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International Journal of Project Management 32 (2014) 921 – 931

International Journal of Project Management

www.elsevier.com/locate/ijproman

Toward a genealogy of project management: Sidewinder and the management of exploratory projects

Sylvain Lenfle [1]

University of Cergy-Pontoise, THEMA & Ecole Polytechnique, Management Research Center, France

Received 16 April 2013; received in revised form 28 October 2013; accepted 31 October 2013
Available online 31 January 2014

Abstract

This paper deals with the management of exploratory projects, i.e. projects where neither the goals nor the means to attain them can be defined at the beginning. It relies on the historical case study of the Sidewinder Air-to-Air missile, designed by the US Navy between 1947 and 1957. The case is interesting because it violated all the best practices of PM, yet involved a short and cheap development process that resulted in a best-seller in missile history. This case thus helps to analyze the inner working of an understudied skunkworks (project-level) and to discuss the governance of exploratory projects (firm-level), more specifically the limits of Stage-Gate processes for radical innovations.

Keywords: Exploratory projects; Genealogy; Sidewinder; Experimentation; Skunkworks; Stage-Gate

> "*I think that a lot of the most interesting and novel solutions come when you don't have a definite specification*".

Dr William McLean, Sidewinder project director, Hearings before the Committee on Armed Services, US Senate, December 1971, p. 233.

1. Introduction. History and the relevance of project management research

There is a growing concern in the project management (PM) research community about the relevance of the existing body of knowledge. Hällgren et al. (2012) thus argue that "*the* [relevance] *problem occurs when simplified, rationalistic and deterministic models (or ontologies) are mistakenly considered to be accurate views of reality. (…) It could be argued, therefore, that PM research is not only an immature field of research, it is also unsubstantial in terms of understanding what is going on in projects*" (p. 462). Such comments reflect a larger research stream which, in various disciplines (accounting, strategy, etc.), emphasizes the need to study the actor's practices in detail in order to build relevant management theories. In the PM field, for example, Cicmil et al. (2006) plead for research on the "actuality" of projects, arguing for a bottom-up, grounded approach to PM theory building. This has led to new understandings of PM (Cicmil et al., 2006; Winter et al., 2006).

The present paper is in line with such renewal of PM research. It will focus on the management of exploratory projects, i.e. projects where neither the goals nor the means to attain them can be defined at the beginning. Recent research demonstrates that exploratory projects are strategic in today's innovation-based competition (Brady and Davies, 2004; Lenfle, 2008a; Loch et al., 2006). The landmark contribution of C. Loch et al. (2006) underlines the need to invent new ways to manage exploratory projects, and demonstrates the irrelevance of traditional risk management techniques in projects confronted to what they called "unforeseeable uncertainties." They thus proposed "learning" and "selectionism" (i.e. the simultaneous pursuit of different solutions) as two generic managerial strategies for exploratory projects, and discussed their managerial implications. However, we still lack a practice perspective that could further our understanding of the organization and management of such projects. Indeed, the PM literature mainly emphasizes the need to set up a dedicated and autonomous project team to manage

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E-mail address: slenfle@hotmail.com.
URL: http://crg.polytechnique.fr/home/lenfle/FR.

[1] Tel.: +33 6 84 98 49 18.

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http://dx.doi.org/10.1016/j.ijproman.2013.10.017

radical innovation, the famous Skunkworks© invented by Lockheed during World War II. But the literature on Skunkworks is very sparse, to say the least (Rich and Janos, 1994), and more information is needed on their inner working and governance.

In this paper we propose to go back to history to better understand the organization and management of exploratory projects. We believe that historical analysis is a powerful tool to complement project management research. Until now, it has not been used to learn about practices. We therefore disagree with Hällgren et al. when they affirm that "*the general story of the rise of PM as a management methodology is well known. The use of structured PM (planning and scheduling) approaches was heavily supported within major US defence projects such as the Manhattan Project and the development of Polaris missile system, as well as other mega projects during the Cold War era, such as the US space program*" (p. 462). Recent research on the history of project management demonstrates that such a statement is inaccurate, particularly for the Manhattan Case (Lenfle and Loch, 2010). Thus we believe that the lack of history of project management is part of the relevance problem.

Our paper is organized as follows: Section 2 discusses the role that history could play in PM research by relying on the work of the French philosopher Michel Foucault, and considers the data we used. Section 3 presents the Sidewinder case that is analyzed in Section 4. Section 5 concludes by discussing questions for further research.

2. History and project management: Foucault's genealogy

The lack of a history of project management should come as no surprise. Indeed, most of management research and teaching is ahistorical (Chandler et al., 1984; Cummings and Bridgman, 2011; Kieser, 1994). The same can be said of the field of project management (Soderlünd and Lenfle, 2013), with the exception of P. Morris's *The management of project*. The most famous case studies, for example Sapolsky (1972) on the Polaris project, proceed from other disciplines, such as business history or political science. This situation raises two concerns: that existing history is oriented mainly to the United States, and that there is weak understanding of the roots and evolution of project management.

Therefore we believe that A. Chandler should not remain an exception. As Kieser (1994, p. 619) pointed, "*historical analyses can serve to reflect on existing organizational designs and to criticize existing organizations theories. Historical analyses do not replace existing organization theory; they enrich our understanding of present-day organizations by reconstructing the human acts which created them in the course of history and by urging organization theories to stand the test of a confrontation with historical developments*". This should also be true for project management research (Morris, 1997; Soderlünd and Lenfle, 2013). Like Cummings and Bridgman (2011), we are convinced that doing the history of management is critical for improving both theory and practices, and making management a more "reflective" discipline (Schön, 1983).

However, which type of historical method is most appropriate? It is not enough to claim that we need a history of project management. We must avoid two classical pitfalls in historical analysis: presentism and finalism. In presentism, "*the historian takes a model, or a concept, an institution, a feeling, or a symbol from his present, and attempts – almost by definition unwittingly – to find that it had a parallel meaning in the past (…) for example if we attempted to interpret Medieval Christianity or a primitive rite entirely in terms of individual psychology, neglecting the hierarchical and cosmological reality, we would be writing the history of the past in terms of the present*" (Dreyfus and Rabinow, 1983, p. 118). The risk here would consist of looking for traces of the present (e.g. PM best practices) in past projects.

