes, which cannot be easily changed. However, the issue of managing the *creation* of the architecture is often either ignored or considered as a random evolutionary process that is subject to the firm's entrepreneurial skills. There is a methodological and analytical reason for this approach; in a case involving stable architecture and combinative capabilities, it is possible to study knowledge creation and renewal by observing knowledge increases in each module or functional domain. The value of knowledge creation can be related to the value of the module or its function in the architecture. In the case of architectural change, it is possible to measure the value of the *final* architecture. That said, how is it possible to characterize knowledge changes *during* the process, before a single stable architecture is identified? Between the 'old' and 'new' architectures, how can one characterize the relationships between pieces of knowledge? How can one analyse the consequences of the transformation of these relationships in terms of the final innovative output? How can one understand the management of these transformations? The intersection of PM and design theory is important here because we believe that understanding this question requires a fine-grained analysis of the design reasoning in question. We thus rely on C-K design theory, which allows this type of analysis of radical innovations. This theory is the topic of the next section.

Design Theory to Formulate Research Hypotheses on the Management of Breakthrough Innovation Projects

The Primary Features of C-K Design Theory for Analysing Projects

Since its inception, design theory has attempted to develop models of designers' reasoning, modelling the interaction between knowledge and innovation and leading to the development of tools that can organize and/or rationalize the design process (Simon, 1969; Suh, 1990; Pahl & Beitz, 1996; for a history of design theory, see also Le Masson, Hatchuel & Weil, 2011). Marples' seminal 1961 paper includes a design tree of engineering design decisions, which enables an understanding of the different options studied by nuclear reactor designers. Subsequently, the same approach has been used by Clark to analyse the implications of innovation (Clark, 1985). Still, these representations have been based on a decision-making process; the tree shape described a search process in a complex decision space – to enable the theory to account for the emergence of new combinations. However, these combinations were actually *determined* by the combination laws defining the decision space. In other words, based on this theory, one cannot account for the emergence of new combination capacities that have not been provided in the initial decision space. In recent decades, it has been demonstrated that decision-making models cannot account for design processes when these processes tend to precisely regenerate the space of constraints and the space of design capacities (Hatchuel, 2002; Dorst, 2006). Several design theories have been proposed to account for the increasing number of generative processes. General design theory (Yoshikawa, 1981; Reich, 1995), axiomatic design (Suh, 1990), the coupled design process (Braha & Reich, 2003), infused design (Shai & Reich, 2004) and C-K design theory (Hatchuel & Weil, 2009) are formal theories that go beyond decision-making theory and account for the processes that help create new objects from known ones by expanding their initial space into one that is both newer and broader (for a comparison, see Hatchuel et al., 2011a).

These works led finally to design theories, such as C-K design theory, that possess three critical properties:

1. Design theories currently account for innovative design processes that go far beyond a mere combination of existing knowledge to include strong knowledge expansion. Accordingly, they are particularly well suited for the study of radical innovation processes (Le Masson, Weil & Hatchuel, 2010; Hatchuel, Le Masson & Weil, 2011b).
2. The generativity of design theories far surpasses the generativity of combinations among known objects in a given set of combination laws; design theories model the generation of new object definitions and the generation of new combination laws. Therefore, they can help analyse projects that ultimately lead to regenerated architectures (Hatchuel, Weil & Le Masson, 2013; Kokshagina, Le Masson & Weil, 2013).
3. Design theory, and particularly C-K design theory, has already been successfully used in the study of innovation processes; therefore, it has proven its 'user friendliness' and efficiency as an analytical tool for innovation processes (see, e.g., Elmquist & Segrestin, 2007; Elmquist & Le Masson, 2009; also Gillier et al., 2010; Lenfle, 2012).

For these reasons, we decided to rely on C-K theory as an analytical framework to study breakthrough innovation and PM. C-K design theory imagines a design process that begins with a set of propositions that are considered true (they are in the K-space, which contains all of the propositions that are considered true) and with one proposition that is neither true nor false (technically, it is called a disjunction). Clarifying the starting point of a design process is one of the main advantages of C-K design theory. More specifically, the starting point is a proposition that has not yet become true. Moreover, it is impossible to prove at the beginning that the proposition is impossible (e.g., non-marketable or unfeasible). For instance, in 1943, the proposition 'there is an atomic bomb' was a concept; nobody could produce an atomic bomb, nor could anyone prove that it would be impossible to build an atomic bomb. A proposition that is neither true nor false cannot – by definition (see above) – reside in the K-space; 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 of using a proposition known in K to refine and 'expand' the proposition in C and to use the proposition in C to create a new, true proposition in K. In C-K design theory, design is a dual expansion process: it creates both new concepts and new knowledge. The process continues until the proposition in C is so refined and the propositions in K are so enriched that a proposition in C finally becomes true – it is no longer a concept because it has become knowledge (technically, it is called a conjunction).

This model of the design process in C-K terms has consequences for the analysis of radical innovation projects:

1. This model helps to track the *evolution of concepts*, i.e., the reformulations, refinements and changes in the product concept along the entire design process. C-K design 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 and chimeras – a space that is often considered irrational and chaotic. C-K design theory confirms that in C, 'truth logic' cannot 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 from the knowledge space are progressively added to that concept. Therefore, this logic helps us analyse the emergence of complex objects from a state in which they are partially unknown to a state in which they are considered known. Moreover, it is possible to understand how multiple alternatives emerge during a design process. *This property is helpful when studying the dynamics of alternatives and concept shifts in radical innovation projects*.
2. Furthermore, these conceptual dynamics can be related to the dynamics of knowledge. C-K design theory models how knowledge helps us produce new concepts and how new concepts lead to the production of new knowledge (e.g., how a chimera leads to the launch of a research program). This property is very helpful when studying one of the unique features of breakthrough innovation projects: the creation of new techniques and, generally speaking, new knowledge related to the emergence of new products.

