PAPERS

Floating in Space? On the Strangeness of Exploratory Projects

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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

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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 (2010), or Hobday, Boddington, and Grantham (2011, 2012), argue that in order to fully grasp the logic of innovation we have to abandon "the traditional view of the firm as a rational, machine-like entity by drawing on the social and creative character of businesses revealed in design thinking" (Hobday et al., 2012, p. 18). The same is true for project management research. Therefore, a switch to design theory (Hatchuel & Weil, 2009) helps us understand the specific logic of exploratory projects. Through this perspective, we show that these types of projects are not floating at all. On the contrary, they follow a logic of expansion of concepts and knowledge that can, in fact, be managed (Lenfle, 2012; Gillier, Hooge, & Piat, 2014). However, to do that, we will have to dig deep in order to understand the design reasoning of the project participants. To study this question, we conduct research in the space industry that lies (with the military) at the origins of modern project management and, as we will see, can be considered an archetype of the dominant model of project management. The emergence of new types of exploratory projects in this context raises important questions that will help us improve our understanding of their specific features.

This article is organized as follows: The second section provides a brief overview of the literature on exploration projects. The third section presents the context of the space industry and research design. The fourth section deals with the emergence of "strange projects" in space telecommunications. In the fifth section, we dig deeper into two archetypal cases of strange projects. The cases are further analyzed in the sixth section in light of design theory. Finally, the seventh section discusses the contributions of this research to the literature on exploration projects.

Literature Review

A rich body of literature exists on the limitations of the rational approach for innovation and the management of exploratory projects (Davies, 2013; Nightingale & Brady, 2011). Since the landmark contribution of Shenhar and Dvir (2007) on the limitations of the "one-size-fits-all" approach to project management, a growing body of research has focused on the management of exploration projects that seek to develop radical innovations. This research streams leads to important results. In this brief review of the literature, we emphasize five principles (extending upon Lenfle, 2008, 2010):

1. In line with the literature on ambidexterity, it is necessary to differentiate the management of exploratory projects from that of more traditional, exploitation-oriented projects (Tushman & O'Reilly, 1996; Christensen, 1997; Burgelman, 2003; Shenhar & Dvir, 2007). Indeed, the blind application of a single, control-oriented method to all projects would surely reduce innovation. This is particularly true of the stage-gate process of project management. Sehti and Iqbal (2008) demonstrate the irrelevance of this process, now widely used, in situations where radical innovations are being made. They show that stage-gate processes lead to what they call "project inflexiblity"—that is, the inability to change the project's goal after initiation. This, they argue, leads ultimately to failure. Thus, the literature on exploration projects emphasizes the need to differentiate between types of projects—for example, by setting up a dedicated and autonomous project team to manage radical innovation, as was done with the famous Skunk Works® invented by Lockheed during World War II. However, the literature on Skunk Works is very sparse, to say the least (Rich & Janos, 1994), and more information is needed on the inner working and governance used for the project (Lenfle, 2014). More recently, Dugan and Gabriel (2013) have offered an inside look at the functioning of the (very) exploratory projects carried out by the Defense Advanced Research Projects Agency (DARPA).
2. Experimentation plays a central role in exploratory projects. This is in line with the literature on innovation management (Van de Ven et al., 1999; Thomke, 2003) and corporate venturing (Frederiksen & Davies, 2008). In particular, the work of Christoph Loch and his colleagues demonstrates the irrelevance of classical risk management when projects are confronted with what they call unforeseeable uncertainties (or "unk unks") (Loch et al., 2006; Sommer, Loch, & Dong, 2009). In such cases, it is impossible to identify the risks. Therefore traditional risk management methods crumble, requiring organizations to identify different managerial strategies to handle these situations—namely, selectionism (experimenting with different situations simultaneously) and learning (trying different solutions one after the other). Instead, organizations may conceptualize projects as "experimental learning processes" during which the goals and the means to reaching them are progressively defined during the course of the project. Such experimental learning strategies actually have older roots (e.g., Brady, Davies, & Nightingale, 2012; Klein & Meckling, 1958), but they had largely been forgotten during the institutionalization of project management (Lenfle & Loch, 2010).
3. Another important principle points to the need to explore the technical and market dimensions of the innovation simultaneously. Gastaldi and Midler (2005) coined the term *concurrent exploration* to define this strategy. The goal is to avoid the symmetrical traps of useless technology and inaccessible needs.
4. The fourth important principle is that the "results" of exploratory projects are different from those of traditional projects. Exploratory projects do not necessarily lead to physical objects. They help to map an "unfamiliar landscape," build new competencies, or explore original concepts. Rather than convergence toward a predefined goal, what is important in exploratory projects is to identify promising concepts that will be developed later (Lenfle, 2012). Reflecting on the journey is

be evaluated from the "products" they deliver as well as the knowledge they create (Iansiti & Clark, 1994).

fundamental, and projects should be evaluated from the "products" they deliver as well as the knowledge they create (Iansiti & Clark, 1994).

5. The last challenge identified in the literature follows from the previous point. Because exploratory projects are "experimental learning processes," it is important to develop managerial methods that will help managers assess the "progress" of the project. Reflection in action (Schön, 1983) is fundamental because the goals will be defined during the project. In particular, the challenge is to manage the expansive nature of these kinds of projects (Gillier et al., 2014). No doubt this constitutes a major question for future research.

These are all important contributions that lay the foundations for a model of exploration project management. However, we are only at the start of research on this question. We have to dig deeper to understand the difficulties encountered by actors in charge of this type of project as they manage in environments that, generally, are ingrained with a rational approach. The implementation of these principles in practice is far from evident. As we will see, these principles require a change in design reasoning. In the next section, we present the context of the research, the space industry, and our methodology.

Context and Research Design

Research Method

This research takes place within the French space agency (known as the Centre National d'Etudes Spatiales, or CNES). In cooperation with partners (industrial firms, research laboratories, public agencies), it is in charge of the definition and implementation of French space policy (both civil and military). Created in 1961, CNES is one of the world's leading space agencies, with an impressive record of success, among which are the Ariane rocket (now the leading launcher in the world), the SPOT lineage of imaging satellites, and the development of operational oceanography, starting with the Topex-Poseidon mission.

Our research unfolds in this context. It is part of a long-term research project that started in 2010 and is still going on. The general goal is to study the strengths and weaknesses of current innovation process at CNES. Indeed, CNES is confronted with important changes in its environment, the most important being the emergence of new competitors such as SpaceX in launchers and the growing demand from diversified stakeholders (government, NGO, and regional and global agencies) to provide "services to society," in particular in the domain of climate monitoring. This is especially true for space telecommunications, which represents by far the biggest market of the space industry (more than 50% in terms of revenue) and in which innovation plays a central role. This was the focus of the third phase of the research.

