le cnam
Lirsa

i3 — Centre de Recherche en Gestion — École Polytechnique ParisTech

Toward a management of exploratory projects

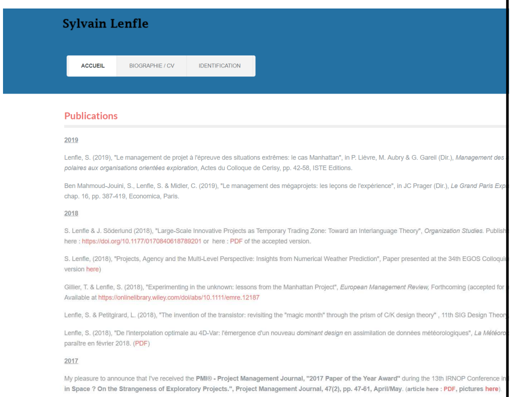
Sylvain Lenfle

R&D MANAGEMENT CONFERENCE

Masterclass – Ecole Polytechnique,
Paris, 19-21 june 2019.

Who am I ?

- Professor of innovation management at the Conservatoire National des Arts et Métiers (CNAM, LIRSA, Paris)
- Associate Researcher at the Management Research Center of the Ecole Polytechnique (i3/CRG)
- Associate Professor at Ecole des Mines / TMCI Chair.



le cnam

Plan of the talk

1. Influences
2. A brief history of PM research
3. « Strange projects » in space telecommunications
4. Lost roots
5. Conclusion

le cnam

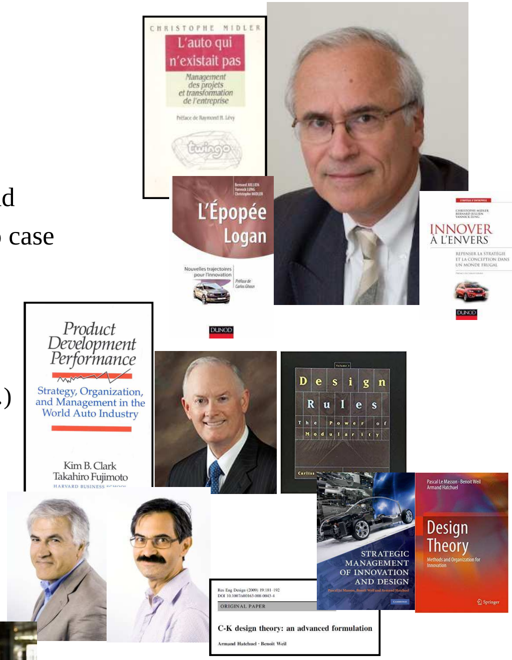
Plan of the talk

1. Influences
2. A brief history of PM research
3. « Strange projects » in space telecommunications
4. Lost roots
5. Conclusion

le cnam

Influences

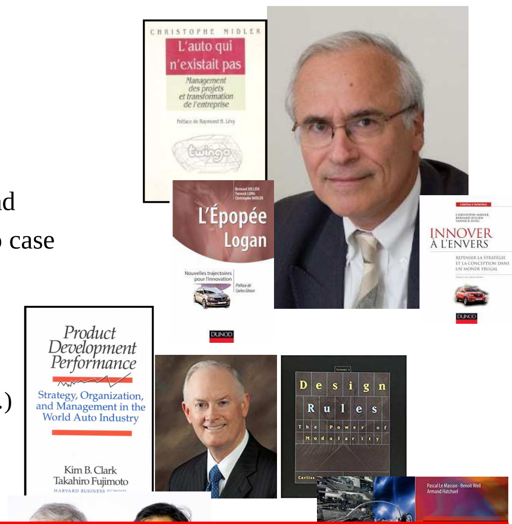
- ***Christophe Midler***
  - PhD advisor, colleague & friend
  - NPD Projects, innovation, auto case
- ***Kim B. Clark***
  - NPD / Auto study
  - Innovation and design (Clark, 1985; Henderson & Clark, 1990…)
- ***A. Hatchuel & B. Weil***
  - CK design theory
  - Innovative design, RID, …
- ***P. Fridenson***
  - Business history



le cnam

Influences

- ***Christophe Midler***
  - PhD advisor, colleague & friend
  - NPD Projects, innovation, auto case
- ***Kim B. Clark***
  - NPD / Auto study
  - Innovation and design (Clark, 1985; Henderson & Clark, 1990…)
- ***A. Hatchuel & B. Weil***
  - CK design
  - Innovative
- ***P. Fridenson***
  - Business hi



=> At the crossroad, between Project and Innovation management, <u>with a heavy dose of innovation</u>. This is quite typical of the 2000's renewal of PM research (Davies & al., 2018).

Plan of the talk

le cnam

Klein & Meckling, « Application of OR to development decisions », *Operation Research* 6(3), 1958

Mr Optimizer	Mr Skeptic
• Rational up-front analysis of the potential solutions • Commitment to the « optimal » solution • Development of this solution with important ressources to speed-up the process • No ressource left for back-up	He makes "***deliberate effort to keep his program flexible*** *in the early stages of development so that he can take advantage of what he has learned. (…) In order to maintain flexibility he commit resources to development only by stages, reviewing the state of his knowledge at each stage prior to commitments* "

Klein & Meckling, « Application of OR to development decisions », *Operation Research* 6(3), 1958

Mr Optimizer	Mr Skeptic
• Rational up-front analysis of the potential solutions • Commitment to the	He makes "***deliberate effort to keep his program flexible*** *in the early stages of development so*

On military R&D projects Klein & Meckling demonstrates the superiority of M. Skeptic approach, even for economically constrained projects.

But (unfortunately) PM took the « optimizing » road early on… (Lenfle & Loch, 2010 ; Lenfle & Soderlünd, 2013; Davies & al., 2018)

A brief history of project management research

(Lenfle & Loch, 2010 ; Davies & al., 2018)

History of Innovation and Project Management Research (selected references).

Time period	Innovation research	Project management research
1950–1959	Models of the innovation process in uncertain projects. Contrasting two different kinds of approaches to innovation projects (Klein and Meckling, 1958).	Critical path method (Kelley and Walker, 1959) PERT (Program and evaluation review technique) Work breakdown structures (Gaddis, 1959) Critical success and failure factors (Avots, 1969)
1960–1969	Contingency frameworks, including organizing structures (Burns and Stalker, 1961) and project procedures (Woodward, 1965). Project managers as integrators (Lawrence and Lorsch, 1967). Projects and matrix structures (Middleton, 1967). Projects as "voyages of discovery" (Hirschman, 1967).	Project control and planning (Souder, 1969). PERT (Program and Evaluation Review Technique) and Critical path methods (Archibald and Villoria, 1967; (King and Wilson, 1967; Miller, 1962) Systems analysis (Cleland and King, 1968). Q-GERT modelling (Pritsker, 1968) Cost, time and scheduling (Lucas, 1971; Perry et al., 1971).
1970–1979	Project managers as organizational metronomes (Sayles and Chandler, 1971). Project	Cost control (Murphy et al., 1974). Project management models (Crowston, 1971). Critical success factors (Murphy et al., 1974; Thamhain and Gemmill, 1974). Systems and software engineering (Brooks, 1975). Project success and failure (Kharbanda and Stallworthy, 1983; Pinto and Prescott, 1988).
		Risk management (Ashley and Avots, 1984). Tools and techniques (Liberatore and Titus, 1983). Scheduling (Levitt and Kunz, 1985). Effectiveness of project structures (Gobeli, 1987). Projects as temporary organizations (Lundin and Söderholm, 1995).
		Typological theory of project management (Shenhar and Dvir, 1996). Low-tech and high-tech project management (Shenhar, 1993). Projects as waterfalls and fountains (Lindkvist et al., 1998). Diamond model (Shenhar and Dvir, 2007). Exploration projects (Lenfle, 2008)

Innovation and Project Management Research: Key Differences.