In the perspective of finalism, one tries to find the foundations of the present in some distant times, and analyze history as a teleological process that necessarily leads from that point to the present. Here "*everything that happened in between is taken up by this march forward, or else left in the backwash as the world historical spirit differentiates and individuates what is central from what is peripheral. Everything has a meaning, a place; everything is situated by the final goal history will attain*" (Dreyfus and Rabinow, 1983, p. 118). In such a determinist perspective, which was famously criticized by K. Popper in his classic *The poverty of historicism* (1957), the history of project management would seem to converge toward the current body of knowledge.

Michel Foucault's approach to history could help avoid the pitfalls of presentism and finalism. Building on Nietzsche's concept of genealogy, Foucault explained how concepts, theories and practices that are now considered evident are, in fact, socially and historically situated and constructed. He insisted on making explicit the conditions of the emergence of objects, knowledge and concepts, as well as their insertion in society. Thus, "*the task of the genealogist is to destroy the primacy of origins, of unchanging truth*" (Dreyfus and Rabinow, 1983, p. 108–109). By carefully analyzing discourses, institutions, tools, and socio-economic contexts, Foucault brought to light the production of knowledge and its associated "*technologies of power*" (Foucault, 1975), and described "*how a field's foundations are actually formed in a piecemeal fashion but then solidify to produce a sense of the development of knowledge while at the same time marginalizing other possibilities*" (Cummings and Bridgman, 2011, p. 81). As explained by Gutting (2013), "*The point of a genealogical analysis is to show that a given system of thought (…) was the result of contingent turns of history, not the outcome of rationally inevitable trends*". He thus elaborated a "*counter memory*" (Foucault, 1971) aimed at reviving forgotten knowledge and reinterpreting shared concepts. Foucault's landmark contributions on the birth of prisons (Foucault, 1975) illustrate the fruitfulness of the genealogical approach (see Gutting, 2013 for a synthesis). In this book Foucault analyzes the transition from old (torture and execution) to modern, gentler, ways of punishing criminals. He emphasizes that this evolution leads to more effective modes of control that, progressively, becomes the model in different settings like factories, schools or hospitals. As Gutting (2013) explained "*At the core of Foucault's picture of modern "disciplinary" society are three primary techniques of control: hierarchical observation, normalizing judgment, and the examination*". However he warns that "*we should not think that the deployment of this model was due to the explicit decisions of* *some central controlling agency. In typically genealogical fashion, Foucault's analysis shows how techniques and institutions, developed for different and often quite innocuous purposes, converged to create the modern system of disciplinary power*" (see Gutting, 2013 for an overview of this book and Foucault's work).

Following the pioneering work of Hatchuel et al. (2005), we believe that project management research could greatly benefit from Foucault's genealogical approach. Genealogy in the Foucauldian sense can help us to examine critically existing PM theory and to uncover project managers' actual practices. This may constitute an important step in building a relevant PM theory (Blomquist et al., 2010; Hällgren et al., 2012). Two different uses of genealogy may prove particularly fruitful:

1. The first one could focus on a genealogy of the rational model represented, for example, by the US Project Management Institute. The goal would be to analyze how, when and why that body of knowledge emerged and became dominant, and who the actors, knowledge and institutions behind it were. The work of Morris (1997), Johnson (1997, 2002a & 2002b) and, more recently, Lenfle and Loch (2010) are the first steps in that direction. They show that PMI resulted from a long process that began after World War II in the US military–industrial complex. S. Johnson's remarkable research describes how the development of large weapons systems, such as the ballistic missile, led to the development of technical and management tools for dealing with the complexity of those systems. Rooted in those military projects, the so-called modern project management led in the 1960s to the development of a body of knowledge mainly composed of PM tools like PERT, earned value, and so forth. This reflects a faith in rational decision making (see Erickson et al., 2013) and the will of the government (first and foremost R. Mc Namara's) to control military spending. Further research is needed to understand the micro-mechanisms that progressively led to the formalization of the model, to analyze its impact on PM practices, and to document its link with decision theory and organizations like the RAND corporation (see Hughes and Hughes, 2000 for an introduction).
2. A second use of genealogy brings us to forgotten paths: the practices, models and organization that became marginalized or lost in this process. In such a perspective, the purpose is to elaborate a counter-memory, an alternative to the dominant (albeit scant) discourses on the history of PM. In this vein Lenfle (2008b) and Lenfle and Loch (2010) demonstrate the inaccuracy of what most textbooks explain concerning the famous Manhattan Project, frequently presented as the origin of modern PM. They thus bring to light alternative practices and theories that may be relevant today and strengthen project management theory.

This paper follows this second line of inquiry. It continues the reexamination of post-war US military projects started by Johnson (2002b), Lenfle (2008b, 2011) and Lenfle and Loch (2010). The post-war period is worth revisiting because it represents a turning point in the history of project management. Indeed, between 1945 and the joint publication by the Department of Defense and NASA of the *PERT/Cost System Design* in June 1962, PM moved from a mainly empirical field to a structured discipline governed by a rational view of project management. The story of this transformation, however, was more complex than usually told. Harvey Sapolsky, a political scientist, was the first to question the dominant view. His landmark contribution on the Polaris project (1972) uncovered the fallacy of the "*myth of managerial effectiveness*" (title of his fourth chapter) behind the PERT system. More recently Lenfle (2008b) and Lenfle and Loch (2010) deconstructed the assertion that "modern" project management originates in the Manhattan project.[2] On the contrary, they argued, this formidable project was a success thanks to its reliance on managerial strategies, such as parallel approach and rapid experimentation, which have disappeared from the PM textbooks. The existence of these other approaches to project management helps to contextualize the relevance of the rational model.

Here we propose to continue our earlier work by focusing on another forgotten project: the development of the Sidewinder missile by the Navy after WWII. We chose this case for two reasons. First, as we shall see, Sidewinder is a fascinating instance of an "illegal" R&D project that became one of the greatest bestsellers in missile history. Second, there is interesting material on this case but, as far as we know, it has not been explored in project management research.[3] More precisely, there are two types of materials concerning Sidewinder. On the one hand, there are histories of the project, which depict its unfolding and management (Marschak, 1964; Westrum, 1999). On the other hand, there is the testimony provided by the project director, William McLean (1960, 1962, 1971). These materials led us to discover a debate that echoes current concern on the relevance of standard PM practices for exploratory project management.