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

The Design of New Architectures: Splitting Knowledge Bases

Recent advances in design theory allow us to take another step forward. They contribute to our analysis of the emergence of new

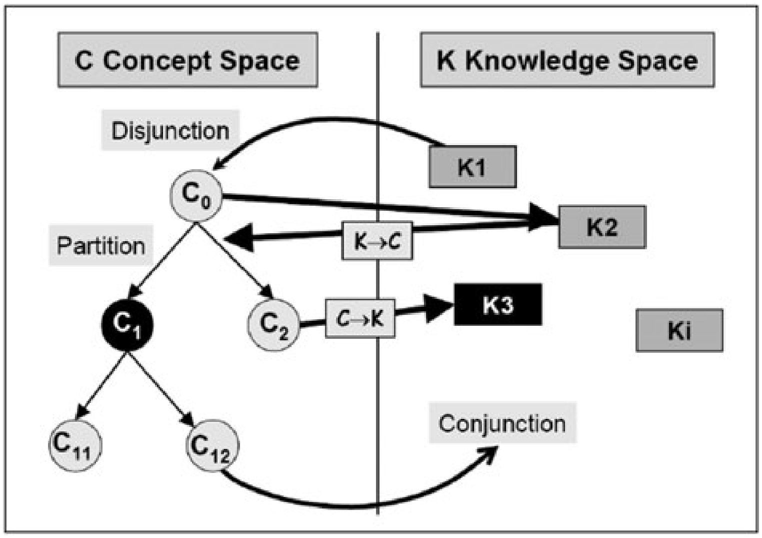

Figure 1. The Generic Pattern of Design Reasoning in the C-K Design Theory (Hatchuel & Weil, 2009).

architectures. To address the issue of architecture in design, the K-space in C-K design theory is modelled as follows: one distinguishes between K-pockets (e.g., skills, expertise and scientific disciplines) and the known relationships between these K-pockets, which we call combinative capabilities.

In this specific K-space framework, we can characterize a design process that relies on a given, fixed architecture; it is a design process in which the designers follow a given 'pattern' existing in K, i.e., specific, known and fixed combinative capabilities. These combinative capabilities consist of fixed interdependencies and independencies among the K-pockets. On the one hand, the 'core' of the platform determines a stable relationship, and some attributes thus determine others in a very constrained way (e.g., for a car, the size determines the range of engine sizes, and the engine determines the cooling system); there are rules that constrain the relationships between objects (engineering science rules, manufacturing rules, logistics rules, etc.). On the other hand, the architecture maintains 'degrees of freedom' and enables modularity: different wheel sizes are possible for one car model; different software works on the same computer, etc.

Therefore, in the case of non-architectural innovation, the combinative capabilities in the K-space are fixed and are characterized by two types of laws: *determinism* (clear dependence) and *modularity* (clear independence). In design theory, such K-spaces are said to be 'non-splitting'. Based on mathematical models, recent results in C-K design theory (Hatchuel, Weil & Le Masson, 2013; Le Masson, Hatchuel & Weil, 2015) show that a 'non-splitting' knowledge base (i.e., with these determinism and modularity properties) guarantees staying 'in the box'; the newly designed object will be obtained by relying on known combinative capabilities. In this process, knowledge creation is possible provided it does not change the combinative capabilities; more generally, knowledge creation is possible if the process follows determinism and modularity; if the knowledge base remains non-splitting; and if the newly created object is obtained through known combinations.

In contrast, a knowledge base can be 'splitting', i.e., it is non-deterministic and non-modular. Combinative capabilities thus follow two laws: at any step in the design process, there is never a situation in which the next step has already been determined by previous steps (i.e., combinative capabilities are non-determinist); in addition, there is never a situation in which it is possible to add the next step independent of all previous steps (i.e., combinative capabilities are non-modular). Therefore, a splitting knowledge base corresponds to 'out of the box' design solutions; the newly created objects are different from all of the combinations that can be made based on the existing knowledge base. Conversely, if a design is different from all of the objects known by their combinations, the knowledge base (at least locally) is splitting.

Consequently, there are two possibilities in the case of a breakthrough project:

1. the initial concept can be obtained with the known combinative capabilities – in this case, the knowledge base is non-splitting (deterministic and modular), and the breakthrough project will rely on a known architecture; or

2. the initial concept cannot be realized with the known combinative capabilities – in this case, the combinative capabilities must be changed or even created (in other words, it is an 'architectural' radical innovation).

A priori and in many cases (particularly in engineering design situations), the knowledge base tends to be 'non-splitting': engineering science laws, design rules and norms tend to imply deterministic rules and create modular relationships. In architectural innovation projects, the knowledge base must therefore be *made* to split, i.e., *some determinisms and some modularity must be redesigned*. *Therefore, design theory predicts the location of the critical issue in architectural innovation*: *the creation of a splitting knowledge base*. Where there is determinism, the designers must invent new alternatives; where there is modularity, the designers must establish mutual conditioning.

We thus posit our first hypothesis:

Hypothesis 1. In breakthrough projects (i.e., in projects with intense knowledge creation), one can distinguish 'non-architectural' innovation projects that create knowledge while maintaining a 'non-splitting' knowledge base (i.e., one with no new combinative capabilities) from architectural innovation projects that create knowledge that 'splits' the knowledge base (i.e., one with new combinative capabilities). This distinction helps to characterize two different design strategies.

Organizing for New Architectural Designs: Managing Systematic Proliferation

These two different design processes should correspond with two different management models. In the case of 'non-architectural' innovation, stable knowledge architecture appears as a firm foundation for organizing the design process as follows. First, deterministic laws determine the prescriptive relationship between functional teams (if the size of the engine determines the size of the brakes, then the engine designers work before – and prescribe the designs of – the brake designers). Second, modular laws enable the organization of concurrent (or simultaneous) engineering or the outsourcing of the design of modules to external suppliers. Consequently, 'non-architectural' innovation – with its non-splitting knowledge base – should correspond with classical PM. Moreover, one critical control issue emerges: PM should also regularly exercise control to ensure that knowledge creation – an essential feature of breakthrough innovation – does not 'split' the knowledge base.

In the case of 'architectural' innovation, PM consists of 'splitting' the knowledge base, which implies that the organization should never choose one single path but should instead support the creation of alternatives as soon as only one path exists (non-determinism). Furthermore, the organization should avoid silos and expert confinement to support fruitful hybridizations (non-modularity) and expansions. Finally, all of these organizational measures should be accomplished through hard work on concepts that create truly new, original and out-of-the-box concepts.