	Innovation research	Project management research
Theoretical foundation	Contingency theory	General systems theory
Approach	Adaptive	Optimizing
Emphasis	Strategy and opportunities	Control and deviations
Managerial level	Top management	Middle management/project management
View on uncertainty and risk	Focus on opportunities, positive risk, risk willingness	Focus on negative risk, focus on methods for risk management, risk aversion, controlling progress, avoiding deviations
Management focus	Designs and structures	Tools and techniques

Projects and innovation

- A complex story (Lenfle, 2008; Davies & al, 2018)
  - PM textbooks points to the relevance of PM to management innovation…
  - … but the standard model remains dominated by a « rational » view of project as the convergence toward a predefined goal
  - IM deals with PM by encapsulating it as an « organic » structure… without looking at PM research (Davies &al., 2018)

- Contemporary research on PM demonstratres
  - The fallacy of the « one size fits all » approach of PM (Shenhar & Dvir, 2007)
  - The irrelevance of the « optimizing » view for exploration i.e. when *unk unks* exists (Loch & al., 2006 & next)

le cnam

Plan of the talk

1. Influences
2. A brief history of PM research
3. « Strange projects » in space telecommunications
4. Lost roots
5. Conclusion

le cnam

Origins of my research on exploratory projects

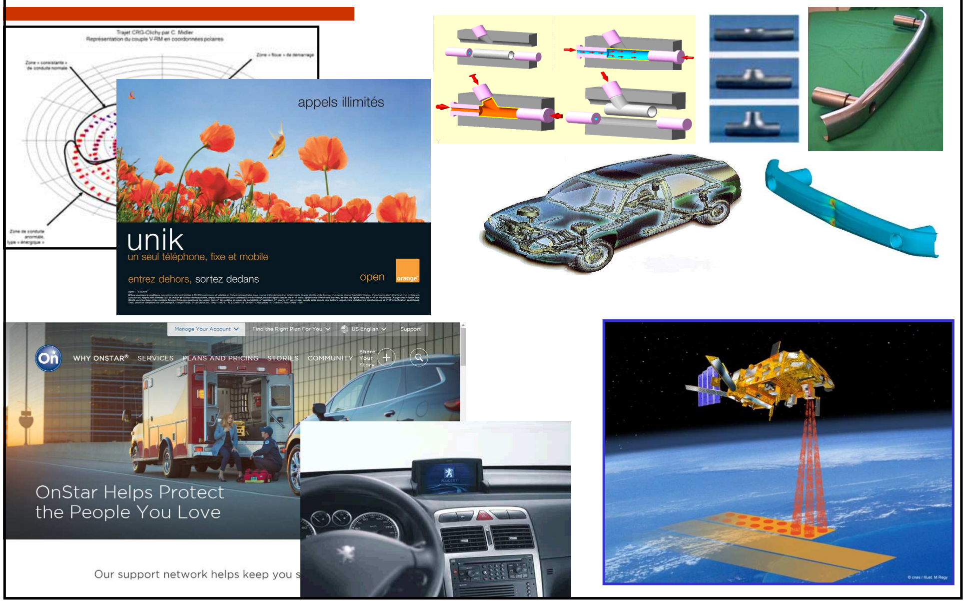
Double surprise

1. ***Field research*** : collaborative research with firms shows a discrepancy between « theory » and practice of PM (Lenfle, 2008 ; also Hällgren & al., 2012)

2. ***History*** : research on the roots of « modern » project management (Manhattan, Atlas/Titan, Polaris, Sidewinder, Apollo)

=> 2nd discrepancy between what textbooks said about these projects and what really happens.

Field research on exploratory projects



Exploratory projects

(Lenfle, 2008, 2011, 2014)

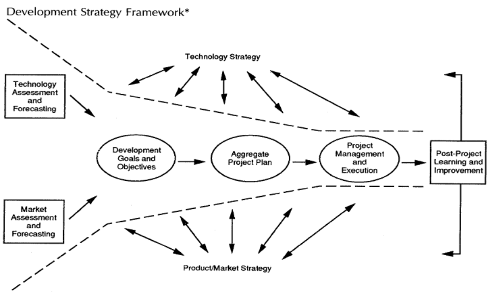
- Exploratory projects : innovative project for which neither the goals nor the means to attain them are clearly defined from the outset since "*little existing knowledge applies and the goal is to gain knowledge about an unfamiliar landscape*" (McGrath, 2001).

- Five characteristics of « exploratory projects » :

  1. Emerging, strategically ambiguous project
  2. Proactive projects
  3. The difficulty of specifying the result
  4. Exploration of new knowledge
  5. Hidden urgency and multiple time horizons

le cnam

Consequence : the inadequacy of the standard model of PM for exploration



Source : Wheelwright & Clark, 1992

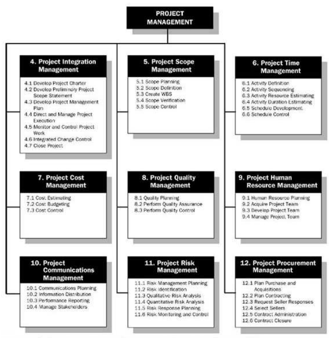
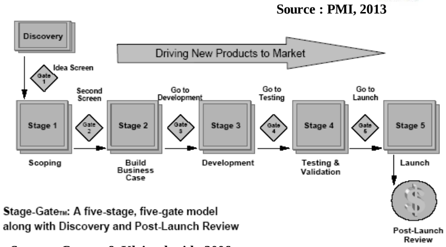


Figure 1-1. Overview of Project Management Knowledge Areas and Project Management Processes

Source : PMI, 2013

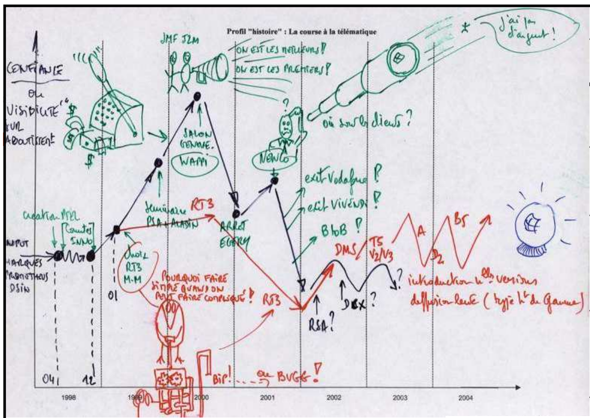


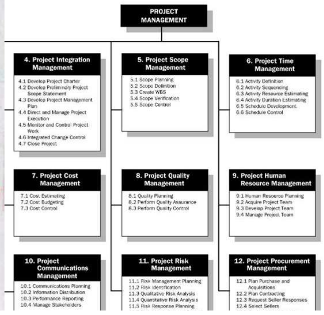
Stage-Gate™: A five-stage, five-gate model along with Discovery and Post-Launch Review

Source : Cooper & Kleinschmidt, 2006

le cnam

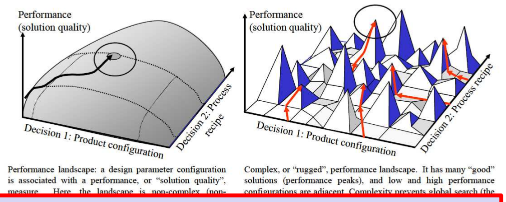
…f the standard model …ation

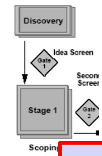




Source : séminaire interne PSA, novembre 2001

Source : Wheelwright & Clark, 1992



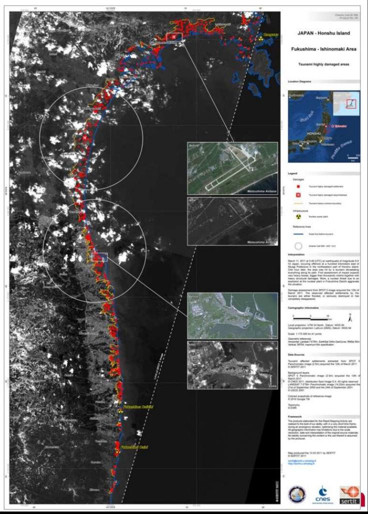


=> The FLIP case at the french space agency (CNES)

Research context : the space industry

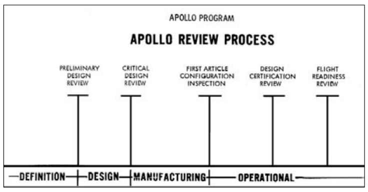
An ongoing research with the Centre National d'Etudes Spatiales (CNES), a leading space agency.