Data collection was performed during a 12-month period from February 2013 through January 2014. As we will explain below, we focus our research on the portfolio of telecom projects over the past 15 years in the telecommunications industry. Our goal was twofold: first, to understand the logic of the entire portfolio, and second, to study in more detail some projects we consider representative of the ongoing evolutions. To do this, we rely on two types of data:

- We conducted 12 semi-directed interviews with nine people involved in the management of these projects (see the list in Table 1). They belonged to functional department (technical or strategic) or were project managers. The interviews lasted from 1 to over 5 hours. They were tape-recorded and transcribed. Email exchange or follow-up interviews were used when necessary to clarify some points; and
- We had access to CNES annual reports from 1980 to 2012 (with the exception of 1989, 1992, 1993, 1996, and 2009) to cross-check the interviews and get a global picture of the evolution of the CNES telecom strategy over the long run.

Following the paradigm of grounded research (Eisenhardt, 1989; Miles & Huberman, 1994; Yin, 2003), our analysis was built on interview transcripts that were compiled into case studies for the different projects. This process was iterative. The cases were updated after follow-up discussions with respondents. The final research report was reread by key informants and discussed during

	Date	Name	Job Title/Function
1.	04/02/2013	Mr. FP	Head of Telecom Project Department
2.	25/04/2013	Mr. FP	Head of Telecom Project Department
3.	04/07/2013	Mr. FP	Head of Telecom Project Department
4.	05/07/2013	Mrs. CAB	SMILE Project Manager
5.	05/07/2013	Mr. JPT	FLIP Project Manager
6.	02/12/2013	Mr. JPD	AGORA Project Manager
7.	11/12/2013	Mr. HG	Manager—Strategic Planning Department
8.	11/12/2013	Mr. JPA	Head of Telecom Research and Technology Studies
9.	11/12/2013	Mr. DP	Manager—Telecom Research and Technology Studies
10.	11/12/2013	Mr. JS	Senior Technical Expert
11.	13/01/2014	Mr. JPT	FLIP Project Manager
12.	13/01/2014	Mr. CA	Head of Telecom Strategic Planning Department

Table 1: List of interviews.

research meetings. These meetings simultaneously enabled the results presented to be confirmed and the directions taken by the research to be discussed.

The Space Industry as an Archetype of Rational Project Management

The context in which this research takes place is of particular significance for our argument. The space industry constitutes an archetype of the rational approach of project management. We can even argue that the rational model has its roots in the aerospace industry. Most of the current tools of contemporary project management come from the U.S. aerospace sector, be it military (the Department of Defense) or civilian (National Aeronautics and Space Administration, or NASA). Most notably, Stephen Johnson (2002a, 2002b) documented the rise of the system approach in U.S. ballistic missile projects and the transfer of these practices to NASA during the Apollo project (see also Seamans, 2005). This gave birth to a model of project management that emphasizes the control of project execution through a phased approach; the use of managerial tools to control time, cost, risk, and quality; and the setting up of strong project structures to implement this approach.

This method of project management is still dominant in the space industry today. The goal of a typical space project is to design a new object (typically, a new satellite) with requirements that have been defined by the customer (e.g., the government, a private firm, or scientists). And there are good reasons to follow a phased approach, given the following: (1) the technical complexity of the objects, (2) their very high cost (€300 million for a typical satellite), and (3) the irreversibility induced by the launch in space (i.e., if the satellite/launcher fails it is forever lost). The phased approach is thus a wise solution to ensure the quality of the design work and the reliability of the object from the drawing board to the launch pad. In this logic, each stage is able to reach a higher Technology Readiness Level (the famous TRL that originated in the space industry). Projects typically unfold in this manner at CNES; the actors frequently refer to it as "tunnel logic."

As explained in the previous section, the strengths and weaknesses of the rational project management approach are well documented in the literature. The great strength of this type of approach is the application of process control techniques developed for production to the design work. Such processes have been shown to improve control of the convergence toward the predefined goal in terms of cost, quality, and delay. For complex, high-cost projects such as those mentioned above, there probably is no alternative to the rational project management approach. But problems arise when this approach is blindly applied to all kinds of projects (see Sehti and Iqbal's [2008] discussion of project inflexibility in the previous section). At CNES, the problem appeared with the emergence of "strange" projects at the end of the 1990s.

The Emergence of "Strange Projects" in Space Telecommunications

Our interest in space telecommunications was triggered by a presentation by Mr. FP, the head of the navigation and telecommunications projects at CNES, during a one-day workshop dedicated to innovation management in the agency. During his presentation, Mr. FP explained that in the telecommunications sector, CNES was increasingly encountering what he calls "strange projects." In order to illustrate his idea, he presented a slide with Hieronymus Bosch's famous painting, *The Garden of Earthly Delights* (see Figure 1). He used the unexpected and confronting nature of the elements in the painting as a metaphor for his perception of a mismatch between the "strange projects" and the phased approach that typify project management at CNES. Indeed, the projects he supervised looked nothing like those defined in classical project management frameworks: The goals were not clear in the beginning, the projects worked on new concepts and not necessarily with objects, it was hard to define deadlines, and they were frequently changing.

The Evolution of Space Telecommunications in the 1990s

To understand these strange projects, it is necessary to take a historical detour. During the 1990s, dramatic changes

Figure 1: Bosch's *The Garden of Earthly Delights* (circa 1503–1504, Prado Museum).

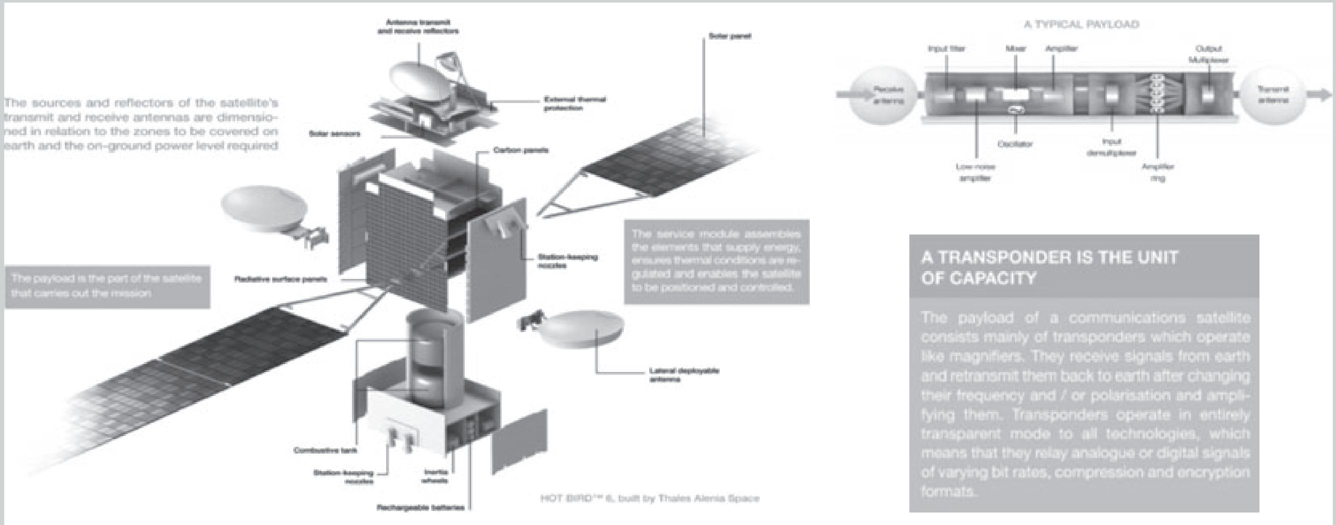

Figure 2: What is a telecom satellite? (Source: *Eutelsat, A Guide to Communication Satellites*, July 2011).