We have concentrated our attention on a particular set of events that, we concluded, best reveal the problems raised by the management of exploratory projects (Langley (1999) refers to this strategy as "*bracketing events for theoretical purposes*"). At the same time, we have included critically relevant details of the development of the project. We are especially indebted to Ron Westrum for sharing with us his vast knowledge of the Sidewinder case.

3. The Sidewinder case

The Sidewinder air-to-air missile (see Fig. 1 below) was developed at the Naval Ordnance Test Station (NOTS) at China Lake[4] in the Mojave Desert between 1947 and 1956. Set up in 1943 to provide a testing ground for the development of Navy rockets, NOTS became after the War one of the main R&D facilities of the US Navy. The story of Sidewinder started in 1947 with a NOTS survey of air-to-air homing devices. At the time, the primary goal of the military was to enhance their ability to shoot down (Soviet) bombers armed with atomic weapons. Due to its

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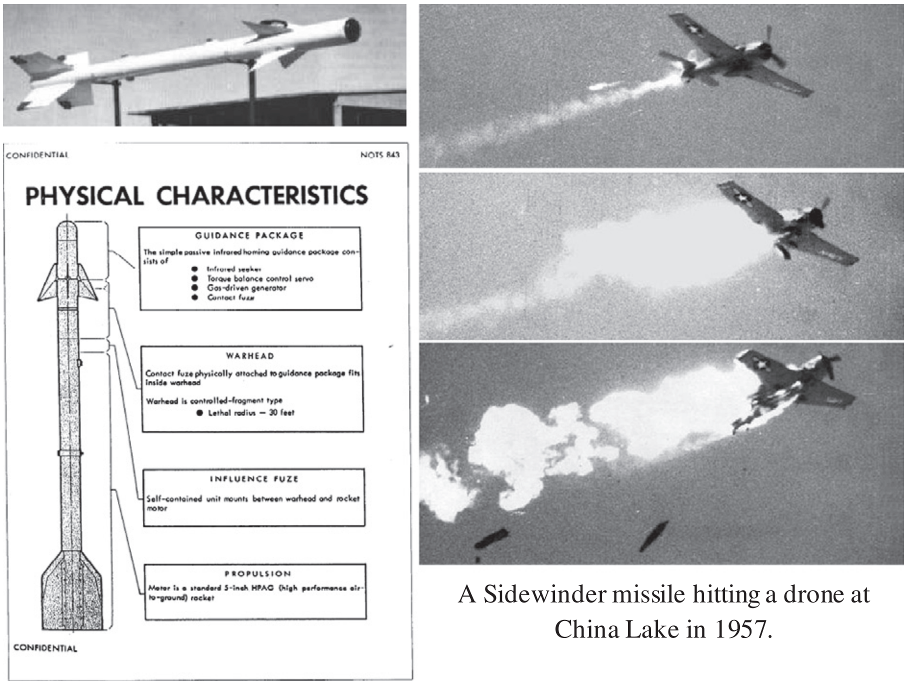
[2] For example, Shenhar and Dvir wrote in their 2007 book that "*The Manhattan Project exhibited the principles of organization, planning, and direction that influenced the development of standard practices for managing projects*" (p.8).

[3] We are grateful to John Byrnes who brings Sidewinder to our attention during the 2011 IRNOP conference.

[4] We will use NOTS and China Lake interchangeably.



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PHYSICAL CHARACTERISTICS

GUIDANCE PACKAGE

The simple passive infrared homing guidance package consists of

- Infrared seeker
- Torque balance control servo
- Gas-driven generator
- Contact fuze

WARHEAD

Contact fuze physically attached to guidance package fits inside warhead

Warhead is controlled-fragment type

- Lethal radius — 30 feet

INFLUENCE FUZE

Self-contained unit mounts between warhead and rocket motor

PROPULSION

Motor is a standard 5-inch HPAG (high performance air-to-ground) rocket

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A Sidewinder missile hitting a drone at China Lake in 1957.

Fig. 1. The Sidewinder air-to-air missile.

very low precision, the existing rocket technology proved inadequate for that purpose.

On the basis of the above-mentioned survey, William Mc Lean, a leading engineer with a Ph D in nuclear physics from Caltech, becomes convinced that the approach to missile development at the Navy and elsewhere was not appropriate. For him, the central problem of guided missiles, especially for highly maneuverable fighter aircrafts, lay in the unpredictability of the target (i.e. enemy planes) after missile launch. As he explained during US Senate Hearings in 1971, "*we were working on air-to-air rockets and fire control systems to guide air-to-air rockets and our problem was to find what introduced errors into the rocketry, fire control, and the total problem; and we found that all other sources of error were small compared to the amount of maneuvering that the target aircraft could do after he fired the rocket, and that convinced us we were never going to solve the problem either by improving the fire control or the rocketry, that the solution had to be in control after firing*" (McLean, 1971, p. 231). Mc Lean imagined that one solution to this problem "*was to put the fire control in the missile instead of the aircraft*" (Westrum, 1999, p. 31). This was a breakthrough insight. Indeed most of the ongoing developments (e.g. Falcon and Sparrow missiles) relied on radar technology: in order to guide the missile, the attacking plane was to define the target by means of its radar. This solution was very complex, and therefore raised reliability concerns, as well as expensive. Moreover, the size of the guidance system shrunk the room available for the warhead.

The idea of placing "fire control in the missile" raised the central question of the technology that was available to guide a missile toward an aircraft. For Mc Lean, "*the key to success was to use an infrared detector – much smaller than a radar*" (Westrum, 1999, p. 36). It turned out that jet tailpipes are good emitters of infrared. This lays the foundations of the Sidewinder design. However, transforming them into a working missile presented extremely difficult technical and organizational challenges.