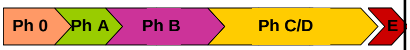
cnes
CENTRE NATIONAL D'ÉTUDES SPATIALES

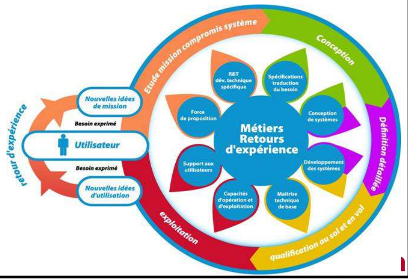


An archetype of rational PM

- The roots of project management methods (Morris, 1994; Johnson, 2002)…
- … still in use today given
  - Technical complexity
  - Very high cost (300 M€ for a telecom satellite)
  - Irreversibility induced by launch in space.
- A wise solution to ensure quality of design work from the drawing board to the launch pad (~ growing TRL)
- Problems arise when this approach is blindly applied to all projects.



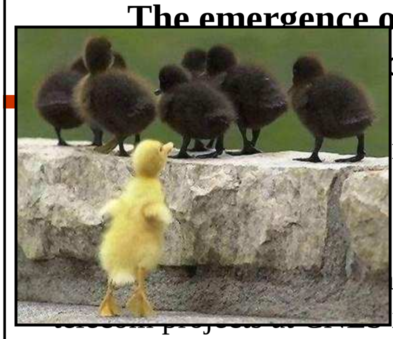




The emergence of « *strange projects* » in space telecommunications.

- Telecom is by far the first market of the space industry (>50% in revenues)
- A one-day workshop on innovation at CNES with the head of telecom projects at CNES in february 2013 .
- He explains that is confronted to « *strange projects* » that does not fit into CNES PM processes : goals not clear and changing, working on concepts and not objects, hard to define deadlines, etc.

The emergence o



- He
  int
  co

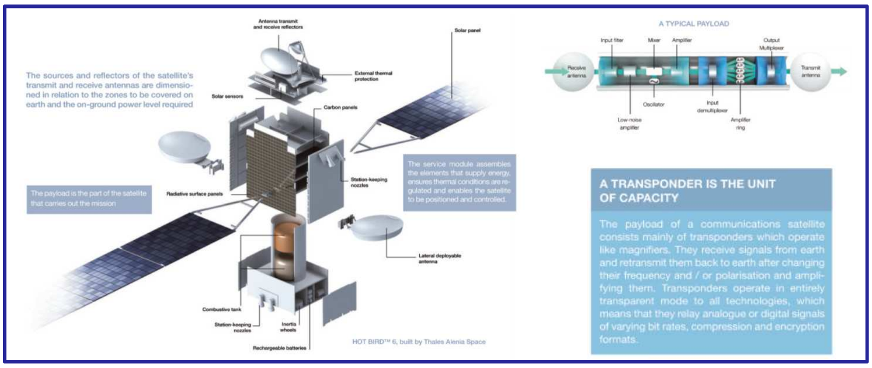
=> With the deregulation of the 90's CNES's mission evolves from chief designer of satellite to a more ambiguous position of "support to industry competitiveness".

⇒ Shift from hardware design to concept exploration and/or competence development.

⇒ *<u>Projects that seems to be floating</u>* compared to the standard model of PM

The FLexible Innovative Payload (FLIP) project

(2006 – 2014)



- A telecom satellite is basically a transmitter that receives a signal for the ground and broadcast it over a predefined territory
- How to make it "flexible" i.e. change in bandwith and/or territory <u>after</u> launch ? => FLIP project launched in 2006.

le cnam

The FLexible Innovative Payload (FLIP) project

(2006 – 2014)

- « Flexibility » is a loosely defined concept.
  - Operators want to reallocate frequencies, change the area, modify the power of the satellite, etc but nobody knows how to do this.
- However FLIP started as a « normal » project
  - Requirements defined by strategic planning department
  - « R & T studies » on payload components.
  - ⇒ Start directly in B phase (proof of concept already done) to move fast.
- BUT they soon discover that
  - Requirements were incomplete
  - R&T study largely useless.
  - ⇒ New round of interview with Telco operator to understand what « flexiblity » really means => 18 months => from 2 to… 27 different missions

The FLexible Innovative Payload (FLIP) project

(2006 – 2014)

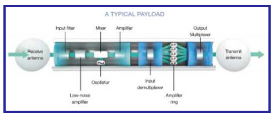
- « Flexibility » is a loosely defined concept.
  - Operators want to reallocate frequencies, change the area, modify the power of the satellite, etc but nobody knows how to do this.
- However FLIP started as a « norm
  - Requirements defined by strategic pl
  - « R & T studies » on payload compo
  - ⇒ Start directly in B phase (proof of co
- BUT they soon discover that
  - Requirements were incomplete
  - R&T study largely useless.
  - ⇒ New round of interview with Telco o really means => 18 months => from

"*We recognize that* [short laugh]… *(…) The solutions proposed by R&T were not competitive and, next, we decided to explore again the needs of the operators.* Moreover "*around 50% of R&D studies were useless so we had to do again upstream engineering studies*" (ibid.).

=> they actually do a "*kind of 0/A phase*" again. They had to abandon the proposed solution and design new ones.

Exploring the design space

- With this in mind FLIP explore the impact and potential solutions for the components of the payload (antenna, transponder, onboard chips…) and its architecture



- **Example 1 : transponder design**
  - 4 solutions identified but none of them are satisfying in terms of performance and/or cost.
  - The 2009 project review split the work in two parts :
    1. the development of an unsatisfying but mature solution for the short-term needs of a customer,
    2. the exploration of the way to satisfy the 27 missions while maintaining the highest level of commonality between the solutions to avoid overdesign and additional costs => give birth to three types of products (in EQM stage) able to cover the range of missions. One of them is under development and planned to fly in two years.

le cnam

Exploring the design space

Example 2 : antenna design

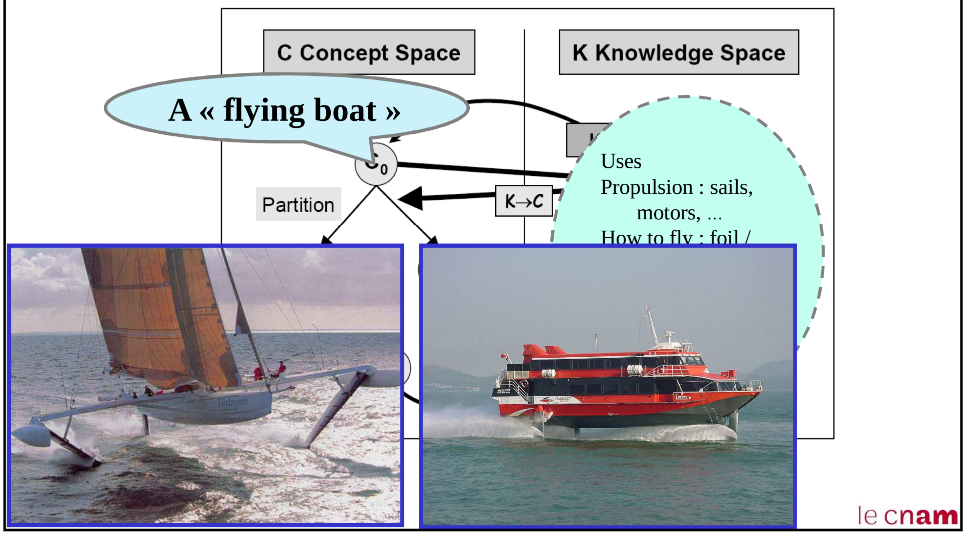
- a central component to enhance flexibility,
- reopen the way antennas were designed : move from the mechanical (non-flexible) dominant design to electro-mechanical designs.
- Different solutions were studied, some of them not for short-term applications but **because they allow the development of fundamental competencies for future antennas**. Ex : X-antenna, too heavy, too expansive but that leads to the development in France of a new production processes that, until now, were mastered by a single US firm.