occurred in space telecommunications, which constitutes, by far, the most important application of space technologies.[1] Four points are worth noting:

- First, the deregulation of the 1990s transformed the space telecommunications sector from a monopoly-dominated industry to a highly competitive one, dominated by privates firms (primarily from the United States but also including European firms). The major consequence of this shift for CNES was the loss of its traditional customer: France Telecom. The shift also raised the question of what role a public agency should play in this context.
- Second, innovation plays a central role for firms in maintaining a competitive position, particularly in Europe, which does not benefit from large contracts from the U.S. Department of Defense. If a firm fails to develop a technical innovation, it could be excluded from the market. In space, telecoms innovation has been, at least up until now, clearly sustaining (Christensen, 1997). Over the past 40 years, the logic has been to regularly increase the power of satellites (from 40W in the 1960s to 20kW in the 2000s) to satisfy new uses (typically, TV diffusion, communications, and so forth). Doing this, however, raises huge technical challenges (such as more precise machining of antennas, complex management of radio frequencies, heavier and bigger satellites, and so on) and explains the relative slowness of the innovation process: For example, it took 10 years to change from C & KU to KA band frequency, which is more useful for broadband Internet applications. Indeed, given the high costs of a telecom satellite, operators are reluctant to invest in a technology with fuzzy uses and unclear market potential.
- Third, the space telecom ecosystem is extremely complex, with at least three different levels: (1) the space segment (that is, satellites and their control centers); (2) the ground segment (telecom networks and the associated devices); and (3) regulating agencies (the International Telecom Union for the management of radio frequencies). Consequently, each innovation proposed by space telecoms must be compatible with all the elements of the ecosystem. Furthermore, some blocking points may appear in "odd places"; far from CNES's core competencies, these blocking points typically involve mobile devices that must be able to receive space satellite signals.
- Last, but not least, the problems are complicated by the mismatch of temporality between space telecoms, with their long development cycle, and ground telecoms, which have very short development cycles. Although the development cycle of satellites themselves is quite short (approximately 36 months), satellites are designed for 15 years of life in space. Therefore, given the 10 years of research necessary to develop a new technology, the challenge, as one CNES engineer explained, is to "design now a satellite which will still be useful in 25 years." (interview with Mr. CA)

A New Logic at CNES: From Satellite Design to Concept Exploration

Historically, as is clear from the interviews and annual reports we used for our research, the main role of CNES was to act as the chief designer of satellites. These included operational satellites (such as Telecom 1, launched in 1984) or technical demonstrators (prototypes that incorporate the latest technologies to demonstrate their usefulness and feasibility) (see Figure 2). The focus on hardware design has ended now as a result of

[1]Satellites are mainly used for TV diffusion (TVs represent 68.7% of the revenues of Eutelsat, the main European operator), but they are also used for communications or to bring Internet access to remote areas.

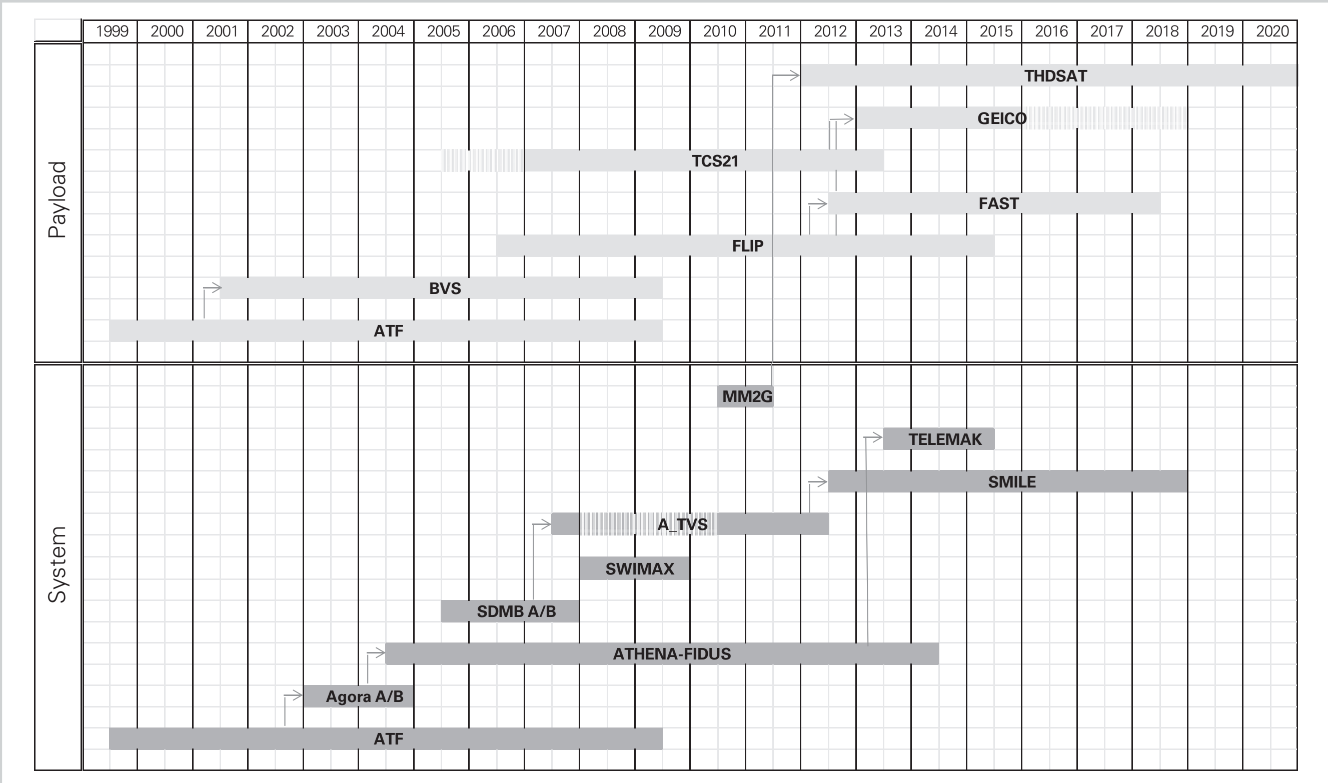

Figure 3: The evolution of CNES project portfolio on telecoms for the general public (1999–2020).