Accordingly, we arrive at our second hypothesis:

Hypothesis 2. With respect to organization, 'non-architectural' innovation is coherent with traditional PM (hierarchical planning, simultaneous engineering, etc.), whereas 'architectural' innovation calls for a new managerial model.

Methodology: Comparative Case Studies

We will now test our hypotheses by analysing two cases: the Polaris Project and the Manhattan Project. We chose these two cases for three reasons. First, they are both considered radical innovation projects. Second, one project is considered the PERT archetype, whereas the other has been shown to be a 'deviant' case. Third, a comparison is relevant because the cases exist in the same industrial universe (the defense industry), and they involve similar professional skills, similar teams and similar socio-professional levels. Moreover, the 'market uncertainty' dimension is irrelevant because the 'customer' desperately needs the product. Therefore, comparative logic enables us to control for many variables while maintaining our focus on the critical difference: the difference in the 'type of breakthrough'. In addition, the Manhattan Project and the Polaris Project have been extensively studied. Therefore, we can draw on a large amount of historical material that has not previously been used to study innovation management. Our objective is not to provide a comprehensive account of the cases or to summarize their unfolding (for the Manhattan Project, see Hewlett & Anderson, 1962; Rhodes, 1986; for the Polaris Project, see Sapolsky, 1972; Spinardi, 1994); instead, we seek to focus on the design situation that these projects confront. Nonetheless, we will include details that are critical to our argument. Given the information available, we believe that the point of 'theoretical saturation', which Glaser and Strauss (1967) have proposed as a criterion for halting data collection, has been attained.

Although our analysis may therefore lack empirical originality, we hope to triangulate the data in original ways.

Finally, based on the design theory framework, we have a clear data treatment procedure. First, we will draw the C-K trees of the two cases. Based on these diagrams, we will analyse whether the knowledge bases are splitting. Note that the C-structure is already symptomatic; it should be a 'depth-first' graph for non-architectural innovation and a 'breadth-first' graph for architectural innovation. The final step of the analysis will be to analyse the organizational patterns.

The Origins of the Rational Model of PM: The Polaris Project

The Polaris Project emerged in the US during the Eisenhower administration (1953–1961), a period in which the fear of a 'missile gap' with the USSR led to the launch of huge projects to develop the first thermonuclear intercontinental ballistic missiles (ICBM), first by the Air Force (the Atlas/Titan Project, 1954–59) and then by the Navy (the Polaris Project, 1956–60).[2] In this paper, we focus on the latter project.

Designing the Polaris Project

The US Navy launched the Polaris Project in 1956 to develop the first submarine-launched ballistic missiles (SLBM) carrying thermonuclear warheads. These offensive weapons, almost impossible to track and destroy, became a key element in nuclear deterrence. Despite its reputation for having introduced PERT, in reality, the Polaris Project was much more focused on strategic choices than on PM techniques. The Navy initiated the project to secure resources from the Pentagon, as the newly created US Air Force (USAF) was appropriating most of the vast resources available for nuclear and strategic defense. What is interesting for our purposes is that to obtain funds, the Polaris Project's specifications were carefully differentiated from those of the competing USAF systems. The Polaris Project emphasized the destruction of urban centres with limited accuracy – as opposed to the USAF's goals of destroying military targets, which required less power but more accuracy (Spinardi, 1994, p. 34).

The technical challenges were considerable because no one had ever designed a submarine-launched ballistic missile. To understand these challenges, we must first explore the technical aspects of missile design. The first important point is that at the time of the Polaris Project, the architecture of a ballistic missile was largely understood. From top to bottom, a ballistic missile is composed of the following elements: (1) a re-entry vehicle carrying the warhead; (2) a guidance system; and (3) propulsion and flight controls. Therefore, the Polaris Project's design created the following issues:

1 The Polaris Project's innovation related primarily to subsystems, the foremost of which was the W47 thermonuclear warhead, which was probably the Project's only radical innovation.[3]

2 There were considerable difficulties due to the complexities of system integration in the missile itself (given the size constraints imposed by submarines), of system integration between the missile and the submarines, and of system integration with the navigation/communication systems required to ensure the accurate positioning of the missile.[4]

Despite these challenges, the available knowledge base was solid enough to enable the project team to identify various technical solutions ex ante. Sapolsky is very clear on this question when he explains (1972, pp. 136–7) that '*if breakthrough means a substantial and unanticipated advance in the state-of-the-art*, *there were*, *it is true*, *no technological breakthroughs* (…) [*in*] *the FBM subsystems*. *In every subsystem*, *the trend of technology could be identified at the initiation of the program and remained essentially unchanged for its duration*. *In every subsystem*, *progress came through a multitude of small steps and not through dramatic leaps*.' He also confirms, '*The technical challenge and breakthrough in the FBM program was the early development of the system itself*. (…) *To build a system that involved interdependent progress in a dozen of technologies was*, *however*, *unprecedented*.*Such a system represents a substantial and historically unanticipated advance in the arts of planning and program management* (on this see also Sapolsky, 2003).' Therefore, if we apply the C-K framework to the Polaris Project, we obtain the project design shown in Figure 2, which emphasizes that Polaris's design strategy was to differentiate itself from the USAF's ICBM.

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

1. the Polaris Project built on previous projects;
2. the Project's conceptual evolution was important (from a silo-based ICBM targeting military forces to a submarine-launched deterrence weapon targeting cities); and
3. the knowledge base was very rich at the beginning: the architecture was given; several solutions were identified for