"***This is a textbook case of a decision that is not directly linked to the product***. *We know that the X-antenna is penalized in terms of performance: we lose 3dB and it's a bit expensive. But the benefit is that it is a technology driver for two process technology that, until now, are mastered only by the US. Now European firms also mastered these technologies. (…) Operators and other projects at CNES are beginning to look at it. And the way we designed it makes it compatible for different applications*" (FILP PM).

le cnam

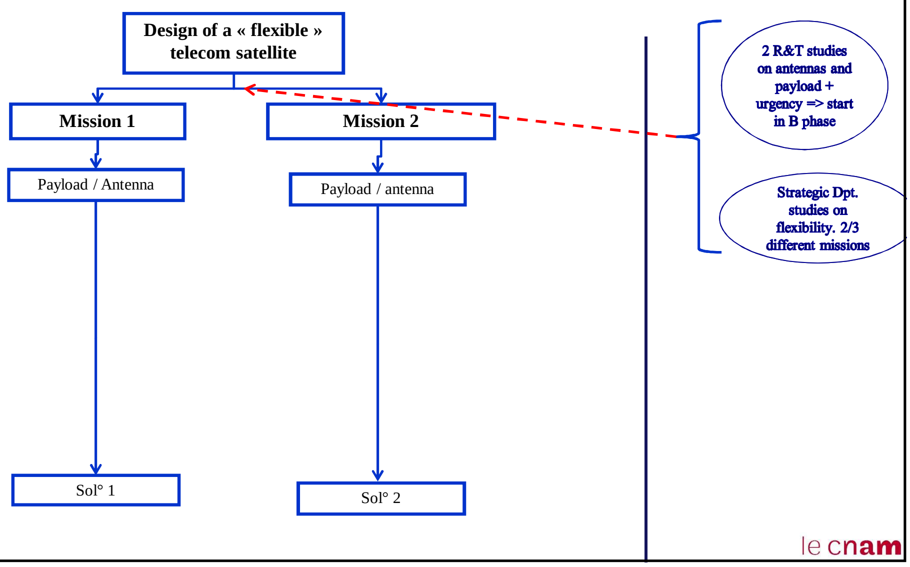
C/K design theory

(Hatchuel & Weil, 2009; Agogué & al, 2014)

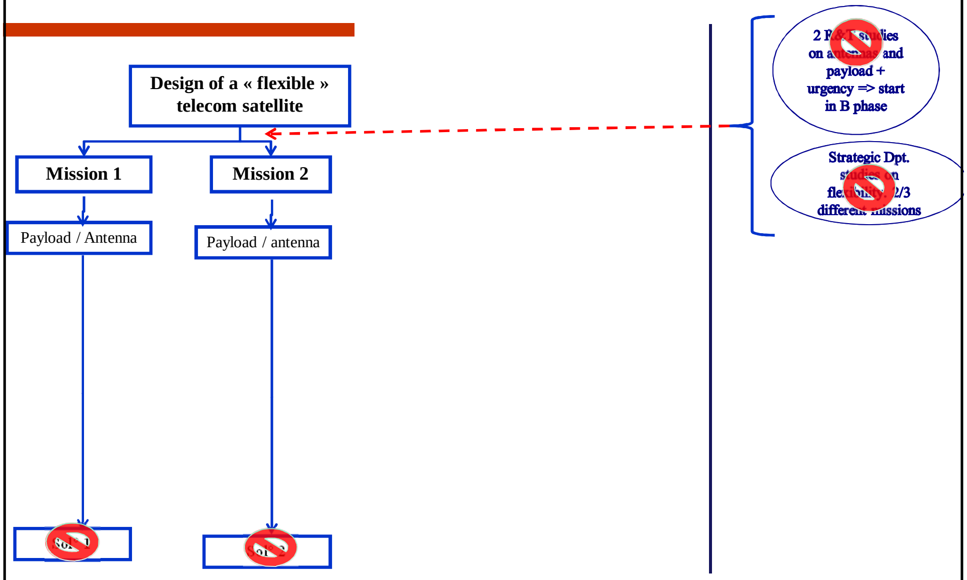


FLIP starting point : classical PM

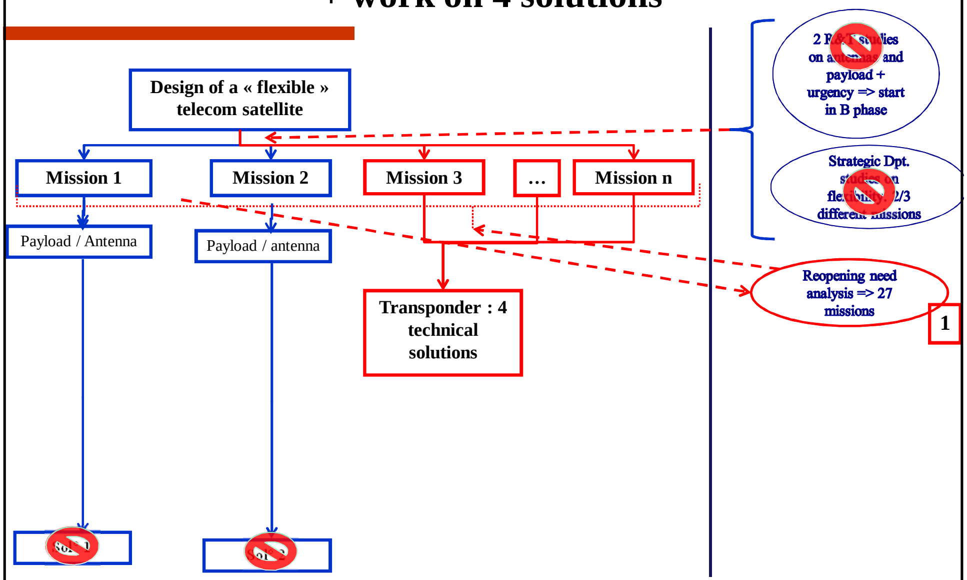
(On C/K see Hatchuel & Weil, 2009 or Agogué & al., 2014)