On the organizational side there was, at the time, a strong US Navy and Department of Defense (DoD) opposition to developing guided missiles. McLean reported that "*every time we mentioned the desirability of shifting from unguided rockets to a guided missile, we ran into some variants of the following missile deficiencies:*

1. *Missiles are prohibitively expensive. It will never be possible to procure them in sufficient quantities for combat use;*
2. *Missiles will be impossible to maintain in the field because of their complexity and the tremendous requirements for trained personnel;*
3. *Prefiring preparations such as warm-up time and gain setting required for missiles, are not compatible with target of surprise and opportunity which are normally encountered in air-to-air and air-to-ground combat;*
4. *Fire control systems required for the launching of missiles are as complex, or more complex, than those required for unguided rockets. No problems are solved by adding a fire control computer in the missile itself;*
5. *Guided missiles are too large and cannot be used on existing aircraft. The requirement for special missile aircraft will always result in most of the aircraft firing unguided rockets*" (Westrum, 1999, p. 34).

In short, as Mc Lean summarized it, the "*real specifications" for the job "were all negative, (…) and so our objective* *on the Sidewinder program was to work out a solution that would avoid all of those objections that were then current about guided missiles*" (Westrum, 1999, p. 230). In addition, guided missiles were not part of NOTS assignments, and China Lake had even been explicitly told "*not to develop an air-to-air missile*" (Westrum, 1999). DoD believed there was already enough under development elsewhere.

Such circumstances did not prevent McLean from engaging in missile design. With the support of NOTS technical direction, he brought together a group of about 10 scientists, engineers and technicians from his division. L. Nichols was appointed head of the team. Everything was unofficial and supported through discretionary funds for exploratory research. The team began with a survey, done through site visits, of different ongoing missile project underway in the US. They thus arrived at the conclusion that a reliable and inexpensive product would have to be simpler than what was being developed. Technically speaking they faced two main problems:

1. They had to design a sufficiently sensitive infrared sensor,
2. They had to design a missile guidance system capable of relying on the information provided by the sensor.

McLean decide to use "*a lead sulfide photocell mounted on a rotating gyroscope [which] relied on electromagnets mounted in a ring around it to create precession and shift the gyroscope's focus toward the target* [and thus guide the missile on a collision course]. *The gyro would find the target, turn toward it, and the signal the missile to turn itself onto an interception course. Obviously the seeker would have to be in the nose of the missile*" (Westrum, 1999, p. 37–40). This constituted a breakthrough innovation. But if the ability of lead sulfide photocells to detect IR was well known, designing a sufficiently sensitive one (i.e. at 2 miles) and putting it in a missile were a huge challenge.

McLean and his team soon realized how big the challenge was. The first tests, in 1948, confirmed that a lead sulfide photocell would work, but also that it was not very sensitive and could not track a target that was more than one hundred meters away. Thus, the team had first to improve photocell performance and subsequently design a tracking device (the seeker). To do this McLean encouraged different groups to try different approaches. On June 10, 1949 a formal proposal was send to the Navy's Bureau of Ordnance (BuOrd). It explained that "*The missile would have*

a) *Infrared guidance, using a gyroscopically stabilized and electronically processed seeker*
b) *Forward guidance fins (canards) drive by pistons*
c) *A hot-gas power supply derived from rapid-burning grains to drive the pistons; battery power for the tubes also came from a gras-grain driven turbine*
d) *A servomechanism system producing a torque on the fin rather than a specific deflection angle (the "torque-balance servo system")"* (Westrum, 1999, p. 60).

This, however, was only a general description of the system. Much remained to be done and the feasibility of the system was far from proven. The seeker was one of the team main concerns. The uncertainty as to its feasibility was so great that, in 1950, five different solutions were under study. A memorandum of 25 October 1950 explained that "*three seekers for the rocket would be developed*

1. *The A seeker and amplifier were being developed in conjunction with Avion Corporation. McLean favored this design*
2. *The B seeker and amplifier project at China Lake directed by R. Estey, used a stationary armature to spin the gyro, external gimbals and magnetic precession.*
3. *The C seeker and amplifier head project at China Lake, directed by J Watson, used a central spherical bearing.*

Meanwhile, Aerojet also was working on a D head, and Estman Kodak was developing a E head" (Westrum, 1999, p. 63). The strategy of parallel development was applied to other key components, such as generators, servo valves and rotating choppers. At the same time, McLean chose to reduce the complexity of the system by reusing existing components. For example he "*decided early to build the missile around a standard propulsion system: the 5-inch high-performance air-to-ground (HPAG) rocket motor, a system China Lake had developed and was refining. China Lake was familiar with the rocket and its aircraft launch equipment was readily available. The HPAG fuselage was large enough to house a powerful warhead*". (Westrum, 1999, p. 74).

By that time (October 1950) the project's core team involved 24 people and, like a classic matrix organization, relied on functional departments and contractors.[5] NOTS was a perfect place to develop an innovative missile since it combined research, engineering and testing facilities. The team was thus able to quickly build and test prototypes.

The outbreak of the Korean War (June 25, 1950) and the development of soviet MiGs underlined the strategic importance of air warfare. At the end of 1950 "*all the key parts of the weapon system were under development: seekers, canard, rollerons generator*" (Westrum, 1999, p. 64). Laboratory tests demonstrated that the propulsion and guidance systems could work, and system integration became a central concern. W. LaBerge and H. Wilcox, both with doctorates in physics and wartime experience,[6] were hired to perform system engineering and project management respectively. In November 1950, the "heat homing rocket" adopted the name of a desert rattlesnake that detects the IR radiation emitted by its prey, and became the Sidewinder project. The project itself remained nonetheless hidden from the Navy's bureaucracy. For instance, in 1951 "*China Lake was told to lie low, cease talking about a missile, and speak instead of feasibility studies. The reason was that the Truman administration wanted to cut a large amount from the budget of Sidewinder, amounting to a cancellation of the program. So Sidewinder drops of the*

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[5] At the height of the project the core team included "*30 scientists and engineers and about a dozen technicians with one aerodynamicist (and assistants)*" (p. 86). This was rather small compared to "*the thousands of engineers and hundreds of aerodynamicist at Hughes working on the Falcon*".