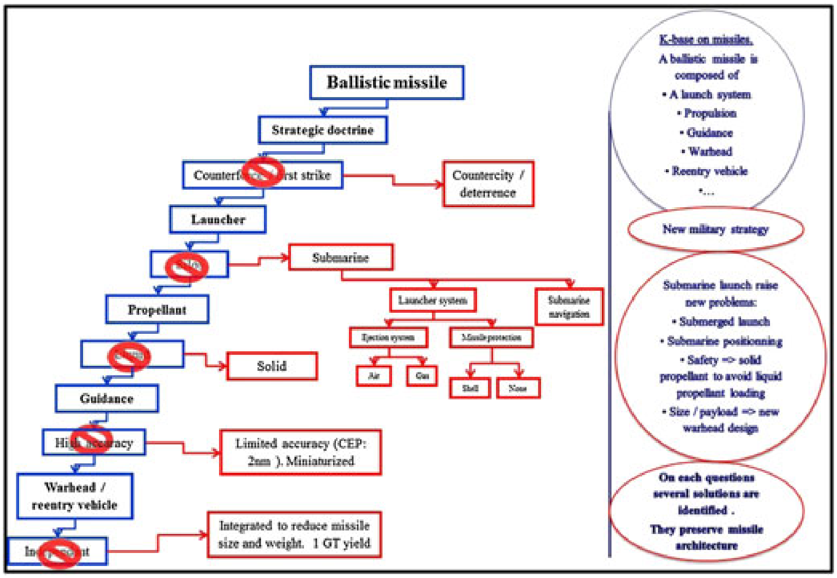

Figure 2. Polaris Design as Differentiation from USAF ICBM

eachcomponent; and competences were available within the Navy through contractors such as Lockheed and universities.

Therefore, the residual uncertainties were not overwhelming. Let us underline the two critical reasons for this relatively low level of uncertainty. On the one hand, there was substantial reuse of existing components and solutions; on the other hand, the project benefitted from the system's modularity because some components could be changed without having a substantial impact on the other components. For instance, the main uncertainties related to the warhead design, which was largely independent of the rest of the missile, the underwater launch system and the solid propellant propulsion.

If we turn to our framework, it appears that the available competences ultimately built a knowledge base that, with respect to the initial concept, was actually modular and deterministic. The knowledge base thus did not follow the splitting condition. Using the theorem mentioned above, we can conclude that the Polaris Project was actually a combinatorial project. Naturally, it tested a combination that had not been tried before, but, as stated by Sapolsky, the combination was ultimately (quasi-)predictable based on the available knowledge.

Managerial Implications

This representation of the design problem helps us understand the end of Sapolsky's comment, as quoted above: '*Such a system represents a substantial and historically unanticipated advance in the arts of planning and program management*.' Indeed, because the design process was foreseeable (despite the inevitable surprises), the primary challenge was controlling the design of an incredibly complex system given cost/time/quality constraints, which led the Polaris Project to rely on two managerial innovations.

The first and unquestionably most important innovation was the creation of a dedicated organization, the Special Projects Office (SPO; see Sapolsky, 1972). This organization allowed the project to overcome the usual bureaucratic struggles among various departments of the Navy. Furthermore, the organization of the SPO mirrored the missile's architecture. It was organized via subsystems (SP 22: launcher; SP 23: guidance and fire control; SP 24: navigation; etc.) and combined the following features:

1. a very tightly centralized system integration – the SPO defined the goals, architectures, and interfaces and controlled the budget; and
2. substantial delegation of the work on subsystems. Contractors were given a very high degree of autonomy *within the SPO guidelines*. There were always several contractors competing in the design process, which maintained pressure and ensured the existence of back-up solutions (see Table 1, p. 151 in chapter 5 of Sapolsky).

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

The second managerial innovation, which is the most famous if not the most efficient, was the PERT approach to project planning. In popular accounts, the success of the Polaris Project is associated with the development of the PERT method, which became almost synonymous with PM following project completion. Sapolsky has demonstrated that this association was a myth (chapter 4); however, a discussion of this issue falls outside the scope of this paper. We are interested in uncovering what the PERT principles reveal about the management of the Polaris Project. We thus refer to the 1959 paper by Malcolm et al. (who were working for the SPO), which marks PERT's first appearance in the literature. Their starting point is clear. 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 complete, and the challenge was monitoring work progression in the context of a 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*.' Their hypothesis reveals the project's huge K-base:

- an ordered sequence of events to be achieved constituted a valid model of the program;
- activities could be determined and were conditioned by identifiable product performance requirements and resource applications; and
- resources were known, and expected technical performance is specified.

Consequently, '*an approach dealing only with the time variable was selected*'. Indeed, to the extent that the system and its components were already specified, the main uncertainty was task duration. The problem was thus one of making decisions despite uncertainty, a question that could be addressed using the operation research methods that were in favour during the 1960s at institutions such as the RAND Corporation (see Marschak, Glennan & Summers, 1967; or for a historical approach, Hounshell, 2000; Erickson et al., 2013). However, we now know the necessary conditions that rely on this method: the existing K-base and its structure allow an (almost) complete definition of the system from the beginning.[5]

The Manhattan Project Case and the Management of Innovative Design Situations

We can now turn to another landmark in the field of PM: the Manhattan Project. Recent research demonstrates that claims that the Manhattan Project is the foundation of modern PM are false (Lenfle, 2008; Lenfle & Loch, 2010). The Manhattan Project instead exemplifies the case of a project confronted by radical innovation and its associated unforeseeable uncertainties. The interesting question is thus to analyse how the project succeeded in designing such an innovation so quickly. We will not describe the unfolding of the project here (for an overview, see Gosling, 1999; Lenfle, 2008). Instead, we will focus on the project's design strategy.

Designing the Bomb

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

The Problem: The Production of Fissionable Materials and the 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 (U^{238}) but represents only 0.72 per cent of its mass. The other, plutonium (Pu^{239}), is a by-product of nuclear fission discovered by Glenn T. Seaborg in 1941. In both cases, the production of fissionable materials raised considerable scientific and technical problems:

- Because of the slight differences in the atomic mass of U^{235} and U^{238} (less than 1 per cent), separating the two isotopes involves extremely complex processes. Seven different separation methods were identified in 1941; as we shall see, three of them were ultimately used (Smyth, 1945).
- Plutonium production involves the design and construction of nuclear reactors, along with the associated chemical separation plants. Twelve separation processes were studied at the University of Chicago Metallurgical Laboratory ('Met Lab') at the beginning of plant construction.

These processes were breakthrough innovations and neither existed before the Project (plutonium production) or had ever been used with radioactive materials (chemical separation). They entailed extremely tight requirements and involved radioactive (and therefore extremely 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 (1945) observed, '*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*'. In C-K terms, they were confronted with a (highly) generative design space. The more they progressed, the more likely they were to face new problems and to create new solutions.