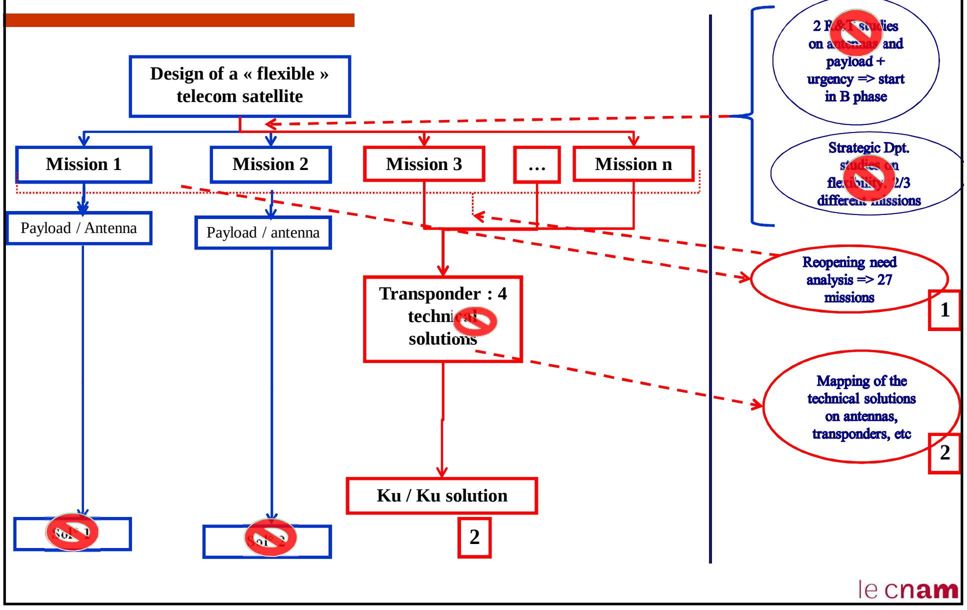
BUT…



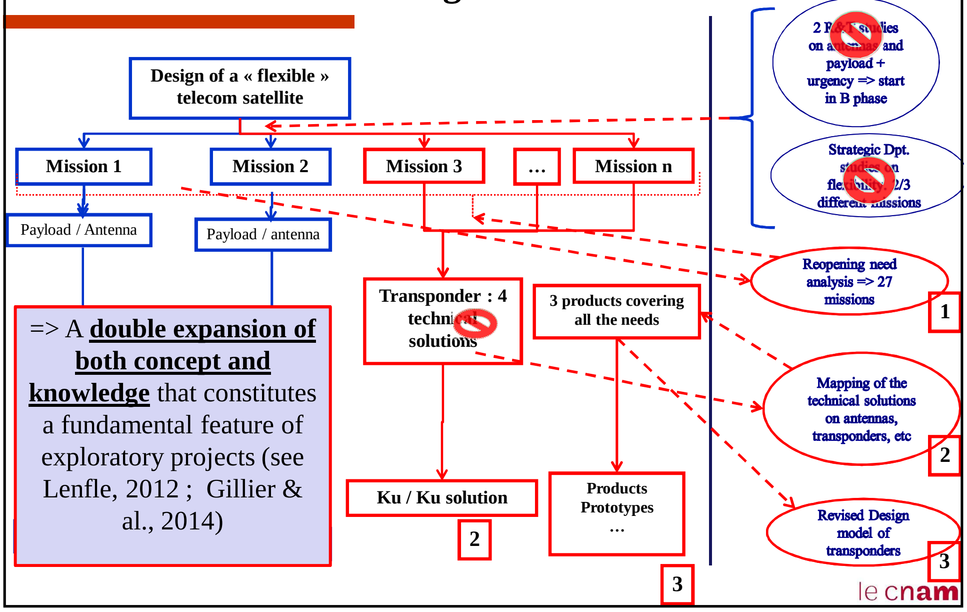
Step 1 : reopening mission need + work on 4 solutions



Step 2 : development of Ku / Ku + mapping of technical solutions



Step 3 : three generic products + revision of the design models



FLIP as an exploratory project

FLIP is a typical case of exploratory projects (Lenfle, 2008) :

- Difficulty to specify the goal ex ante ;
- Questioning of the stage-gate process => a constant back and forth between stages, sudden acceleration, stage overlapping, etc.
- It manage simultaneously different temporality, both short-run development and long-term exploration.
- Creation of new knowledge and new design rules to re-open the dominant design ;
- “Results” are more complex than in traditional development project (mainly a product). .

=> Projects that maps an “*unfamiliar landscapes*” and build new competencies, instead of mainly using what already exists to reach a clearly defined goal.

The « results » of FLIP

1. Qualified products (i.e. EQM)
2. Prototypes that demonstrates the usefulness and feasibility of a solution
3. Mapping of the design space defined by the concept of flexibility
4. New design models that can be reused for future project.
5. New competencies as exemplified by the X-antenna.

New design models :

“***This is probably the major result of FLIP****. (…) Now that we’ve done this thinking we keep it for future projects. For example we find similar question on THD-Sat. They were quickly converging on a solution but they didn’t really understand why. So we stop the project and apply a FLIP-like logic to put the problem in perspective*”.(FLIP PM).

“***Cross-fertilization is central here****, including application outside of telecoms or flexibility. It’s a dimension we always try to take into account.* ***It’s bizarre compared to traditional projects which are focused on the components they need, and that’s all. We try to break this logic that consists in strictly following the requirements***”. (FLIP PM).

The illegitimacy of exploratory projects

- SMILE PM : “***<u>you can only be the dunce</u>***. *(…) Even building a work plan is complicated. I found myself at a kick off meeting were I was asked to define budget requirements even though we were a bit in the dark on what we wanted to do. We try to present it under an acceptable form*”. (…) “*It’s not an easy project. You need to have the faith. You need to balance it with something else. It’s good to be a team. We talk, we lift each other spirit.”*
- “*SMILE is a fuzzy object, people outside the team have problems to understand what it is about*” (head of PM department)
- FLIP PM : “*there is an important risk of developing the wrong product because the schedule target is too stringent*”.

le cnam

The illegitimacy of exploratory projects

- SMILE PM : “***<u>you can only be the dunce</u>***. *(…) Even building a work plan is complicated. I found myself at a kick off meeting were I was asked to define budget requirements even though we were a bit in the dark on what we wanted to do. We try to present it under*

=> these projects have to circumvent existing processes by putting on make-up : “***in order to survive the only solution is to dress the project like it is supposed to be: with a red nose if you need a red nose, white shoes, yellow tie and so on***” (Head of PM department)

- This workaround strategy, frequently observed in innovation management, should be a last resort.
- The challenge for firms, and for PM research is, on the contrary, to recognize the specificities of exploratory projects and to differentiate the management processes.

Structuring exploration through projects

- **Orientation toward practical goals** : «*We are not exploring for exploring*. (…) *We try to do something that works, to answer efficiently to the way we see the goal. We are not here to explore a lot of things, we want that things serves to do something tomorrow. Whereas sometimes in R&T you search everywhere. Here we have constraints of cost, delay and feasibility*”. (FLIP)

- **Pacing the exploration** : “*project reviews are a huge added value compared to individual action. The project has to justify collectively toward the outside world. It gives visibility, it gives deadlines, it gives meaning.*” (SMILE)

- **Building a structured community** :
  - “*each department considered separately (or our partners) would not have any interest in exploring. Here to combine our forces creates a* ***critical mass****. And people are happy with this, it creates contact, it creates challenge. (…) I found that we need to see each other, there are also human stakes, the feeling to get things moving together. (…)*” (SMILE PM)
  - “*it creates coherence, an impressive dynamic. Instead of doing small R&T studies the team knows that we are also going to build products, there something of development, we consider the interfaces with the entire system*”. (FLIP PM)

Structuring exploration through projects

- **Orientation toward practical goals** : «*We are not exploring for exploring*. (…) *We try to do something that works, to answer efficiently to the way we see the goal. We are not here to explore a lot of things, we want that things serves to do something tomorrow. Whereas sometimes in R&T you search everywhere. Here we have constraints of cost, delay and feasibility*”. (FLIP)

- **Pacing the exploration** : “*project reviews are a huge added value compared to*
  in
  gi
- **Bu**

When is it finished ?

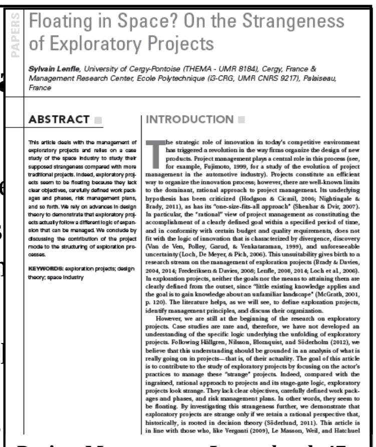
1. the budget is exhausted
2. the project has reach the end date (see also Dugan & Gabriel, 2014 on PM at DARPA)
3. the innovation field has been sufficiently studied.
   - ***a)*** ***Saturation*** : “*Today, on flexibility, we have what we need.*”
   - ***b)*** ***Expandability*** : the ability to generate new explorations (“*The concept of generic solution is out of FLIP scope but will be studied in another project named GEICO”)*

« Floating »… really ?

- Exploratory projects are not “floating” : they obey to a different logic.
  - They are experimental learning processes (Loch & al., 2006). Goals and means are progressively identified during the course of the project.
  - Design theory helps us to clarify the expansive logic of these projects in C and K. We are able to
    - characterize their unfolding (double expansion in concept and knowledge),
    - specify their results
    - and identify promising criteria (saturation and expandability) for their evaluation (see also, Gillier & al., 2014).
  - Exploratory projects constitutes a powerful tool to structure the, potentially very fuzzy, exploration processes.

« Floating »… rea

- Exploratory projects are not “floating” : the
  - They are experimental learning process
    Goals and means are progressively iden
    the project.
  - Design theory helps us to clarify the ex
    projects in C and K. We are able to
    - characterize their unfolding (double
      knowledge),


PAPERS

Floating in Space? On the Strangeness of Exploratory Projects

Sylvain Lenfle, University of Cergy-Pontoise (THEMA - UMR 8184), Cergy, France & Management Research Center, Ecole Polytechnique (i3-CRG, UMR CNRS 9217), Palaiseau, France

ABSTRACT

This article deals with the management of exploratory projects and relies on a case study of the space industry to study their supposed strangeness compared with more traditional projects. Indeed, exploratory projects seem to be floating because they lack clear objectives, carefully defined work packages and phases, risk management plans, and so forth. We rely on advances in design theory to demonstrate that exploratory projects actually follow a different logic of expansion that can be managed. We conclude by discussing the contribution of the project mode to the structuring of exploration processes.