[6] Wilcox spent most of the war at Los Alamos working for the Manhattan project.

budgeter's radar scopes. Previously it had been both Local Project 602 and Feasibility Study 567. (…) For about two years, the Sidewinder project was known as 'Fox Sugar 567' "(Westrum, 1999, p. 61).

This was a good idea since by mid-1951 the project desperately needed to show some concrete result, yet no seeker was ready to be mounted on a missile. To overcome such a situation the team decided to build a rough and low-cost prototype by mounting "*the detector on a radar antenna and use the feedback from the IR detector to get the antenna to follow the target. The antenna would thus become the seeker*" (Westrum, 1999, p. 50). An old surplus SCR-584 radar pedestal was quickly found and the team designed the prototypes in approximately four months. The SCR 584, as it was named, exemplifies China Lake approach to design: rapidly build low-cost prototypes to test the research findings, then modify the design according to the results. The IR-guided antenna was a complete success. It "*immediately became not only a critical test instrument but also an unparalleled marketing tool (…) crowds came to committee meetings just to watch the tracking films.*" So "*a second detector was soon mounted on the pedestal* [to compare] *the performance of different components of the optical system, such as reticles or filters"* (Westrum, 1999, p. 50)). The prototype also pointed to an area that needed hard work, namely the ability of the missile to separate the target from bright clouds: "*an antenna-mounted camera showed what the detector was tracking. The tracker was the visible proof that an IR seeker could track a bright object automatically, something that has not been demonstrated before. It tracked lighted candles, birds and even* bugs" (Westrum, 1999, p. 50)).

It was high time to solve that problem. Indeed, in May 1951 "*Mc Lean applied for navy funding to move the project from exploratory phase into development as a fleet weapon*" (Westrum, 1999, p. 87). A visit of Admiral W. Parson,[7] deputy director of BuOrd and representative of the R&D board was organized on October 11, 1951. During this critical meeting, "*films* [from the SCR 584] *still wet from the developing tank was brought into the meeting to provide convincing proof of concept*" (Westrum, 1999, p. 51). Impressed, Parson authorized 3.5 million for the current fiscal year to develop Sidewinder, and China Lake was granted full authority on the project.

In fact, however, the missile was far from ready. Several seekers were still under development and nobody knew which one would work best. To enhance the design, flight tests begun in 1951 with the old planes available at NOTS or elsewhere in the area. This led the team to simultaneously invent means (photography, telemetry and debris analysis) to analyze flights that were, by definition, very fast and short. Furthermore, as Westrum explains, test pilots, "*did more that fire the missile; they also evaluated system design*" (Westrum, 1999, p. 101). One of the pilots recalled: "[we] *went up in the F3D on the first captive flight with the missile and he said, "now, we've got this nickel-and-dime voltmeter in the middle of the cockpit and when that voltmeter shows 1½ volts, that's the right signal for the missile to see and fire." And I said, "You mean the pilot in a flying situation has to take his eyes off his target and look at the gauge to see if the missile, find out if the missile see the target? That's unacceptable." That's when we start differing. "We've got to get something besides the damn gauge. You can't have a pilot, a fighter pilot in combat looking at funny little gauges to see if he can fire or not". So he came up with a tone, and it's been use ever since*" (Westrum, 1999, p. 101). The pilots' input proved invaluable to the design of Sidewinder because it allowed for the integration into the missile design of real conditions of use, i.e. customer needs.

Tests and modifications continued throughout the two following years. "*The first air firing of a complete missile (using the type B seeker later rejected) took place in August 1952, and in November the first of 30 custom made missiles (with Type A seeker) were delivered. Philco was selected as prime contractor for the guidance and control section*" (Marschak, 1964, p. 111). Yet stabilizing the design proved extremely difficult. Thus in 1953 "*several types of seeker remained under consideration, and on every bench there was an engineer with a different design or different approach. No two missiles were ever fired the same way*" (Westrum, 1999, p. 108). Such a situation raised important problems. For example in 1953 missiles underwent unexpected guidance system shot after launch. Here again intense experimentation led to a solution. That same year, "*satisfactory performance of the Type A seeker head was achieved and, after a final demonstration of the Type B seeker, Type A was selected for the missile*" (Marschak, 1964, p. 111). By then, the organization had greatly expanded to 250–300 people, according to Mc Lean (1962), who also reported that they monitored "*work of at least four other government installations, as well as the prime contractor, and about ten or twelve other industrial organizations*" (p. 170).

Finally, for the first time on September 9, 1953, Sidewinder successfully shot a drone. Wilcox then established the list of tasks to be accomplished. The target date for fleet evaluation was January 1, 1956. This marked the beginning of the development phase, but modifications continued since many problems remained, among others, with guidance, fuses, and stability during flight. It took the team almost four months to obtain a second successful shot in January 1954. Thus, design/experiment cycles continued throughout 1954 and 1955, and led to a progressive definition of the missile characteristics; the guidance and control section was frozen for production at Philco in March 1954, and later in the year the influence and contact fuses (respectively designed by Eastman Kodak and Bulova R&D laboratories) went into production.

In January 1955 McLean, always attentive to user friendliness, recruited P. Nicols to prepare the fleet for Sidewinder. "[His] *job was to find out what was needed for regular shipboard installation of Sidewinder, a task for which no blueprint existed. Nicols, however, soon turned this inquiry into a fine art. He developed a comprehensive description of the missile and he drew up an exhaustive list of the shipboard support equipment that might be required to handle the missile, its component, checkout gear, and assembly process. This involved considerable imagination, but Nicols simply continued in his logical way to sort it all out. In the end, he prepared a large document that did much to ready the ships to receive Sidewinder* (Westrum, 1999, p. 127). As Nicols recalled, *"This was probably the first time that anyone from China Lake had*

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[7] Formerly, a key figure of the Manhattan project.

actually gone aboard a ship for the pure purpose of getting a weapons system, especially a guided missile system, aboard a ship. (…) *they were extremely happy to receive me and to receive the information that I gave them, and several of them sent back letters of appreciation, which indicated to me that not many, if any, people had done this previously*" (Westrum, 1999, p. 128). This reinforced McLean's initial insight that conditions of use should absolutely be integrated into the design. For example, he had the fins used to turn the missile located near the nose instead of near the tail. The reason was that he knew missiles were disassembled for stowage on aircraft carriers. Therefore guidance and control units were designed as a single assembly. This reduced the problems linked to the plugging and unplugging of the electrical connectors between the guidance unit and the fin when the missile was assembled.