The team faced a similar situation with respect to the design of the atomic bomb. In a seminar that Oppenheimer organized at Berkeley in July 1942, scientists discussed bomb designs. Several fission bomb assembly possibilities were envisioned: the gun method, the implosion method, and the autocatalytic method, among 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 to these designs was not simple. Furthermore, the Berkeley discussion was theoretical because no prototypes had been built, and no experiments had been performed. It remained to be seen, for example, whether a gun design would work for both uranium and plutonium and whether an implosion device would even be feasible.

Managerial Implications

This situation had fundamental managerial implications, the most important of which was that the entire project was characterized by unforeseeable uncertainties. 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 5 October 1942, soon after his nomination as project director, Groves '*asked the question that is always 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*' (Groves, 1962, p. 40). He thus concluded, '*While I had known that we were proceeding in the dark, this conversation brought it home to me with the impact of a pile driver. There was simply no ready solution to the problem we faced*, *except to hope that the factor of error would prove to be not quite so fantastic*' (Groves, 1962, p. 40).

Therefore, it is clear that the Manhattan Project encountered a completely different design situation compared with that of the Polaris Project. The K-base was largely non-existent; there was no existing industrial base; and, therefore, nobody could predict how the project would unfold. Thus, if we rely on a traditional engineering framework, we can say that, for the technologies in Polaris, the TRLs were higher, and the trajectory of those TRLs much clearer. The challenge, as Sapolsky said, was in managing the simultaneous development of a number of technologies. Whereas for Manhattan, the elements available to the engineers were at lower TRLs, it was not clear in which direction they would develop, and fundamental scientific work had to be done to understand the phenomena well enough to make engineering judgements.[6] This explains why Groves quickly realized the impossibility of any reliable planning (see Groves, 1962, p. 15). One could even question its manageability. In this type of situation, the design strategy plays a central role because the challenge is not to control a complex but predictable design process (as in the Polaris Project) but instead to manage the unknown.

Design Strategy

We can roughly summarize the design problem as follows: given the available K-base, nobody knew what was feasible in terms of fissionable material [mt] and ignition mechanism [im]. Several solutions were identified (see Serber, 1992), but it was impossible to anticipate which one would work. Moreover, there were probably incompatibilities in the K-space, i.e., not all of the [mt; im] combinations would work. Therefore, in contrast to the Polaris Project, there were strong interdependencies present. Consequently, the choice of one alternative may have led to a necessary redesign of the remainder of the project. We recognize the two features of the splitting condition theorem: with respect to the initial concept, the knowledge base was non-deterministic and non-modular.[7]

In such a situation, it is necessary to think 'outside the box', beyond a pure combination of available components, and to fulfil all of the 'constraints' or requirements of the initial concepts. This necessity implies a strong knowledge-creation effort for each of the constraints. Because no modularity can be expected, it is necessary to explore a large set of alternatives, which thus sheds light on the design approach of Groves and the steering committee. Indeed, as shown in Figure 3, they would make two fundamental design decisions:

1. The separation of material production and bomb design. The idea was, on the one hand, to explore different ignition mechanisms working '*in one or more of the materials known to show nuclear fission*' (Serber, 1992, p. 3) and, on the other hand, to produce fissionable materials that would be as pure as possible. The goal was to avoid exploring *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 simultaneously explore and implement different solutions for the production of fissionable materials and for bomb design (for a detailed analysis of the parallel approach in the Manhattan Project, see Lenfle, 2011).

The fundamental goal of this strategy was to build a large K-base that would enable different weapon designs via what would eventually be discovered. Figure 3 summarizes the possible solutions envisioned by the project team. In the remainder of the paper, we will use this strategy to describe the evolution of the process of designing the atomic bomb, which will help us understand how the strategy explains the final success of a project that otherwise could have been a complete failure.

Given the available knowledge in September 1942, the participants' first strategy (shown in Figure 4, the preferred choices appear in grey, and back-up choices appear in darker dotted lines) was the following:

1 To favour fission over fusion which, although clearly envisioned, was too uncertain to be of any utility during the war.
2 To focus on electromagnetic separation (codenamed Y12), with gaseous diffusion (K25) as a back-up, when producing fissionable material.
3 To favour the seemingly more robust gun method for bomb design and to use that method with plutonium, which at the time was less well known. It was supposed that if the gun design worked with plutonium, it would also work with uranium. However, given the unknowns, a small team studied implosion as a back-up.
4 For DuPont to choose a simpler-to-design, water-cooled reactor for plutonium production.

However, the unforeseeable uncertainties soon manifested and, in the spring of 1944, the project leaders, primarily Groves and Oppenheimer, realized that the project had manoeuvred itself into a dead end (see Figure 5):

1 None of the uranium enrichment methods had 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 (for a synthesis, see Lenfle, 2011).

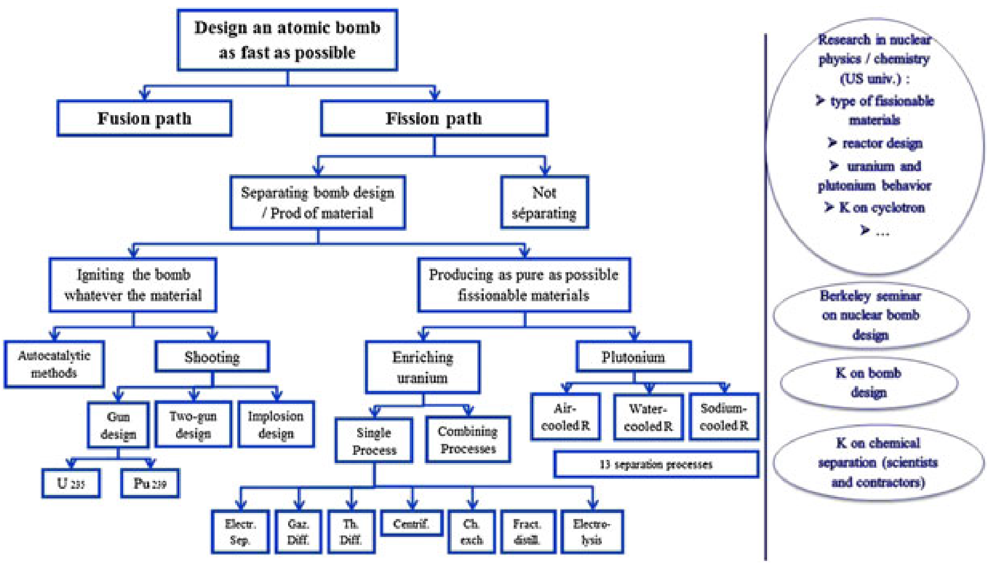

Figure 3. A Complex and Generative Design Space (end of 1942)