KEYWORDS: exploration projects; design theory; space industry

INTRODUCTION

The strategic role of innovation in today's competitive environment has triggered a revolution in the way firms organize the design of new products. Project management plays a central role in this process (see, for example, Fujimoto, 1999, for a study of the evolution of project management in the automotive industry). Projects constitute an efficient way to organize the innovation process; however, there are well-known limits to the dominant, rational approach to project management. Its underlying hypothesis has been criticized (Hodgson & Cicmil, 2006; Nightingale & Brady, 2011), as has its "one-size-fits-all approach" (Shenhar & Dvir, 2007). In particular, the "rational" view of project management as constituting the accomplishment of a clearly defined goal within a specified period of time, and in conformity with certain budget and quality requirements, does not fit with the logic of innovation that is characterized by divergence, discovery (Van de Ven, Polley, Garud, & Venkataraman, 1999), and unforeseeable uncertainty (Loch, De Meyer, & Pich, 2006). This unsuitability gives birth to a research stream on the management of exploration projects (Brady & Davies, 2004, 2014; Frederiksen & Davies, 2008; Lenfle, 2008, 2014; Loch et al., 2006). In exploration projects, neither the goals nor the means to attaining them are clearly defined from the outset, since "little existing knowledge applies and the goal is to gain knowledge about an unfamiliar landscape" (McGrath, 2001, p. 120). The literature helps, as we will see, to define exploration projects, identify management principles, and discuss their organization.

However, we are still at the beginning of the research on exploratory projects. Case studies are rare and, therefore, we have not developed an understanding of the specific logic underlying the unfolding of exploratory projects. Following Hällgren, Nilsson, Blomquist, and Söderholm (2012), we believe that this understanding should be grounded in an analysis of what is really going on in projects—that is, of their actuality. The goal of this article is to contribute to the study of exploratory projects by focusing on the actor's practices to manage these "strange" projects. Indeed, compared with the ingrained, rational approach to projects and its stage-gate logic, exploratory projects look strange. They lack clear objectives, carefully defined work packages and phases, and risk management plans. In other words, they seem to be floating. By investigating this strangeness further, we demonstrate that exploratory projects are strange only if we retain a rational perspective that, historically, is rooted in decision theory (Söderlund, 2011). This article is in line with those who, like Verganti (2009), Le Masson, Weil, and Hatchuel

***Project Management Journal*, vol. 47 n°2, 2016. Best paper award, 2017.**

=> Massive fixation-effect on the standard model of PM => urgency to re-open the concept of project that, for too long, has been equated with the rational, decision-based, model.

=> this hinders our ability to think other (exploratory) project logic that are important in today’s innovation-based competition.

Plan of the talk

1. Influences
2. A brief history of PM research
3. « Strange projects » in space telecommunications
4. Lost roots
5. Conclusion

le cnam

Origins of my research on exploratory projects

Double surprise

1. ***Field research*** : collaborative research with firms shows a discrepancy between « theory » and practice of PM (Lenfle, 2008 ; also Hällgren & al., 2012)
2. ***History*** : research on the roots of « modern » project management (Manhattan, Atlas/Titan, Polaris, Sidewinder, Apollo)

=> 2nd discrepancy between what textbooks said about these projects and what really happens.

le cnam

Historical cases of parallel strategies

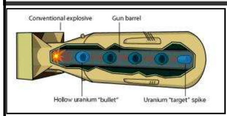
Manhattan Project (e.g. Lenfle, 2011)

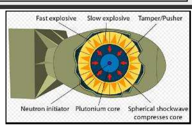
Managerial strategy (1943)

- *Electromagnetic separation (Y-12)*
  - *Research*
  - *Engineering*
  - *Plant construction*
- *Gaseous diffusion (K-25)*
  - *Research*
  - *Engineering*
  - *Plant construction*

Uranium path

- *Plutonium production*
  - *Research*
  - *Engineering*
  - *Protype Pile (X10 – Oak Ridge)*
  - *Plant construction (Hanford)*
- *Bomb design (project Y)*
  - *Research on plutonium and uranium*
  - *Research on implosion*
  - *Gun design*
  - *Los Alamos construction*





Navy Nuclear Propulsion Program in 1953	*Water-Cooled reactor*	*Sodium-Cooled reactor*
AEC Field Office	Pittsburgh	Schenectady
AEC Contractor	**Westinghouse** (Bettis Laboratory)	**General Electric** (Knolls Atomic Power Laboratory)
Land prototype	Submarine Thermal Reactor STR (Mark I) National Reactor Testing Station (Idaho)	Submarine Intermediate Reactor (SIR) Mark A, West Milton, New York
Nuclear submarine	Nautilus SSN 571 STR Mark II	Seawolf SSN 575 SIR Mark B
Shipyard	Electric Boat	Electric Boat

Parallel strategy on the Atlas Project (1954 – 1959)	*Atlas*	*Titan*
Airframe	Convair	Martin
Guidance 1. Radio-Inertial	General Electric	Bell Telephone
Guidance 2. All inertial	A.C. Spark Plug	American Bosch / MIT
Propulsion	North American	Aerojet General
Nose cone	General Electric	AVCO
Computer	Burroughs	Remington Rand

But also :

- **Polaris A1 to A3**
- **Sidewinder on guidance seeker (up to 5)**
- **Appolo LEM descent engine in 1963-64 and ascent engine in 1967**
- **…**

Historical cases of parallel strategies

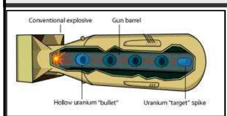
Manhattan Project (e.g. Lenfle, 2011)

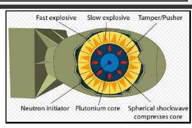
Managerial strategy (1943)

- *Electromagnetic separation (Y-12)*
  - *Research*
  - *Engineering*
  - *Plant construction*
- *Gaseous diffusion (K-25)*
  - *Research*
  - *Engineering*
  - *Plant construction*

Uranium path

- *Plutonium production*
  - *Research*
  - *Engineering*
  - *Protype Pile (X10 – Oak Ridge)*
  - *Plant construction (Hanford)*
- *Bomb design (project Y)*
  - *Research on plutonium and uranium*
  - *Research on implosion*
  - *Gun design*
  - *Los Alamos construction*





Navy Nuclear Propulsion Program in 1953	*Water-Cooled reactor*	*Sodium-Cooled reactor*
AEC Field Office	Pittsburgh	Schenectady
AEC Contractor	**Westinghouse** (Bettis Laboratory)	**General Electric** (Knolls Atomic Power Laboratory)
Land prototype	Submarine Thermal Reactor STR (Mark I) National Reactor Testing Station (Idaho)	Submarine Intermediate Reactor (SIR) Mark A, West Milton, New York
Nuclear submarine	Nautilus SSN 571 STR Mark II	Seawolf SSN 575 SIR Mark B
Shipyard	Electric Boat	Electric Boat

Parallel strategy on the Atlas Project (1954 – 1959)	*Atlas*	*Titan*
Airframe	Convair	Martin
Guidance 1. Radio-Inertial	General Electric	Bell Telephone
Guidance 2. All inertial	A.C. Spark Plug	American Bosch / MIT
Propulsion	North American	Aerojet General
Nose cone	General Electric	AVCO
Computer	Burroughs	Remington Rand

And the (forgotten) « RAND » literature :

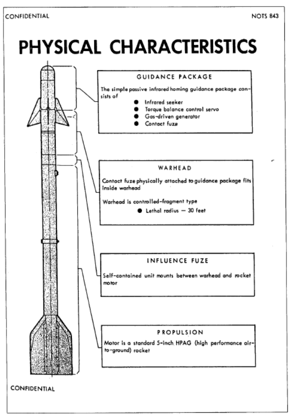
Alchian & Kessel (1954); Arrow (1955); Klein & Meckling (1958); Nelson (1959 & 1960); Abernathy & Rosenbloom (1969).