The design freeze finally took place in March 1955. BuOrd began evaluation immediately, and the Fleet on January 3, 1956, exactly on schedule. The first operational Sidewinder squadron started on July 17, 1956 on the USS Randolph aircraft carrier. Tests carried at sea were so successful that the Chief of Naval Operations ordered all carriers to be equipped with Sidewinders. The first successful use in combat took place on 22 September 1958, when Taiwanese fighters with Sidewinder missiles provided by the US shot down four Soviet MiGs over the Formosa Strait. Sidewinder efficiency was later confirmed during the Vietnam War, when it demonstrated a "kill ratio"[8] double that of the competing Sparrow radar-guided missile. However, combat use revealed the need for pilot training to avoid shooting outside the envelope, i.e. too far and/or with the wrong tail angle.

In the end, Sidewinder development cost 32 million dollars between 1950 and 1957. This, according to Marschak (1964), represented "*a very low total development cost and a short development time compared to other air-to-air missile*" (p. 111). Since then, Sidewinder has given birth to a lineage of increasingly effective missiles, from the AIM-9B of 1956 to the AIM-9X (developed by Raytheon), which entered service in 2003. It has been adopted by all US armed services and more than 27 nations, and remains the most successful air-to-air missile in history both for the quantity produced and for combat efficiency.

4. Lessons for the management of exploratory projects

We now turn to the main question: what can we learn from the Sidewinder case, particularly as far as managerial practices are concerned? We believe that the case constitutes a valuable contribution to the emerging literature on the management of exploratory projects (Brady and Davies, 2004; Klein and Meckling, 1958; Lenfle, 2008a; Loch et al., 2006). This body of research has so far produced several significant results:

1. A definition of exploratory projects (Brady and Davies, 2004; Lenfle, 2008a) and the demonstration that traditional project management is irrelevant when faced with unforeseeable uncertainties (Loch et al., 2006; Pich et al., 2002);
2. A conceptualization of exploratory projects as experimental learning processes (Loch et al., 2006, p. 119), the identification of two fundamental strategies for dealing with unforeseeable uncertainties, namely selectionism (parallel testing of design alternatives) and learning (sequential testing), and a framework for choosing between them (see also Sommer et al., 2009);
3. A discussion of the organizational and managerial implications of this approach that emphasizes sensemaking (Loch et al., 2006).

We nonetheless still lack research on the practices involved in the management of such exploratory projects. The Sidewinder case makes in this respect two interesting contributions. First, at a conceptual level, it again brings to light the lost roots of this "experimental" model. In this connection, history matters, and we hope that the case has demonstrated its relevance. Second, at the level of practice, it helps to strengthen the emerging model of exploratory project management. We shall now develop this second point, focusing on the input of the Sidewinder case to two fundamental dimensions of project performance: its inner functioning (project level) and its infrastructure and governance, which encompass its monitoring systems (e.g. Morris, 1997 or Loch and Kavadias, 2008). Indeed Sidewinder throws light into the functioning of a Skunkworks at this two level.

4.1. The Skunkworks: an "experimental learning processes" in action

One of the most theoretically fruitful evolutions in project management research is, in our view, the conceptualization of exploratory project as an experimental learning processes (Loch et al., 2006, p. 119). In this perspective exploratory projects are represented as plan/do/check/act (PDCA) cycles of experiments whereby the team in charge progressively maps the design space it explores. This model is fruitful because it bridges project and innovation management literatures which, until recently, have remained separate (Lenfle, 2008a). Innovation management research shows that the innovation process is first and foremost driven by experimentation (e.g. Thomke, 2003; Van de Ven et al., 1999). Thus the challenge is to define the "best" strategy of experimentation (see Loch et al., 2006).

There is not much research on the organizational dimension of this "*experimental learning process*". How is the team set up? What are the conditions for managing the PDCA cycle efficiently, beyond some general principles (e.g. recognize the value of failure, organize for rapid experimentation; Thomke, 2003)? What are the practices involved in this kind of project? One classical answer emphasizes team autonomy from the parent organization to manage radical innovation (Tushman and O'Reilly, 1996; Wheelwright and Clark, 1992). Such autonomy enhances the team's freedom, focus, creativity and integration. The much celebrated Lockheed Skunkworks® are presented as the classical example of this approach, but relatively little is known about its functioning. Ben Rich autobiography (Rich and Janos, 1994) and Miller' (1995) official history are the main, and a bit hagiographic, sources of information. This is where the Sidewinder case provides valuable materials.

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[8] Number of missiles shot/number of targets destroyed.

First, from the theoretical point of view, Sidewinder corroborates Loch's framework. As we saw, in connection with the SCR-584 prototype for example, the project's basic philosophy consisted of engaging in experimentation and rapid design cycles. There was no predefined phase, no specifications, and no clear deadline. The entire endeavor was based on experimentation. Moreover, given unforeseeable uncertainties, the team relied on a strategy of parallel design of different alternatives. Second, as far as practices are concerned, Sidewinder evinces the organizational conditions necessary to perform such strategy efficiently. It turns out that autonomy is not the only, perhaps not even the main, feature of Skunkworks. Most striking here is the significant role played by a small number of highly skilled engineers and by China Lake as a whole. The available material suggests the crucial importance of two features of the NOTS, namely:

1. China Lake combined research labs and testing facilities in the same location. This was invaluable, since it provided "*all the tools needed to do a complete job, from basic research from testing, plus conversations with fleet personnel as to which techniques were most likely to be acceptable to the people who would be using the equipment*" (McLean, 1962). Nichols' comments on the role of the SCR-584 prototype in China Lake's success are illuminating (Westrum, 1999, p. 55):

   "I attribute all that to the old radar facility that has been the work horse over the years. You can do this filter selection on paper, at a desk, having the spectrum of the background target, but there is no substitute for going out and doing it against the real thing. That we did! We had everything we needed here at the weapons center. We had airplanes, a place to fly them, the aircraft ranges, and we could talk to the pilots. We could tell them what we were trying to do. We had air controllers out on the ranges that knew what we were trying to do, close cooperation. We could sit and wait for the right day, with these puffy cumulus clouds, and we were all ready to go the minute we had the 'bad background' we wanted… It was just this set up that let us study these parts and choose the proper ones".