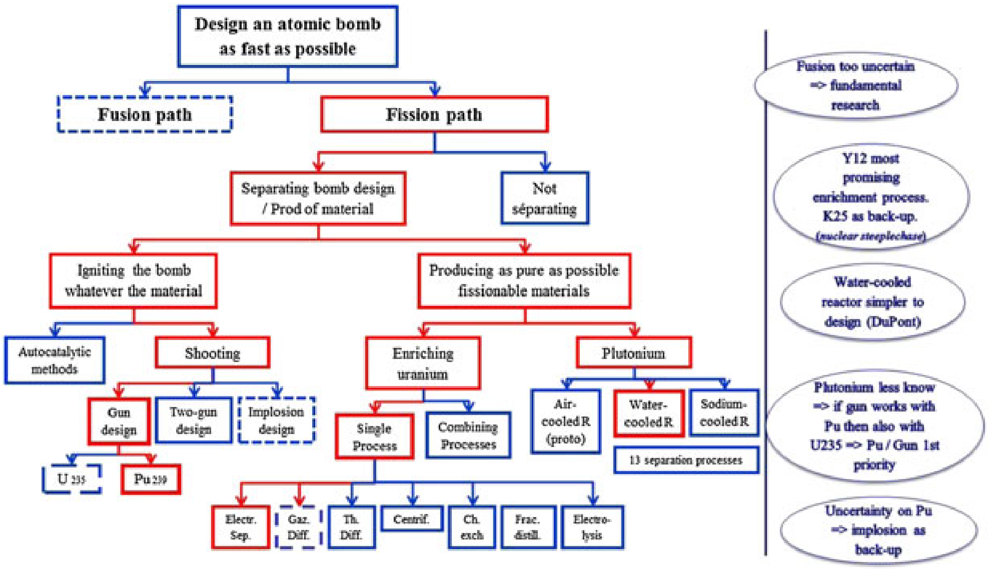

Figure 4. First Design Strategy Space (Sept. 1942–Spring 1944)

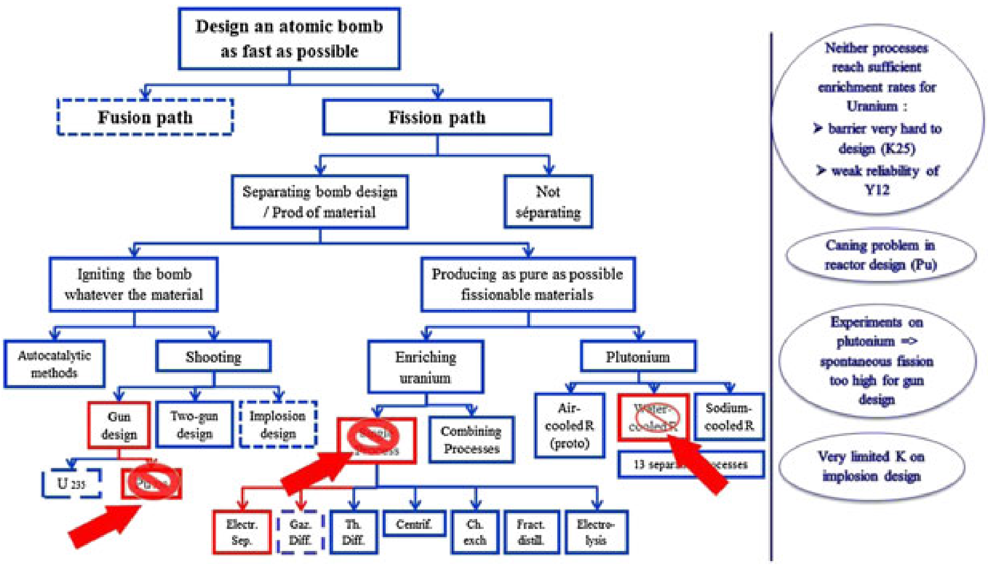

Figure 5. The Spring of 1944 Crisis

2 The production of plutonium looked more promising, but ‘canning’ the uranium slots to protect them from water created huge problems.
3 Even worse, the gun design proved to be unsuitable for plutonium (this episode, known as the ‘spontaneous fission crisis’, is described in detail in Hoddeson et al., 1993, chapter 12 et seq.).

Therefore, at that point, they had a fissionable material (plutonium) without a bomb design and a bomb design (the gun) without a workable fissionable material (uranium 235). However, the chosen design strategy revealed its relevance at this pivotal moment. The building of a large K-base and the decision to simultaneously explore different solutions allowed the team to do the following (Figure 6):

1 To switch from the plutonium gun to the implosion design as the first priority (although the gun design continued for uranium 235), even if many people doubted the feasibility of implosion.
2 To add a new separation process for uranium enrichment and to combine the