the growing role of private firms in satellite design and because demonstrators are now considered too costly and risky. Indeed, the last of them, called Stentor, exploded with Ariane 5, 7 minutes after launch in December 2002 after a 10-year development process and more than €300 million spent. This accident had major consequences for CNES. Its role has evolved from chief designer to a more ambiguous position of "support for industry competitiveness," as explained in CNES reports. CNES's role now encompasses all the activities needed to help the space telecommunications industry maintain its technological potential in a context where intense competition limits private firms' R&D spending. This evolution, which is actually a rather broad and fuzzy mission, raises major questions for an organization that was originally structured to design satellites. Indeed, this triggers a fundamental change in the design process, which shifts from designing objects (satellites) to exploring concepts that may be interesting to support competitiveness. The immediate consequence of this evolution has been a radical change in the content of CNES's projects, which have shifted from hardware design to concept exploration and/or competence development. It is beyond the scope of this article to describe in detail all the telecommunications projects launched by CNES during this period. It is nevertheless useful to provide an overview of the portfolio projects (Figure 3) and their content (Table 2).

Figure 3 and Table 2 show that the content of many projects has shifted from hardware design to concept exploration and that the exploration focuses simultaneously on satellite payload and the telecom system as a whole. This change has led CNES to gradually map the innovation domain,[2] develop its competencies, and build legitimacy for the industry as a whole. It is possible to identify three lineages of projects—that is, a succession of projects focused on the same applications:

1. The first lineage is oriented around the development of competencies and the study of generic questions (such as flexibility). These projects allow CNES to partner with industrial firms. This category includes the ATF, BV, FLIP, FAST, and GEICO projects.
2. A second lineage is centered on space telecommunications for Internet access in remote areas. These include the AGORA, ATHENA, MM2G, and THD-SAT projects.
3. The third lineage focuses on mobile broadband access and includes the SDMB, A-TVS, SWIMAX, and SMILE projects.

Our data reveal that this rich and fruitful exploration has been made possible by the (rare) convergence of four factors:

[2]Ben Mahmoud-Jouini, Charue-Duboc, and Fourcade (2007) discuss a similar case of innovation domain management at Valeo, a French leading automotive supplier.

Project Name	Goals	Main Results
ATF (1999–2004)	1. Develop CNES competencies as a system architect 2. Explore new capabilities that could be useful for the industry	Design of the system transmission bench (abbreviated BST in French) to simulate and test the behavior and performance of the emission/reception chain that constitutes the heart of the payload of a telecom satellite
BV (2002–2007)	Continuation of ATF; design of a validation bench to validate new payload on the ground (including whose interaction with the ground segment)	Validation bench used by partner to test engineering qualification models, development of competencies, and solutions later used on the military satellite Syracuse
AGORA (2002–2004)	Develop a satellite to provide high-speed Internet access in remote areas not covered by ground networks	Feasibility study and A-phase completed with industrial partners; ended in 2004: B-phase never launched because of the reluctance of telecom operators to fund the project; launch of a new study for civilian application (THD-Sat) in 2010, launch planned in 2018
Athena-Fidus (2005–2014)	Develop a satellite to provide high-speed Internet access in remote areas not covered by ground networks for the armed forces (French and Italian)	Follow-up of AGORA; satellite developed by Thales and Telespazio and launched in February 2014
FLIP (2006–2014)	FLexible Innovative Payload: Study the impact of flexibility on satellite design	New products for the payload, prototypes, new engineering models, competencies (see below)
SMILE (2012–2017)	Satellite Mobile Innovation Laboratory and Engineering. Follow-up of several disrupted A-phase (SDMB, A-TVS, SWIMAX); study the potential and implications of space technologies in mobile communication	In progress (see below)

Table 2: Main projects of CNES in telecommunications since 1999.

- A guiding concept for exploration: space telecom for the general public;
- Stable management during this period;
- Stability of the design teams that frequently work on several projects; and
- The control that the project department has had over resources, allowing for fast resource reallocation according to the unfolding needs of the different projects

Though this overview is useful for understanding the dynamic of CNES's portfolio of projects, it does not tell us how this new type of strange projects unfolds, the problems that are raised by them, and how they are managed. That is the topic of the next section, which focuses specifically on two projects: FLIP and SMILE.

Archetypes of Strange Projects: FLIP and SMILE

In this section, we focus on two projects that represent archetypes of strangeness. Indeed they are not developing an object but rather exploring new concepts for future telecommunications systems. Two areas were favored: (1) innovative payloads, the importance of which was pointed out by the ATF and BV projects, and (2) mobile communication using satellites, which has been a recurring subject since 2004.

FLIP (FLexible Innovative Payload)

Concerning payload, the main question for satellite operators has been the development of flexible payloads in the foreseeable future. Indeed, until now, a telecom satellite has basically been a transmitter that receives a signal from the ground and broadcasts it over a predefined territory. This logic seemed to reach its limit in the mid-2000s. The long life of satellites in space (more than 10 years) and the saturation of some bandwidths (such as the Ku band) make flexibility an important concern for telecom operators. Operators would like to be able to modify the use of a satellite after launch. This was the central question studied by the Flexible Innovative Payload project (FLIP, 2006–2014), launched in 2006 by CNES. However, at the beginning of the project, flexibility was only a loosely defined concept. Nobody knew what impact flexibility would have on the payload's design. The operators wanted to reallocate frequencies, change the coverage area, and modify the power of the satellite, but it was unclear how to best do this, especially as it needs to be done without raising the costs of the payload. With this in mind, the FLIP team started the project. They had requirements defined by the strategic planning department, as well as "research and technology" studies that identified some promising new technologies. Because the goal was to complete the project quickly, the team decided to start the project directly with the detailed design (known as B-phase in the CNES project process—that is, the proof of concept is already done). However, they quickly became dissatisfied with the initial requirements and the technologies proposed by the Research and Technology Department, which seemed promising but were not ready for development. Therefore, in 2007, they decided to start a new round of interviews with operators to get a better understanding of their needs. This process lasted 18 months and led the team to identify 27 types of missions, which they grouped into seven different families (including, for example, "flexibility and resource allocation"). Based on this new knowledge of the requirements, the team explored potential technical solutions such as antennas, transponders, new architectures, and onboard chips. It is impossible here to follow in detail the complex unfolding of the FLIP project. Indeed, the goal of the project was not to design a product but rather to explore different solutions for flexibility and to map the design space. Therefore, we present two examples that are typical of the project's logic:

- Concerning the transponder (see Figure 2), the team initially identified four solutions but none of them was satisfying in terms of performance and/or cost. During the 2009 project review, the team decided to split the work into two parts: (1) the development of a mature solution for the short-term needs of a customer, and (2) the exploration of ways to satisfy the 27 missions while maintaining the highest level of commonality between the solutions in order to avoid overdesign and additional costs. This ultimately gave birth to three types of products that are able to cover the full range of missions. One of these is under development and is planned to fly in two years.
- Concerning the antennas, a central component for enhancing flexibility, the project team decided to reconsider the way antennas were designed. The dominant design was mechanical, with an extremely precisely molded antenna designed to cover a predefined area that was completely nonflexible. To overcome these limitations, the team explored electro-mechanical designs. Here again, different solutions were studied, some of them not appropriate for short-term applications but still worth exploring because they allowed the development of fundamental competencies for future antennas. This is the case of the X-antenna: It was too heavy and too expensive but it led to the development in France of new production processes that, until now, had been mastered by only one U.S. firm.

Finally, FLIP give birth to multiple results: It delivered a product for short-term needs, developed new process technologies, mapped customer needs, built competencies for payload design, created generic products, and new chips that will be developed by another project (FAST, 2012–2017).

SMILE (Satellite Mobile Innovation Laboratory and Engineering)

The SMILE project (2012–2017), like FLIP, is an archetype of the strange projects described previously. It is part of a larger reflection on the role that space technologies could play in the rapidly expanding domain of mobile communications. Although satellite phones have existed for 15 years, they remain part of a small market niche. The development of space technologies toward mass-market applications is therefore a strategic question first studied at CNES by the SDMB project (2004–2006). The goal was to develop a solution to broadcast multimedia content directly to mobile phones in remote areas. The feasibility study conducted by CNES was so promising that Alcatel Alenia Space (AAS),[3] a leading aerospace manufacturer, joined the project. The B-phase was thus co-financed by CNES and AAS. It led to the creation of a joint venture between the operator Eutelsat and AAS to develop a new payload. Launched on the W2A satellite in 2009, it unfortunately suffered from technical problems after launch that will limit the satellite to experimentations. The project nonetheless demonstrated space technology's interest in multimedia broadcast on mobile phones and the competencies of CNES within this domain. Following SDMB, CNES has launched several feasibility studies on various aspects of the question (ATVS: TV on mobile, SWIMAX: Internet access, technical standards), but none of them has led to a B-phase. Considering the potential of these applications, however, CNES launched the SMILE project in 2012 to continue the exploration. Given the relentless evolution of the fast-paced telecom industry, the goal was twofold: first, to ensure that space technologies remain a solution for the industry (indeed, if regulators decided that the frequencies dedicated to space telecoms are reallocated to other uses, then the space industry would be ruled out of the ecosystem), and second, to study technological solutions in order to be ready if a window of opportunity opens. Indeed once a user is convinced of the relevance of a space solution, firms have to act very quickly. Because time is of the essence, CNES must invest upstream to be able to satisfy the short development time of telecom satellites (36 months). These two objectives explain why SMILE consists of four parts:

1. Regulations—in other words, lobbying to keep the S-band for the space industry;
2. Standardization in order to be able to insert the space solution in future telecom standards that suppose both technical work and lobbying;
3. Collaborative projects with telecom operators, component providers, and so forth to demonstrate the relevance of space technologies by building prototypes and organizing collaborative events with potential partners; and
4. Competence building: developing the necessary competencies and design tools to accelerate the design process if a window of opportunity opens.

SMILE, therefore, which is an ongoing project, includes plans to propose a roadmap in 2014 while simultaneously improving CNES's competencies on mobile communication (test bench, simulation software, engineering models, and so on).

Analysis: The Logic of Strange Projects—A Design Perspective

To better understand the logic of exploratory projects, we dig deeper into the

[3]AAS is now called Thales Alenia Space.

FLIP project. After the first round of interviews, we organized a second interview with the project manager. Our goal was to clarify some technical questions and, in so doing, to reconstitute the design reasoning of the project. The interesting point about FLIP is that, as presented earlier, it started out as a "normal" project. Customer needs were identified and R&T had developed promising new technologies. The team was thus confident and, given the urgency, decided to start directly in the B-phase with the detailed design. However, as the project manager said, "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. Indeed, we needed to understand, beyond the concept of flexibility, what were the missions they were interested in. We wanted to identify all the potential missions with them. This proved to be a long and complex dialogue because they are reluctant to unveil their strategy. It took us one year and a half and we ended with 27 missions grouped in seven types. Hence, we realize that we know the need conceptually but we didn't understand what it means operationally" (Interview with FLIP's project manager, July 13, 2014). Starting from this new understanding, the project unfolded using a completely different logic. What the team actually did was a "kind of 0/A phase" again. They had to abandon the proposed solution and design new ones. As the project manager explained, "Around 50% of R&T studies were useless so we had to do upstream engineering studies again" (Interview). The project then faced a complex task because the team had to explore the widest possible range of technical solutions and needs, while simultaneously satisfying the more short-term demands of the customers. This led to an original strategy. In 2009, four solutions (let's call them A, B, C, D) were explored for the transponder. The project manager recalled:

> "The A solution already exists, B has to be developed, C is a legacy of an older solution and is not a priority, D is the more complex because it is flexible and has challenging requirements in terms of performance. The project review for D arrives and we realize that what we were designing does not satisfy the requirements. The architecture was not adapted; it cannot handle the different situations identified with the operators. However, it was good enough for some configurations (called Ku/Ku). So we decided to go on and develop this Ku/Ku solution that would allow a little flexibility and give us time explore the other solutions. The Ku/Ku solution will do the spadework on flexibility and moreover could be integrated in a classical payload. By the way, it has already been sold to IntelSat[4] and will fly in two years. So we were right to do it. But simultaneously we reconsidered from scratch the different architecture for a flexible payload able to take into account the different missions. It's a very complex task. There is huge number of combinations in term of bands (S/Ku, S/Ka, Ku/Fi, C/Fi, and so on). So you have to design some commonality, otherwise you will not be competitive. It took us one year to understand the problem and map the different solutions. ... And finally we've designed three products that cover all the different architectures for the transponders. Now we have three EQMs (Engineering Qualification Models). They can be combined to cover the different missions and they are in the final stage of development at our industrial partners." (Interview with FLIP's project manager, January 13, 2014)

Note that this type of reasoning is not limited to the transponder but is applied to all the components. Antennas, for example, are core elements of a telecom satellite that play a central role in its flexibility (e.g., the possibility to broadcast the signal in different geographical areas). Here is what FLIP's project manager said about the design of the X-antenna mentioned earlier: "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 3 dB and it's a bit expensive. But the benefit is that it is a technology driver for two-process technology that, until now, had been mastered only by the United States. Now European firms have also mastered these technologies. We've designed it in this perspective. And we've been to the engineering model. We've designed a prototype that demonstrates the feasibility and that can be tested. We didn't want to limit ourselves to R&T. Look at it: It's beautiful [showing the X-antenna on his computer]! We wanted to prove that we know how to build it, to force the industrial partner to build a working version. These process technologies are so interesting—for example, to save weight, we had to show that the main critical problems have been solved. But we know from the beginning that the X-antenna will not be chosen in the short run. That said, operators and other projects at CNES are beginning to look at it. And the way we designed it makes it compatible for different applications" (Interview with FLIP's project manager, January 13, 2014).