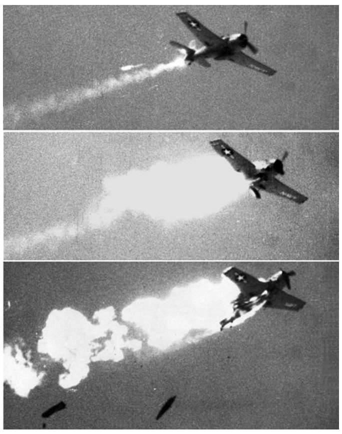
« Parallel strategy » ?
(PM BoK, 5th ed., 2013)

?

The Sidewinder air-to-air guided missile
(1947 – 1956 & after)

(Lenfle, IJPM, 2014)





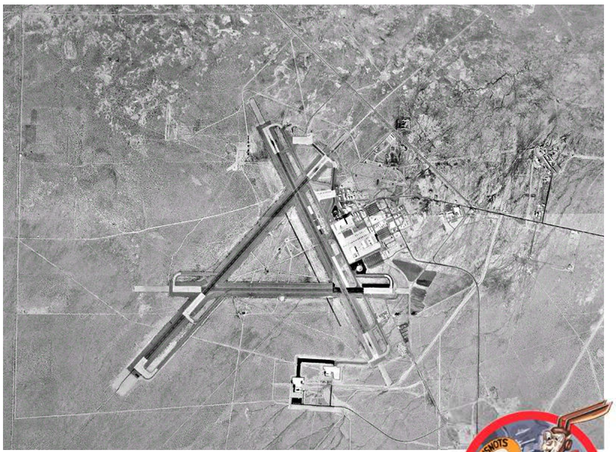
A Sidewinder missile hitting a drone at China Lake in 1957.

The **AIM-9 Sidewinder** is a heat-seeking short-range, air-to-air missile carried by fighter aircraft. It is named after the Sidewinder snake, which detects its prey via body heat.

Naval Air Weapons Station China Lake

(formerly Naval Ordnance Test Station – NOTS)

- Created in 1943 as a R&D and test center for the Navy
- In the Mojave desert, 240km north-east of Los Angeles
- Involved in the Manhattan & Polaris projects, among others.



le cnam

Starting point : the NOTS survey

- Context : cold war and fear of USSR nuclear attack with bombers
  - How to shoot them down ?
  - A survey launched by the Naval Ordnance Test Station at China Lake, California (involved in Manhattan and, later, Polaris)
- McLean conviction : "we *were working on air-to-air rockets and fire control systems to guide air-to-air rockets and (…) we found that all 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**"*(1971, p. 231).

le cnam

Starting point : the NOTS survey

- Context : cold war and fear of USSR nuclear attack with bombers
  - How to shoot them down ?
  - A survey launched by the Naval Ordnance Test Station at China Lake, California (involved in Manhattan and, later, Polaris)

- McLean conviction : "w*e were working on air-to-air rockets and fire control systems to guide air-to-air rockets and (…) we found that all sources of e[…] that the targ[…] convinced u[…] improving t[…] in **control a**[…]*

Two technical breakthrough

- Putting the fire control into the rocket to provide pilots with *fire and forget* capability (vs. Radar guidance)
- Infrared-based heat-seeking guidance (heat homing rocket that « catch » jet tailpipes)

A strong opposition within Navy / DoD

According to McLean "*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.*
2. *Missiles will be impossible to maintain in the field.*
3. *Prefiring preparations (…) are not compatible with target of surprise and opportunity which are normally encountered in combat;*
4. *Fire control systems required for the launching of missiles are as (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.*" (Westrum, p. 34)

McLean (1971) : "***we started Sidewinder without a set of specific specifications****. Our main objective was to find something that would do the job of air-to-air rockets more effectively and cheaper, and* ***our real specifications to start with 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.*"

Moreover :"***China Lake had been told not to develop an air-to-air missile***" (ibid.) since, DoD thought, there was already enough under development.

D

ty of shifting
ariants of the

3. Prefiring preparation … rise and opportunity which

4. Fire control systems r … (or more) complex than th … problems are solved b … ile itself;

5. Guided missiles are to … aircraft." (Westrum, p

The voice of the customer…

A skunkwork© story

- A small unofficial project team supported through discretionary funds for exploratory research.
- Hidden from the Navy until may 1951 : code name "*'Fox Sugar 567' (…) dropped off the budgeteer's radar scopes"*.
- A very clever design strategy
  - Parallel exploration of different solutions for the seeker (up to five in 1950) and key components…
  - … while reusing others (i.e. propulsion system) to reduce complexity and delays.
- A close and continuous interaction with users (pilot and carriers)
- A strategy of rapid experimentation based on China Lake facilities

A skunkwork© story

- A small unofficial project team supported through discretionary funds for exploratory research.
- Hidden from the Navy until may 1951 : **code name "*'Fox Sugar 567' (…) dropped off the budgeeter's radar scopes*".**
- A very clever design strategy
  - Parallel exploration of different solutions for the seeker (up to five in 1950) and key components…
  - … while reusing others (i.e. propulsion system) to reduce complexity and delays.
- A close and continuous interaction with users (pilot and carriers)
- A strategy of rapid experimentation based on China Lake facilities

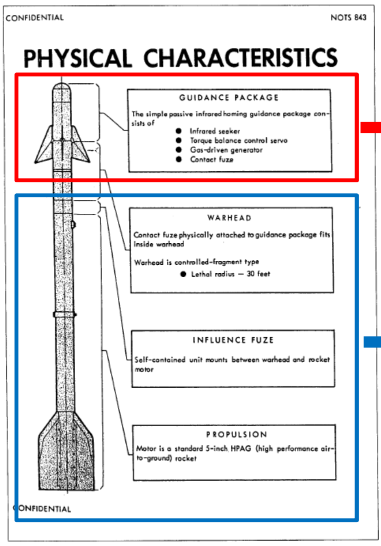
le cnam

A skunkwork© story

- A small unofficial project team supported through discretionary funds for exploratory research.
- Hidden from the Navy until may 1951 : code name "*'Fox Sugar 567' (…) dropped off the budgeeter's radar scopes*".
- A very clever design strategy
  - **Parallel exploration** of different solutions for the seeker (up to five in 1950) and key components…
  - … while reusing others (i.e. propulsion system) to reduce complexity and delays.
- A close and continuous interaction with users (pilot and carriers)
- A strategy of rapid experimentation based on China Lake facilities

le cnam

Sidewinder's design strategy



Parallel strategy : *massive unk unks* => up to 5 solutions quite late in the project (1953) on the seeker

Re-use of architecture and existing components

le cnam

A skunkwork© story

- A small unofficial project team supported through discretionary funds for exploratory research.
- Hidden from the Navy until may 1951 : code name "*'Fox Sugar 567' (…) dropped off the budgeeter's radar scopes*".
- A very clever design strategy
  - Parallel exploration of different solutions for the seeker (up to five in 1950) and key components…
  - … while reusing others (i.e. propulsion system) to reduce complexity and delays.
- A close and continuous interaction with users (pilot and carriers)
- A strategy of rapid experimentation based on China Lake facilities

le cnam

Rapid experimentation in action : The modified SCR-584 (1951)

- An old surplus WWII radar (1942) modified with an IR-seeker to track planes and test/compare IR-seekers performances
- Exemplifies China Lake approach to design : rapid building of low-cost prototypes to test the research findings and, then, modify the design accordingly.
- 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.*"
  - Points to central design problems. Ex : the ability of the missile to separate the target from bright clouds.

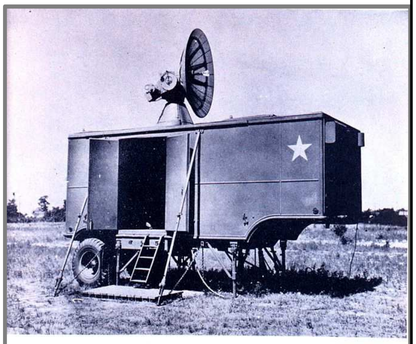


Figure 22. Exterior View of Radio Set SCR-584.

le cnam

A skunkwork© story

- First flight tests in 1951 => integrate conditions of use

  "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." (Westrum, p. 101)
- Official Navy funding in october 1951
- First firing of a complete missile in august 1952…
- … but several seekers are still in development until 1953
- First successful shot of a drone on September 9, 1953.
- Start of the work to prepare the fleet for Sidewinder in 1955.