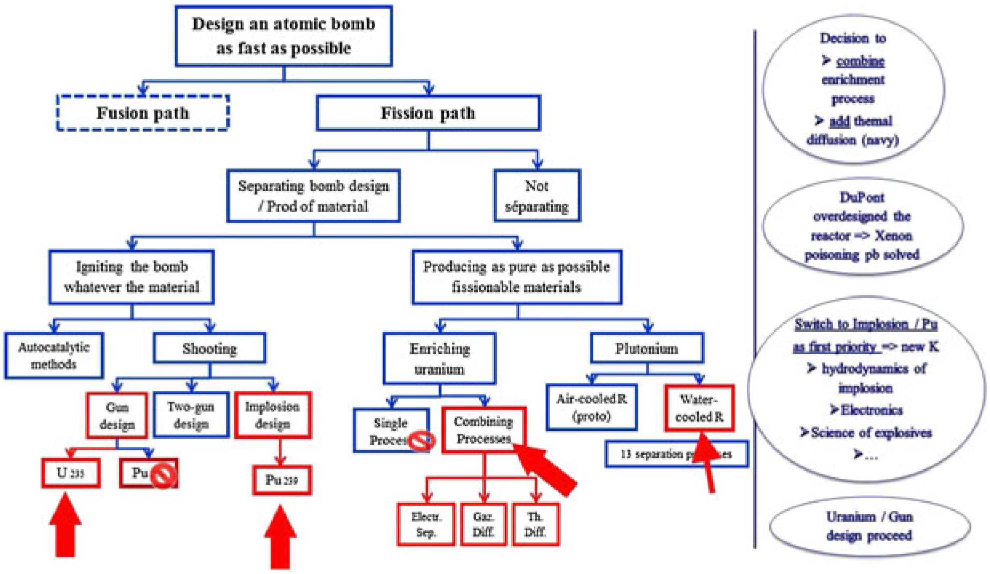

Figure 6. Escapes (Summer 1944–August 1945)

different processes to reach the desired level of enrichment; in addition, to combine the uranium-enrichment processes (on this decision, see Lenfle, 2011).

3 To adapt a strategy of intense experimentation related to the 'canning' problem in plutonium production.

In terms of design theory, we observe a fascinating phenomenon. The initial knowledge base meets the splitting conditions, and it has been so enriched during the exploration process that it incrementally becomes non-splitting; modules and deterministic rules have been created. At this stage of the process, moreover, it is possible to combine pieces and components to arrive at a new 'modular' solution. Once the knowledge base appears (most likely) modular, it is possible to return to a combinatorial process, which results in the surprising speed of the final design phase.

This flexibility, allowed by the design strategy, explains the final 'success' of the Manhattan Project, which ultimately proceeded at incredible speed. The implosion design was settled on very late, probably on 28 February 1945. Oppenheimer then created the 'cowpuncher committee' to oversee the final phase (see Hoddeson et al., chapters 15 and 16). However, the remaining uncertainties around the new device were so great that Groves – finally, but reluctantly, and despite the considerable cost – approved Oppenheimer's request to test the bomb. The Trinity Test marked the dawn of the nuclear age. On 16 July 1945, the Manhattan Project tested the implosion bomb in a remote area in the deserts of New Mexico. 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 and Nagasaki followed three weeks later.

Results

By analysing radical innovation projects through the lenses of knowledge creation, one tends to confuse two different types of projects: those that maintain their product architectures and those that change them. The former case is said to be manageable through the use of PERT and classical PM techniques, but we have a very limited understanding of the latter case. What is its design logic? What are its organizational principles? Even more: if we do not clearly understand the difference, are we sure that we clearly understand the management principles of non-architectural breakthrough projects? In this paper, we uncover one hidden contingent variable in the management of breakthrough projects: in both cases, there is substantial knowledge production, but in the case of non-architectural breakthrough projects (the Polaris Project), there is *no* knowledge production related to combinative capabilities; and in the case of architectural breakthrough projects, knowledge production changes in combinative capabilities (Manhattan Project). This finding corresponds to our first hypothesis. Moreover, we can now identify the consequences of these changes in terms of both design strategy and PM.

R1: Design Strategy

Architectural innovation is required when a project's initial concept (its 'brief') cannot be obtained by relying on known combinative capabilities; in this case, combinative capabilities must be increased to become 'splitting', i.e., combinative capabilities should be both non-deterministic and non-modular. The case studies highlight several properties related to this result:

- For radical, but non-architectural, innovations, there is knowledge creation under the constraint of keeping combinative capabilities unchanged, as was the case with the Polaris Project. The architecture is given at the beginning, and innovations occur on subsystems and their integration. However, this innovation process does not imply the creation of new combinations. The existing combinative capabilities remain relevant.
- In contrast, designing architectural innovations requires the creation of a splitting knowledge base, i.e., the generation of both non-determinism and non-modularity. In practice, this condition means that the project must (1) create new alternatives when only one path has been considered, and (2) explore mutual interactions in which independence and modularity have been considered. In this situation, many new architectures are possible. Therefore, we are confronted by the paradox that a project attempting to design a radical innovation is not driven by a single architecture but instead aims to create a splitting knowledge base that will generate *multiple* architectures. This radical innovation occurred in the Manhattan Project case, in which different alternatives were systematically generated efore the more interesting one was chosen. Even more: in the end the two bombs were based on two different architectures.

R2: Project Management

This research also helps to clarify how to (differently) manage architectural and non-architectural projects, i.e., how to manage projects in a way that maintains their combinative capabilities or causes their combinative capabilities to split. This clarification relates to our second hypothesis. The two cases demonstrate the following:

- In the case of non-architectural innovation, the existing combinative capabilities allow for work packages to be defined, planning to be organized, etc., which reflects the classical PM model. Knowledge creation can occur *as long as* it maintains its combinative capabilities, which implies a critical PM task: control over maintaining combinative capabilities. Here, we highlight a largely underestimated or ignored (or even misunderstood) role of PM management: control over the stability of architectures. Therefore, silos and limited interactions between work packages are not an unwanted consequence of PM; instead, they are actually a critical condition for its success. PM is organized to avoid (and contain) propagations and interactions. As we have observed, the Polaris Project is typical of this type of situation (see, e.g., the efforts to keep the SPO guidelines).
- Conversely, when there is a change in the architecture, the team confronts a complex process of the systematic generation of alternatives that require a strong, co-ordinated effort. Indeed, the challenge is being able to generate these alternatives, to constantly assess their relevance according to the evolution of the situation, and to adapt PM to do so. This distinct challenge explains a fundamental difference between the Polaris Project and the Manhattan Project. Whereas the main challenge of the Polaris Project was monitoring the evolution of a complex (but clearly structured) development process, the Manhattan Project confronted major unknown unknowns that led to several complete reorganizations of the project. To adopt the expression of Thorpe and Shapin (2000), Los Alamos – the crux of the Manhattan Project – was in '*a continual state of flux and turbulence*' (p. 557). Therefore, as stated by Hoddeson et al. (1993, p. 247), the structure of the laboratory '*was by nature ephemeral; experiments and responsibilities changed overnight as priorities that the war gave to the project changed*'. The spontaneous fission crisis and the development of the implosion bomb are examples of this type of paroxysmal event. The contrast with the Polaris Project is striking in that the structure of the SPO, which mirrored the Polaris Project missile components, remained unchanged throughout the project. Again, this unchanged structure shows that the profusion of alternatives, trials and prototypes is not an unwanted consequence of breakthrough PM; instead, it is an intentional and organized process that tends to maximize the exploration of different paths.