Because this constitutes the heart of our argument, it could be useful to put these quotations in perspective. To do this, we rely on recent advances in design theory. This will help us show that what happens in this case is a fundamental shift in the logic of project management that, historically, has its roots in decision theory (Lenfle & Loch, 2010; Söderlund, 2011). However, as we suggested earlier, understanding innovation should lead us to abandon the rational, decision-based view of firms and projects. More precisely, we will rely on the C-K theory of design (Hatchuel & Weil, 2009; see also the Appendix) to analyze the design reasoning of the FLIP project, which, in our view, is typical of exploratory projects. What is fascinating here is that the first step in the design process is typical of the dominant model of project management: We begin with a description of the customers' need from the strategic department and technical "off-the-shelf" solutions proposed by R&T. On

[4]IntelSat is a leading telecom satellite operator.

this basis, given the urgency, the project quickly starts to develop two different solutions for the two main missions (see Figure 4). The hypothesis is that we have the concept and that the K-base is sufficient to start development.

However, what we see is that this logic does not hold for exploratory projects. Indeed, in exploratory projects case, one can neither make the assumption that the goal of the project is clearly defined beforehand, nor that the knowledge base is sufficient. Flexibility is actually a general concept that will be defined during the course of the project. Therefore, the logic of the project is fundamentally different from that of traditional project management. The concept of flexibility questions the very nature of the satellite and modifies the design rules in use, as illustrated by the FLIP antenna case. The project quickly becomes trapped in a dead end and adopts a logic of expansion (Hatchuel, 2002), which is typical of exploratory projects (Gillier et al., 2014; Lenfle, 2012). We summarize this expansive logic in three steps, shown in the gray sections of Figure 5.

- **Step 1**: This step involves the reopening of the initial mission concept that leads to the identification of 27 different missions grouped into seven families.[5] Work takes place in parallel on four technical solutions that seem promising.
- **Step 2**: These solutions appear to be unsatisfying given the great variety of the different missions and the constraint of commonality. However, one of the solutions, called Ku/Ku, is developed as a first step to satisfy a customer's short-term need. Meanwhile, the team explores the different solutions, which leads them to map the design space.
- **Step 3**: At the end of the project they develop three products to the EQM stage, build prototypes to demonstrate the feasibility of some of the technology, and enhance their competencies. This leads them to completely revise the design models of the payload to make it flexible.

What we observe here is a double expansion (see the gray parts of Figure 5) of both concept and knowledge that, we believe, constitutes a fundamental feature of exploratory projects (see Lenfle, 2012). What makes the FLIP project an exemplary case is that it brings together many of the characteristics of exploratory projects (Lenfle, 2008):

- Difficulty in specifying the result ex ante;
- A questioning of the stage-gate process: What we see here is a constant back-and-forth between stages, sudden acceleration, and stage overlapping;
- Simultaneous management of different temporality—both short-run

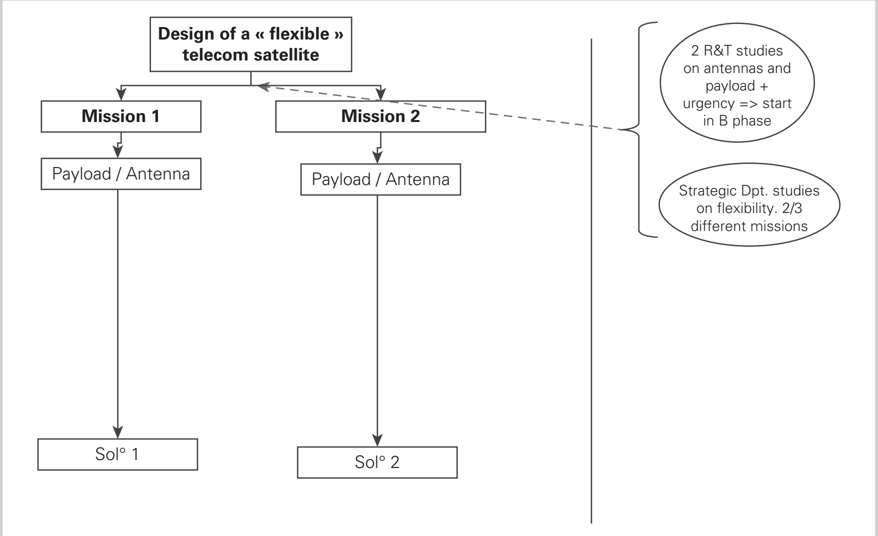

Figure 4: The beginning stage in traditional project management.

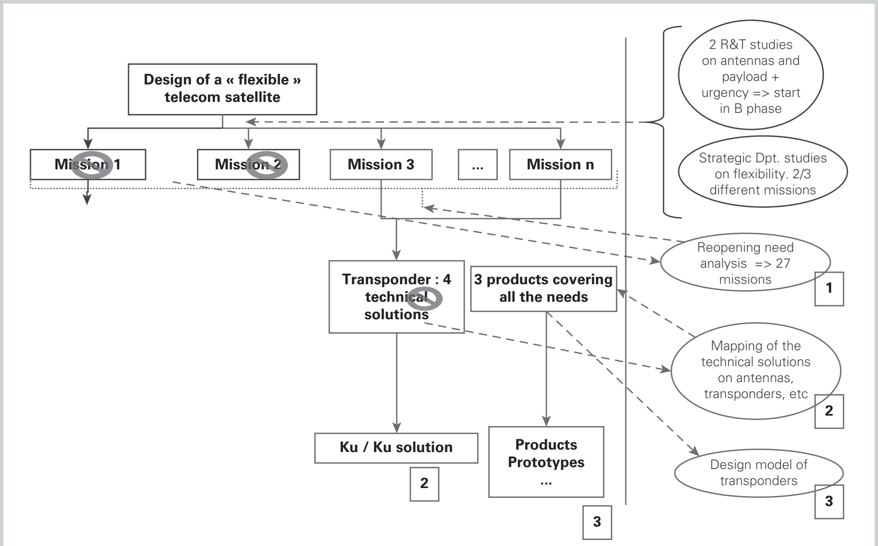

Figure 5: The expansion logic of exploratory projects.