   The central role of the technical infrastructure around the team has remained understudied. The integration of activities from basic research to testing in a single location, as well as their constant availability allowed the team to test novel ideas without much paperwork and delays. Each time the Sidewinder team had a solution to experiment with, they were able to test it quickly. As Thomke (2003) observed, rapid experimentation is an organizational problem (see for example his analysis of the New-Zealand team organization in the America's cup). In short, Sidewinder demonstrates that autonomy is nothing without the supporting infrastructure — something that is also obvious, at Lockheed Skunkworks.
2. China Lake is located in the Californian Mojave Desert, 240 km north of Los Angeles. Its isolation facilitated the creation of a tightly knit community. According to McLean (1962) "*communications were facilitated by the fact that the group was isolated in a small community in the desert. (…) People could and did communicate with each other all day, through the cocktail hours, and for as long as parties lasted at night. This isolation in a location where the job could be performed provided large measures of the intimate communication which is so essential for getting any major job completed*". Sidewinder participants remembered a "*university-like atmosphere… Communication between higher levels and lower levels of the community was very good. If you though it was a problem that* [McLean] *might know something about, you could go over and talk to him about. And he would show great interest in what you were doing. So the result was that each guy working on that program developed a real commitment to get his part of the job done*" (C. Smith, in Westrum, 1999, p. 68). There were no organizational barriers at China Lake. "*For example McLean took sketches directly to machinists and might get a part in half a day – and test it immediately. This can be contrasted with standard procedures, which might take three days* [or more]. *(…) This approach saved time. But more important, the direct contact between the head of the project and technicians resulted in fewer communication hurdles and inspired the technicians, who subsequently went extra miles to get things right*" (Westrum, 1999, p. 97). All these features contributed to bring about a sense of common fate. As Westrum put it, "*McLean's appearance in the lab after hours and his constant presence on the firing ranges showed that no one was above getting his hands dirty*" (p. 223). "*The project made sense. Every hour put in after work, every postlaunch party, every interaction with McLean, Wilcox, LaBerge or Ward told the members that they were part of something special.*" (p. 223). Thus the opposition by some members of the Navy's bureaucracy only "*raised our morale, sharpened our thinking, and kept our costs down – it's too bad every project cannot have this type of opposition*» (Wilcox, in Westrum, 1999, p. 114).

In sum, the Sidewinder case not only exemplifies the power of the so-called "skunkworks" to manage exploratory projects, but also throws light into their inner functioning, i.e. into the conditions of "integrated problem solving" (Clark and Fujimoto, 1991) in situations of exploration. To be successful, such situations require four mutually reinforcing elements:

a. a small, dedicated team in close interaction with the user;
b. the immediate availability of the necessary equipment to build prototypes and thus accelerate the design/build/test cycles;
c. a kind of isolation (though not necessarily a desert!) to foster the creation of a real community; and
d. an innovation champion such as W. Mc Lean. It comes as no surprise that Marschak (1964) attributes a large part of Sidewinder's success to the "*main organizational property of the project – the great amount of authority given to the developing laboratory, and in particular to its head, who happened to be a gifted designer as well as the originator of Sidewinder*" (p. 112). Leadership is obviously crucial. McLean possessed the features of the "champions" often described in innovation management research (see Gemunden et al., 2007 for a synthesis) — individuals who are both undisputed technical leaders and central management figures. It is thus

1. Coordinate work carefully to avoid duplication : Everything new can be made to look like something we have done before, or are now doing
2. Keep the check reins tight; define mission clearly; follow regulations : Nothing very new will ever get a chance to be inserted
3. Concentrate on planning and scheduling, and insist on meeting time scales : New, interesting ideas may not work and always need extra time
4. Ensure full output by rigorous adherence to scheduled workday : Don’t be late. The creative man sometimes remembers his new ideas, but delay in working on them helps to dissipate them
5. Insist that all plans go through at least three review levels before starting work : Review weeds out and filters innovation. More level will do it faster, but three is adequate, particularly if they are protected from exposure to the enthusiasm of innovator. Insist on only written proposals
6. Optimize each component to ensure that each, separately, be as near perfect as possible : This leads to a wealth of « sacred » specifications which will be supported in the mind of the creative man by the early « believe teacher » training. He will the reject any pressure to depart from his specifications.
7. Centralize as many functions as possible : This create more review levels and cuts down on direct contact between people.
8. Strive to avoid mistakes : This increase the filter action of review
9. Strive for a stable, successful productive organization : This decrease the need for change and justifies the opposition to it

Fig. 2. “How to squelch genius”. McLean, California Management Review, 1960.

striking to note the similarity between China Lake and other successful laboratories. Wartime Los Alamos immediately comes to mind (see Hoddeson et al., 1993), and we find in McLean the charismatic traits that characterized Robert Oppenheimer and other famous leaders[9] (on Oppenheimer see Thorpe and Shapin, 2000).

4.2. Sidewinder and the relevance of the Stage-Gate© process for radical innovation

From the standpoint of project governance, the Sidewinder case appears paradoxical. Indeed, it constitutes an indictment of the current body of knowledge, for it violates all the alleged “best practices” of project management. There were no customer, no requirements, no planning, and no WBS… and yet it was unquestionably successful in time/cost/quality, and resulted in a major innovation and a long lasting best seller. Moreover, had McLean listened the customer’s voice or followed a Stage-Gate process, there would probably never have been a Sidewinder missile since, at the time, nobody in the Navy believed in guided missiles. As a classic story in the management of innovation, the Sidewinder case should make us question the rational approach for innovation, for it is consistent with recent criticism of the Stage-Gate process as having a potentially devastating impact on the development of radical innovation (Sehti & Iqbal, 2008; van Oorschot et al., 2010).