  "This was probably the first time that anyone from China Lake had actually gone aboard a ship for the pure purpose of getting a weapons system, especially a guided missile system, aboard a ship"
- Design freeze in march 1955
- First operational sidewinder squadron started in july 17, 1956.

A skunkwork© story

- First successful use in combat on 22 September 1958: Taiwanese fighters shot 4 soviet MiGs over the Formosa Strait.
- Sidewinder development has cost 32 million between 1950 and 1957, which was, according to Marschak (1964), "***a very low total development cost and a short development time*** *compared to other air-to-air missile*" (p. 111).
- **High performance** compared to radar-guided missiles
- **A best seller in missile history** : starting point of a lineage of missiles, from the Sidewinder AIM-9B of 1956 to the AIM-9X (developed by Raytheon) which entered service in 2003.

le cnam

Sidewinder : an indictment of PM « best practices »

- Tremendous success... despite the fact that ***Sidewinder transgress all PM « best practices »***
  - Customer's skepticism and opposition to the project
  - No requirements / No planning / No budget
  - An understaffed « illegal » project team
  - Parallel exploration of different solutions (up to 5 on critical components) throughout the project AND reuse of existing components
- This cases, debates and practices disappeared from the official history and theory of PM after Abernathy & Rosenbloom's 1969 paper (= foundation of the PMI and … Armstrong's moon landing)
- … but the managerial practices remains relevant in today's innovation-based competition => This is why genealogy matters.

le cnam

Conclusion.
Toward a management of exploratory projects

le cnam

Five principles for the management of exploratory projects (Lenfle, 2008)

- Principle 1 : set up a specific organization
- Principle 2 : projects as experimental learning process
- Principle 3 : concurrent exploration

le cnam

Five principles for the management of exploratory projects (Lenfle, 2008)

- Principle 1 : set up a specific organization
- Principle 2 : pro
- Principle 3 : con

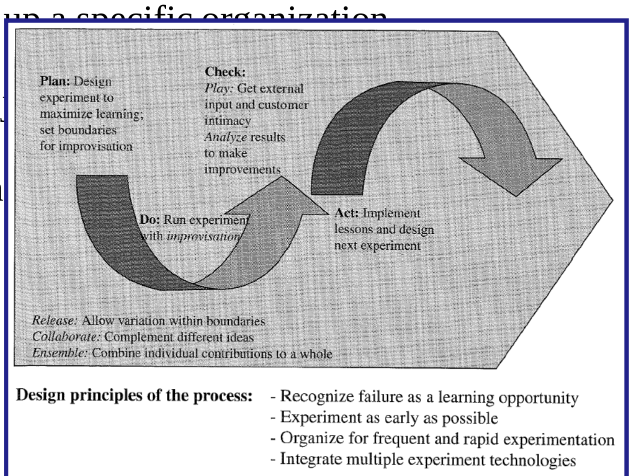
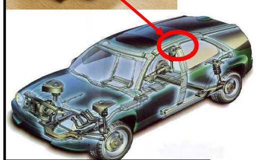
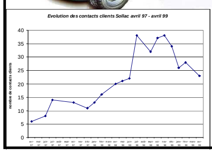


Figure 5.6 An experimental learning process

Hydroforming and *concurrent exploration*

(Gastaldi & Midler, 2005)

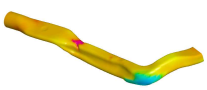




Exploring simultaneouly the technique *AND* its applications allows

1. Avoiding the trap of inaccessible target or technical solution without application
2. Learning simultaneously on :
   - ✓ Operational implementation (plumbing)
   - ✓ Theory of hydroforming
   - ✓ Numerical simulation
   - ✓ Potential applications of hydroforming

$$P(\varepsilon_{00}) = \frac{4}{3}\frac{\sigma(1+\frac{\alpha}{2})}{\sqrt{\frac{4}{3}(1+\alpha+\alpha^2)}}\frac{t_0}{R_0}e^{-\varepsilon_{00}(2+\alpha)}$$



le cnam

Five principles for the management of exploratory projects (Lenfle, 2008)

- Principle 1 : set up a dedicated organization
- Principle 2 : projects as experimental learning process
- Principle 3 : concurrent exploration
- Principle 4 : the dual nature of performance
- Principle 5 : constant reformulation of goals

le cnam

Five principles for the management of exploratory projects (Lenfle, 2008)

- Principle 1 : set up a dedicated organization

Four different results for exploration projects

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

=> Given unk unks, there is a need to manage this questions **during** the project

The road ahead

- Cases, cases, cases and more cases are needed to understand their inner functioning, how coordination occurs (Lenfle & Soderlund, 2018), the problem they met, the strategy they use, etc
- Management tools & methods to manage these projects, in particular project evaluation (e.g. CK as sensemaking).
- Governance / role of steering comittee (Loch & al, 2017) ~ political process of legitimacy building.
- Exploratory projects and lineages management / transition between exploration and exploitation / portfolio management
- Theory of agency in EP : design theory, pragmatism…

le cnam

The road ahead

- Cases, cases, cases and more cases are needed to understand their inner functioning, how coordination occurs (Lenfle & Soderlund, 2018), the problem they met, the strategy they use, etc
- Management tools & methods to manage these projects, in particular project evaluation (e.g. CK as sensemaking).
- Gov
proc
- Expl
expl
- Theo

Forthcoming

1. Special issue of *Project Management Journal* on exploratory projectswith C. Midler & M. Hällgren) this year
2. Handbook of projects and innovation (Elsevier, with A. Davies, C. Loch & C. Midler) – end of 2020.

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

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

sylvain.lenfle@lecnam.net
http://www.sylvainlenfle.com

le cnam

REFERENCES

- Brady T, Davies A. 2004. Building Project Capabilities: From Exploratory to Exploitative Learning. *Organization Studies* **25**(9): 1601–1621.
- Davies A, Manning S, Soderlünd J. 2018. When neighboring disciplines fail to learn from each other: The case of innovation and project management research. *Research Policy* **47**(5): 956–979.
- Gillier T, Lenfle S. 2019. Experimenting in the unknown: lessons from the Manhattan project. *European Management Review* **16**(2): 449–469.
- Klein B, Meckling W. 1958. Application of Operations Research to Development Decisions. *Operations Research* **6**(3): 352–363.
- Lenfle S. 2008. Exploration and Project Management. *International Journal of Project Management* **26**(5): 469–478.
- Lenfle S. 2011. The strategy of parallel approaches in projects with unforeseeable uncertainty: the Manhattan case in retrospect. *International Journal of Project Management* **29**(4): 359–373.
- Lenfle S. 2012. Exploration, project evaluation and design theory: a rereading of the Manhattan case. *International Journal of Managing Projects in Business* **5**(3): 486–507.
- Lenfle S. 2014. Toward a genealogy of project management: Sidewinder and the management of exploratory projects. *International Journal of Project Management* **32**(6): 921–931.
- Lenfle S. 2016. Floating in space ? On the strangeness of exploratory projects. *Project Management Journal* **47**(2): 47–61. ***PMJ 2017 Paper of the Year Award***.
- Lenfle S, Loch C. 2010. Lost Roots: How Project Management Came to Emphasize Control Over Flexibility and Novelty. *California Management Review* **53**(1): 32–55.
- Lenfle S, Soderlünd J. 2018. Large-Scale Innovative Projects as Temporary Trading Zones: Toward an Interlanguage Theory. *Organization Studies* Published online august 25**.**
- Loch C, DeMeyer A, Pich M. 2006. *Managing the Unknown. A New Approach to Managing High Uncertainty and Risks in Projects*. John Wiley & Sons, Inc.: Hooboken, New Jersey.
- Sommer S, Loch C, Dong J. 2009. Managing Complexity and Unforseeables Uncertainty in Startup Companies: an Empirical Study. *Organization Science* **20**(1): 118–133.
- Loch C, Mähring M, Sommer S. 2017. Supervising Projects You Don't (Fully) Understand: Lessons For Effective Project Governance By Steering Committees. *California Management Review* **59**(2): 45–67.