[5]Note that the work with the operators to redefine the missions underlines the fundamental role of stakeholder management in projects (on this question, see Eskerod, Huemann, & Savage, 2015).

development and long-term exploration. This is a constant tension in exploratory projects;
- Development of new design rules instead of relying on existing rules, challenging the dominant design; and
- The fact that the "result" of the project is more complex than in a traditional product development project. Rather than a product, the result is generally one of the following:
  - qualified products (EQM)
  - prototypes that demonstrate the usefulness and feasibility of a solution
  - mapping of the design space defined by the concept of flexibility
  - new design models that can be reused for future projects
  - new competencies, as exemplified by the X-antenna.

We note that this analysis draws a picture of very altruistic projects. Fundamentally, these projects work to prepare for other, more development-oriented projects. They do the spadework that leads to lineages of future projects that builds on their exploration (Le Masson et al., 2010; Maniak, Midler, Lenfle, & Le Pellec-Dairon, 2014; Midler, 2013). Thus, as the project manager of FLIP explains: "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 focus on the components they need, and that's all. We try to break this logic that consists of strictly following the requirements." He insists particularly on the importance of the development of 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" (Interview with FLIP project manager, January 13, 2014). We see here how, in contrast to the rational approach, such projects help map an "unfamiliar landscapes" (McGrath, 2001) and build new competencies, instead of mainly using what already exists to reach a clearly defined goal. Therefore, the project mode seems to be an interesting way to structure exploration processes. We discuss this point in the next section.

Discussion: Structuring Exploration Through Projects

In the end, what we have here is a new type of project in terms of both logic and results. These exploratory projects respond to the growing and strategic role of innovation-based competition that has emerged during the past two decades. As we have seen, relying on projects to manage exploration assumes the development of specific management principles. These have already been identified in the literature (Loch et al., 2006; Lenfle, 2008; Davies, 2013; Dugan & Gabriel, 2013). This research goes one step further by making explicit the design reasoning that underlies their unfolding. This example demonstrates that their logic differs radically from that of the dominant model of project management: Goals are progressively defined during the project, new knowledge has to be developed, results are multiple, stages are overlapping, and temporality is complex. Based on all these differences, one might wonder if we can still talk about projects. Indeed, when compared with traditional projects, they really look like Bosch's painting: bizarre, undefined, hard to understand, and without clear meaning. And during our interviews, project managers complained about the difficulty of managing these projects within CNES's project management processes. As the project manager of the SMILE project said, "You can only be the dunce. Even building a work plan is complicated. I found myself at a kickoff meeting where 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 in an acceptable form." The problem is all the more complex because "we speak to people who are not from telecom and ignore what is at stake" (Interview, July 5, 2013). The head of the project department, who participates in all project reviews, confirmed this problem when he recognized that "SMILE is a fuzzy object; people outside the team have problems understanding what it is about" (Interview with Mr. FP, April 25, 2013). The risk, in this case, is that the blind application of the usual project management process leads the project into a dead end. As explained by the FLIP project manager, "There is an important risk of developing the wrong product because the schedule target is too stringent" (Interview with FLIP project manager, January 13, 2014). Until now, projects needed to circumvent the process mainly by putting on "makeup." One of the project managers explained, "In order to survive, the only solution is to dress the project as it is expected to be, with a red nose if you need a red nose, white shoes, yellow tie and so on."

This workaround strategy, frequently observed in innovation management, should, in our view, be a last resort. The challenge for firms, and for project management research, is to recognize the specificities of exploratory projects and to differentiate the management processes accordingly. Indeed, the CNES case and others in the literature demonstrate that the project form is relevant for managing exploration. Here again, the interviews with the project managers are precious. They point to the three fundamental contributions of the project form of organizing: its orientation toward practical goals, the time pacing of exploration, and the creation of a community.

Orientation toward Practical Goals

Concerning this first point, the project managers unanimously recognize that being organized as a project is fundamental and different from both R&T and development projects. As summarized by Mr. FP, head of project department "We are not exploring for the sake of exploring." The project manager of FLIP refers to this idea as "being pragmatic:" "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 serve to do something tomorrow, whereas sometimes in R&T you search everywhere. Here, we have constraints of cost, delay, and feasibility." We find the same ideas when the SMILE project manager explains that these projects "are not like departments that put in place tools for themselves; we work for the future A- or B-phase. We always think globally. And we have to justify what we do at each project review" (Interview with the SMILE project manager, July 5, 2013). Indeed, these kinds of projects are not at all "floating," as they might appear at first glance. They have to deal with the classical constraints of project management: cost, quality, and time. Project reviews help discuss what happens, identify the relevant tracks to be explored, and define the next steps. This corresponds to the experimental learning process proposed by Loch et al. (2006). Of course, as we have seen, the requirements are more the result of the project than the beginning (see McLean, 1971), but this does not mean that exploratory projects are not projects, only that they are just that: exploratory. Moreover, as we explained in the previous section, we are now able to characterize more clearly these results, which could be products, prototypes, a mapping of the innovation field, new design models, or new competencies.

Pacing the Exploration

An important difficulty of exploration is its apparent endlessness. When is it finished? How can people give a rhythm to the exploration process? On these two questions, project-based organizing provides two important answers: project reviews and time limits. On the first point, the project managers we interviewed recognized the fundamental role of project review. The FLIP project manager explained: "Project reviews are a huge added value compared to individual action. The project has to be justified collectively to the outside world. It gives visibility, it gives deadlines, it gives meaning" (Interview with FLIP project manager, July 5, 2013). This demonstrates that these kinds of projects are not floating. They are carefully managed and the project review plays a central role in this management. Of course, these reviews differ from a traditional review. In the rational approach, reviews serve to check convergence toward the objective. In exploratory projects, they are an instance of sensemaking (Loch et al., 2006; Lenfle, 2011), or a moment of reflection in action (Schön, 1983) during which results are collectively discussed and the course of action decided.

Moreover, projects are temporary. All projects have an end. However, that end is not necessarily the realization of an object, which means the end can be hard to identify. When is it time to stop exploration? From the cases we researched, we can identify three (non-exclusive) criteria:

- The budget is exhausted;
- The project has reached the end date[6]; and
- The innovation field has been sufficiently studied.