Discussion and Further Research

In this paper, we have attempted to bridge the literature on PM and recent advances in design theory. What can we learn from this first attempt and, in particular, from the comparison of the two cases?

First, this attempt demonstrates the power of design theory in overcoming the limitations of traditional typologies of innovation. Indeed, both the Polaris Project and the Manhattan Project are traditionally presented as examples of radical innovations. However, our analysis demonstrates that the problem is more complex. Both projects were radical innovations, of course, but we show that the Polaris Project benefited from a large, non-splitting K-base and could rely on an industrial network of contractors already active in the field of missile design. Therefore, as noted by Sapolsky, '*In every subsystem, the trend of technology could be identified at the initiation of the program and remained essentially unchanged for its duration. In every subsystem, progress came through a multitude of small steps and not through dramatic leaps*.' The Polaris Project was risky, but there were few unforeseeable uncertainties. The knowledge base was essentially structured in a non-splitting way, it was fundamentally modular and deterministic. Conversely, the Manhattan Project was plagued by unknown unknowns and had no industrial base on which to rely. More precisely, the analysis with C-K theory reveals that in the Manhattan Project, the initial knowledge actually corresponded to the splitting condition; any new attribute had critical consequences, and there was never one single, self-evident alternative. As predicted by the splitting condition theorem, the Polaris Project's design strategy was quite straightforward, whereas the Manhattan Project had to adopt a much more original approach to manage the unknown and to learn. As Groves said, '*the whole endeavor was founded on possibilities rather than probabilities. Of theory, there was a great deal, of proven knowledge, not much*' (1962, p. 19). We thus show how design theory is more precise than the traditional typology of innovations in understanding what happens in projects. Indeed, understanding the design of architectural innovation can help organize such design processes. Our paper helps us clarify some evaluation criteria (i.e., whether the final knowledge base is splitting). It also enables us to characterize the type of 'design spaces' that might be required by designers; 'non-architectural' innovations would favour validation tools, whereas architectural innovation would favour explorations that go beyond any local determinisms – techniques such as user involvement, new extended digital mock-ups, and use scenarios might help us renew the discussion about the existing dominant design.

We thus explicitly link the design situation and strategy to PM, which is our second contribution. This link contributes to the ongoing effort to excavate the roots of PM techniques (Lenfle & Loch, 2010; Söderlund & 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 the team (1) to define the problem and (2) to identify the different solutions in advance. This rational approach is largely reflected in the case of the Polaris Project. Conversely, an innovative design in which unknowns exist in the K-space and/or the C-space, traditional PM techniques become completely irrelevant. This irrelevance cannot be more clearly stated than by General Groves' insistence on deciding '*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). Our analysis of the Manhattan Project with C-K design theory demonstrates the need for new managerial approaches based on the construction of a large K-base to *design* the necessary flexibility. Moreover, we discover one key feature of the success of the Manhattan Project: not only did the team learn, but also the knowledge created actually led *to the creation of a knowledge base that – this time – was non-splitting*. We better understand Groves' very smart strategy of exploring all of the extreme combinations of alternatives, in the hope of creating new pieces of knowledge that could be considered as modules or deterministic rules. Ultimately, this finding leads us to a new understanding of parallel strategies in projects with unforeseeable uncertainties. Until now, such strategies have been justified in terms of the increased probability of finding the 'best' solutions, given that several solutions are tried simultaneously (Abernathy & Clark, 1985; Loch, DeMeyer & Pich, 2006). However, the various trials are presented as independent of one another. Therefore, the rationale for this approach, beyond its use for random trials, remains unclear. In using design theory and introducing the structure of the K-base in the analysis, our research sheds new light on this question. Indeed, the Manhattan Project case shows the rapid creation of a rich, non-splitting knowledge base that would ultimately allow the team to succeed despite the existence of many unknowns.

This study is, of course, exploratory. It was limited to two historical cases, and further studies will be needed to consolidate its results. Much work remains to be done, but we believe that this dialogue between PM and design theory constitutes an important avenue for future research on the management of exploratory projects. Indeed, it may help develop new strategies of PM that will account for advances in design theory. In particular, we think about the notions of expansion (Hatchuel & Weil, 2009) and expandable rationality (Hatchuel, 2002), which, in our view, reopen a field that has thought of projects as convergence processes for too long. This task has already started. Lenfle (2012) and Gillier, Hooge and Piat (2014), for example, have studied how C-K design theory could lead us to rethink the evaluation of projects that produce much more than they deliver. Design theory helps formalize the 'much more' in terms of C and K. Therefore, we generally believe that design theory offers a new way of representing/discussing/managing the exploration process.

NOTES

1. We thank an anonymous reviewer for suggesting the following references.
2. For a general presentation of the challenges of ICBM designs, see Johnson (2002).
3. To reduce the warhead's size and the weight, engineers and scientists decided to integrate the re-entry vehicle and the warhead, which became a single unit. This integration required close co-operation between the Laboratory and the Navy, establishing a new way of doing business for both.
4. One must remember that the first satellite-based localization system, Transit, was designed for the Polaris Project.
5. This is obvious in a Navy study of 1956–57, which almost gives the final characteristics of the Polaris Project (see *The China Laker*, 9, 2003).
6. We thank an anonymous reviewer for this remark.
7. Note that some design theories are very close to an extended combinatorics, such as general design theory (GDT) and axiomatic design (AD). These theories are sufficient to describe projects that do not meet the splitting condition. The Polaris Project might have been described using either GDT or AD. As soon as a project knowledge base fulfils the splitting condition, it will be necessary to rely on design theories that are more generative, such as the coupled design process (CDP), infused design (ID) or C-K design theory. This theoretical division confirms that we were right to choose C-K design theory to compare and characterize the Polaris Project and the Manhattan Project.

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