The value of the historical perspective becomes clear at this point. Indeed, the debate over innovation strategies has a long but forgotten history. W. McLean himself was a fierce critic of the US Department of Defense bureaucratic processes that are at the origin of the phased approach (Johnson, 2000) today known as the Stage-Gate Process. According to him, such an approach killed creativity, and increased costs and delays (McLean, 1960, 1971). He explained his position first in a *California Management Review* paper of 1960 and again in 1971 during hearings before the Committee of the Armed Service of the US congress.

The *CMR* paper focused on the impact of formal organizational processes on creativity. McLean ironically explained “how to squelch genius” i.e. how to “*change an innovative organization into one doing only routine productive work*”. He identified nine practices that would destroy creativity by imposing a primarily managerial logic on what he called “creative scientists” (Fig. 2, p. 9). In his view, all the principles that, at that time as today, constitute the core of the dominant model of project management (clear goal, predefined schedule, one best solution, strict review process…) destroy an organization’s creative capability.

McLean repeated his criticism before the Armed Service Committee of the US Senate in 1971, focusing on the drift of the weapons acquisition process toward a purely “*ritualistic*” procedure almost completely severed from real design work (Fig. 3, p. 10). He implicitly denounced a sort of self-contained and self-driven process that led to:

A. the scattering of the design work among different departments without sufficient coordination (hence his emphasis on the role of the senior engineer, which seems to anticipate Clark & Fujimoto’s stress on the Heavyweight project manager to enhance “product integrity”);
B. the prevalence of paperwork, which generated unrealistic requirements, cost explosion and schedule drift (points 2 and 3 below);
C. an administrative burden that reduced engineers’ productivity and creativity (point 4).

Mc Lean’s position is sobering for the discipline of project management. Half a century ago, it anticipated current debates about the relevance of a stage-gate process for innovative projects. It thus reveals that there has always been disagreement

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[9] For example H. Rickover for the design of Nuclear Submarines, W. Rabborn for Polaris, and W. von Braun for Saturn V in Apollo.

> *"the weapon system acquisition process is now dangerously inadequate because*
> 1. *We have forgotten the importance of a senior designer to guide development of each system*
> 2. *The need for development prototypes to demonstrate technical feasibility before the writing of military requirements has been ignored;*
> 3. *The total acquisition process reward the design of complex and expensive systems and penalize work on simpler, and therefore, less expensive ones*
> 4. *The budgetary process, I believe, has become ritual with no content, which is occupying more the 50 percent of the productive time of our best technical people at the laboratory level and the full time of large numbers of technical people in Washington."* (p. 225)

Fig. 3. McLean hearings before the Armed Service Committee of the US Senate (1971).

concerning the relevance of formalizing project management processes for the purpose of innovation. As Sehti & Iqbal in 2008, already in the 1960s McLean warned against the rigidity of formal PM processes and their negative side-effects on creativity and innovation, and advocated, from his Sidewinder experience, multiple experimentations and a process where requirements come at the end rather than at the beginning. As he explained before Congress, "*military personnel need a chance to test a developmental prototype in operational tests and on the basis of this experience they will be in a position to write realistic requirements for the procurement process*" (p. 226). This is close to Sehti & Iqbal's demonstration that rigid adherence to initial (and generally wrong or incomplete) requirements and gates leads to *project inflexibility* and failure. In short, McLean defended the exact opposite of the dominant PM process, which sees clear specifications as a necessary starting point.

It is striking that, in the same period, the RAND Corporation theorized the need for flexibility in the management of *exploratory development* (Alchian and Kessel, 1954; Arrow, 1995; Klein and Meckling, 1958; Nelson, 1959). Thus, in the 1960s, researchers on the one hand and practitioners like McLean on the other agreed on the specificity of exploratory projects, as well as on the diversity of the problems faced by military R&D and the need to differentiate management methods accordingly (Alchian and Kessel, 1954). Although they were at the heart of the US military R&D process, they failed to prevail — or gave up their conclusions when they reached top positions in the McNamara administration (for example C. Hitch, who became controller of the DoD budgeting process; Johnson, 2000). We hope that the Sidewinder case suggests the value for PM research of a genealogical analysis of the disappearance of those debates in favor of a strictly rational, "one size fits all" approach.

5. Conclusion

We started this article with the ongoing debate on the lost relevance of the current project management body of knowledge, specifically its disjunction with practices (Hällgren et al., 2012). We argued that this body of knowledge is unsuitable for managing exploratory projects, i.e. those whose goals and means cannot be defined ex ante. We thus advocate a historical approach that, in our view, may help to learn about practices and, therefore, strengthen both the critics and the design of alternative models of project management. This led us to rely on Michel Foucault's genealogical approach in order to avoid the classical pitfalls of historical analysis, namely presentism and finalism. We decided to focus on the forgotten paths, practices, models, and modes of organization lost during the institutionalization of PM through professional associations like the PMI. Our purpose has been to question common views about the development of PM. We have therefore explored one of the post-war US military projects that are usually, but mistakenly presented as the roots of modern project management.

We hope to have made four contributions. First, we discussed, through Foucault, new possibilities for doing a history of project management. Second, we sketched a case that, as far as we know, has never been studied by the project management research community.[10] The case is particularly valuable for PM research, since it violated all the so-called "best practices" yet ended in a splendid success. Third, the Sidewinder case strengthens our understanding of exploratory project management as well as of the role of the organizational and technical infrastructure of the famous, but understudied, skunkworks. Finally, we brought back to light old debates about the relevance of formal project management processes to manage radical innovation — debates that, at half-a-century's distance, anticipated discussions concerning the widely-used stage-gate process.

Of course, a lot remains to be done. We pointed to forgotten practices, but did not explain how they disappeared as the rational approach to project management became institutionalized. Johnson (2000) and Lenfle and Loch (2010) have taken some steps in that direction, but more genealogical research is necessary to grasp the micro-mechanisms that led to the prevalence of the current body of knowledge. We nevertheless hope to have contributed to enriching our knowledge of the history of PM and to developing a research stream (Blomquist et al., 2010; Cicmil and Hodgson, 2006; Hällgren and al., 2012; Loch et al., 2006; Sahlin-Andersson and Söderholm, 2002) that proposes more relevant PM models.

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[10] For example, this case is not quoted in Morris's (1997) landmark *The management of projects*.

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[11] Thanks to Ron Westrum for sending us this rare document.