For example, we find a combination of these three criteria in the FLIP case. When asked when the project is finished, the project manager explained: "When we arrive at the end of the budget and the date. For FLIP, the end is planned for 2014: Budgets will be exhausted; EQM will be validated … even if this does not prevent us to think about their evolution. But globally that's it—either we haven't any money, or we are out of ideas, or we are out of the scope and it's forbidden. For example, in FLIP, the ground segment is out of the scope. The concept of a generic solution is also out of the scope, but will be studied in another project named GEICO. But today, on flexibility, we have what we need" (Interview, July 5, 2013). We think this statement is very important. It points to two criteria to evaluate, and therefore manage, in the unfolding of exploratory projects: (1) a kind of theoretical saturation (we have what we need), with the same meaning as used in grounded theory (Glaser & Strauss, 1967), and (2) "expandability"—the ability of the project to generate new explorations (Hatchuel, 2002; Gillier et al., 2014). There is no doubt that further research is needed in this area.

Building a Structured Community for Exploration

Our interviews reveal that the project perspective also plays a fundamental role in the structuring of the exploration process. Indeed, one of the risks of exploration is remaining spread out in different parts of the organization with only loose coordination, to say the least (this is a well-identified problem in innovation management—see, for example, Dougherty & Hardy, 1996). Setting up a project helps avoid this trap. The SMILE and FLIP project managers again converged on this question. For the SMILE project manager, "each department considered separately would not have any interest in exploring. Here, to combine our forces creates a critical mass. And people in the departments are happy with this; it creates contact, it creates competition, challenge. We are also linked by the news, our ability to listen to what happens in a world that changes very quickly. I found that we need to see each other; there are also human stakes, the feeling to get things moving together. 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's spirit" (Interview, July 5, 2013).

[6] Note that we find similar criteria at DARPA (Dugan & Gabriel, 2013), a place known for managing breakthrough projects, where the end date is defined at the start of the project and cannot exceed five years.

The FLIP project manager confirms this sentiment: “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 is something of development; we consider the interfaces with the entire system” (Interview, July 5, 2013). These statements demonstrate the power of projects to create momentum and to build links between scattered people, links that underpin the success of the projects. This typical characteristic of projects (see, for example, Clark & Wheelwright, 1992; De Marco & Lister, 1999) is all the more important in highly uncertain contexts.

Conclusion

We started this article by noting the growing role of exploratory projects in today's innovation-based competitive environment and the limitations of the dominant model of project management for managing such projects. This gave rise to a growing body of research on these highly uncertain projects. However, we pointed to our lack of knowledge on the practices of managers of exploration projects and the difficulty they encounter. Indeed, compared with the dominant model of project management, exploratory projects look strange because there are ambiguous goals and no requirements, the projects work on new concepts and not necessarily on objects, it is hard to define deadlines, and the risks are unknown. In other words, they seem to be “floating.” Our purpose has been to study the management of these “strange” exploratory projects within the context of the space industry that has, historically, served as an archetype of the rational approach to project management. This led us to conduct interviews with actors in charge of the kinds of projects that are now emerging in the space industry, particularly in the telecommunications sector.

In so doing, we have made three contributions. First, the case studies provide rich material on how exploratory projects unfold; their management; and the problems encountered by the actors. This information is still missing in the literature. Second, we demonstrate that exploratory projects are not at all floating. They may appear so, if they are viewed through the rational model. We show that, on the contrary, these projects are carefully managed and actually obey a different logic. At a conceptual level, they correspond to the experimental learning process proposed by Loch et al. (2006) in which goals and the means to reaching them are progressively identified over the course of the project. Design theory helps us clarify the expansive logic of these projects, which are exploring both new concepts and new knowledge. We are thus able to characterize how they unfold (double expansion in concept and knowledge), specify their results, and identify promising criteria (saturation and expandability) for their evaluation (see also Gillier et al., 2014). Third, we demonstrate that exploratory projects constitute a powerful tool for structuring the potentially very fuzzy processes of exploration. They are oriented toward goals, they help pace exploration, they provide opportunities for sensemaking, and they foster coordination between different disciplines that, otherwise, would remain scattered throughout an organization.

We believe that what is at stake here is important for the evolution of project management research and practice. Indeed, we have to reconsider the concept of the project itself that, for too long, has been equated with the rational model. This perspective has hindered our ability to think about other types of project logic. As a result, project managers of exploratory projects have considered themselves the “dunce” and their supervisors talk of “strange” projects. Given the role of innovation in today's competitive environment, it is all the more important to formalize and circulate a relevant model of exploratory project management.

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Appendix

A Brief Introduction to C-K Theory of Design

The C-K theory of design describes design reasoning as the interaction between two spaces: the concept space C and the knowledge space K. Design begins with an initial concept, a proposition that is neither true nor false—that is, it is undecidable in the K space. The concept, let us say of a "flying boat," cannot initially be said to be feasible or unfeasible, marketable or not.

The design process consists of refining and expanding the concept by adding attributes that come from the knowledge space (flying boats have sails or motors, hulls, foils or wings, and so forth). The process can also lead to the production of new knowledge that may be used in the design process—for example, as a result of an experiment conducted to understand the effect of foils on the boat's behavior. The initial concept set is thus partitioned into several subsets.

The process unfolds until one refined concept is sufficiently specified to be considered true by the designer. At that point, the concept becomes a piece of knowledge (hence, the notion of a "C-K conjunction"). The generic structure of design reasoning is presented in Figure A1. For a complete presentation of C/K theory, its roots, and its applications, see Hatchuel and Weil (2009), Le Masson et al. (2010), and Agogué, Hooge, Arnoux, and Brown (2014).

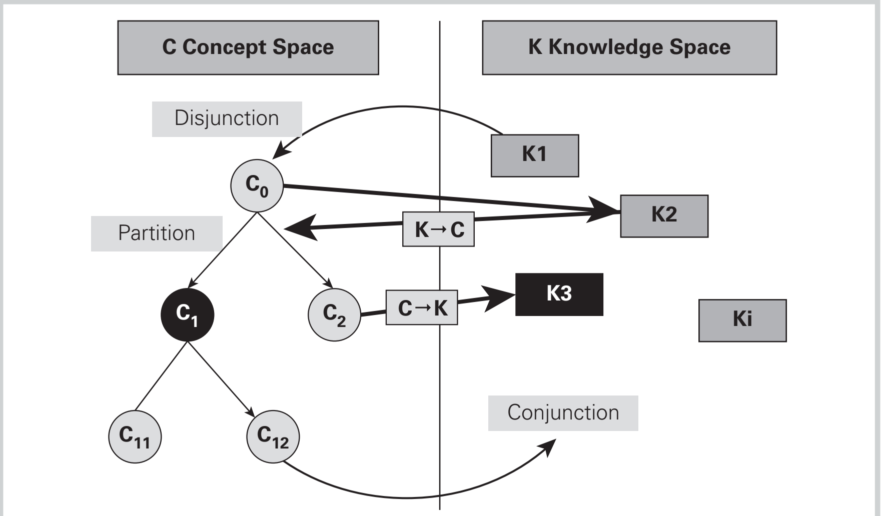

Figure A1: The generic pattern of design reasoning in the C-K design theory (Hatchuel & Weil, 2009).