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Submitted to the in-between event "Innovation and the (re)foundation of management" – SIG Innovation EURAM Mines ParisTech, 9-10 November 2015

Thomas GILLIER
Grenoble Ecole de Management
thomas.gillier[at]grenoble-em.com

Sylvain LENFLE
Université de Cergy-Pontoise
i3-CRG, Ecole polytechnique, CNRS, Université Paris-Saclay
sylvain.lenfle[at]polytechnique.edu

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EXPERIMENTING IN THE UNKNOWN : LESSONS FROM THE MANHATTAN PROJECT

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Thomas GILLIER
Professeur assistant, Grenoble Ecole de Management

Sylvain LENFLE
Maître de conférences, Université de Cergy-Pontoise
Chercheur, Centre de Recherche en Gestion, Ecole polytechnique

Abstract

Experimentation is of paramount importance in innovation. On this topic, the seminal book of Prof. Thomke entitled "experimentation matters" has received a warm welcome from innovation scholars and practitioners. Unfortunately, the companies still have major difficulties to organize experimentations in situation of high uncertainty. This research aims at verifying the validity of Thomke's principles in such situation. For doing this, this research studies the exemplary experimentations carried out by the Manhattan Project to create the first implosion type fission bomb. Our findings show that the Thomke's organizational principles by are ill adapted to manage experiment in the unknown. In particular, the lack of theoretical knowledge, the crisis of the scientific instruments and the absence of pre-established organizations are critical aspects. Finally, research avenues for experimenting in the unknown are formulated.

Keywords : *experimentation, radical innovation, exploration, project, unknown*

Résumé

L'expérimentation joue un rôle fondamental dans le processus d'innovation. En la matière l'ouvrage de S. Thomke, *Experimentation matters* (2003) constitue une référence incontournable. Cette recherche étudie la validité des principes proposés par Thomke en situation de grande incertitude. Pour ce faire ce travail analyse le cas du projet Manhattan qui, pendant la seconde guerre mondiale, abouti à la conception des premières bombes atomiques. L'exemple de la conception de la bombe implosion nous permet de montrer les limites des principes de Thomke en situation d'expérimentation dans l'inconnu. Le manque de connaissances théoriques sur les processus à l'œuvre, l'absence de moyens d'expérimentation et d'organisation pré-existante nous amène à questionner le modèle de Thomke et à proposer des pistes de recherches pour l'expérimentation dans l'inconnu.

Mots clés : e*xpérimentation, innovation radicale, exploration, inconnu*.

INTRODUCTION

The topic of experimentation occupies a central place in the innovation and management literature. Trial-and-learning experimentation is often used as method to solve problems and to innovate (Simon, 1969 ; Thomke, 2003 ; Thomke, von Hippel, & Franke, 1998). Innovative teams often organize different types of experimentations such as prototyping or focus group in order to progressively converge toward new solutions. Despite the importance of experimentation for firms, the empirical studies that provide managerial recommendations are still scarce. One major exception is the work provided by Prof. Stefan Thomke. Entitled 'Experimentation matters : unlocking the potential of new technologies for innovation', the landmark contribution of Thomke (2003) received a warm welcome in the innovation management community (Ball, 2004 ; Seidel, 2004 ; Smith, 2004). This book provides an excellent overview of the organizational issues and the managerial principles for experimenting.

The objective of this research is to investigate the validity of the Thomke's principles in the case of experimentation in the unknown. We define the term 'experimentation in the unknown' as the experimentation conducted in order to create not-yet-invented products while dealing with incomplete knowledge or profound ignorance. Despite the insightful Thomke's works, companies still encounter great difficulties for managing experimentations in such extreme situation. Furthermore, several theorists have casted doubts on the relevance of trial-and-learning experimentations in situation of high uncertainty (Garud & van de ven, 1992 ; Lynn, Morone & Paulson, 1996 ; Sommer, Loch & Dong, 2009 ; Van de Ven & Polley, 1992). Contrary to standard experimentations, one critical aspect of conducting experimentations in the unknown resides in the fact that the different problems to solve, the set of alternatives to test and the evaluation criteria are not pre-given (Boland & Collopy, 2004; Garud, Jain, & Tuertscher, 2008 ; Garud & Karunakaran, 2012 ; Hatchuel, 2001). The appropriate organizational principles that could offer both control and creativity remain obscure.

Our two main research questions are thus the following : Are the Thomke's principles consistent for conducting experimentations in the unknown ? Are they sufficient ? The paper is organized as follow. Section 1 summaries the five organizational principles proposed by Thomke and their echoes with the innovation management literature. The section 2 presents our research methodology in term of data collection and analysis. This explorative research is based on the historical analysis of the experimentations that leads to the development of a world-changing innovation during the Second World War : the atomic bomb created by the Manhattan Project. The section 3 describes emblematic examples of experimentations carried out by the Manhattan Project. In the section 4, the Thomke's principles are systematically compared with our case study. Experimentation is of paramount importance in innovation. Our findings show that the several principles proposed by Thomke are not sufficient and even ill adapted to the specificities of the unknown (eg. profound ignorance, crisis of scientific instruments…). Finally, new directions of research are proposed.

II. REVIEW OF THE THOMKE'S PRINCIPLES FOR EXPERIMENTING

Thomke represents the experimentation process as a model comprising four main iterative steps (see fig 1) : a 'design' step for conceiving the key concepts and ideas to experiment, a 'building' steps during which the research protocol, the models or prototypes to be used are prepared, the strictly speaking 'running' steps and the 'analysis' steps, where the data are observed and the causes-and-effects relationships are scrutinized (Thomke, 2003, p.94). The underlying logic is that of problem-solving, itself in decision theory (Simon, 1969). Based on this four-step model, Thomke discusses at length the fundamental role of experimentation in the innovation process, the organizational problems it raises and the way to improve its performance. His thesis is summarized in the concept of "enlightened experimentation" which comprises five managerial principles[1]. The next sections summaries these five principles and their echoes to the existing literature (see Table 1).

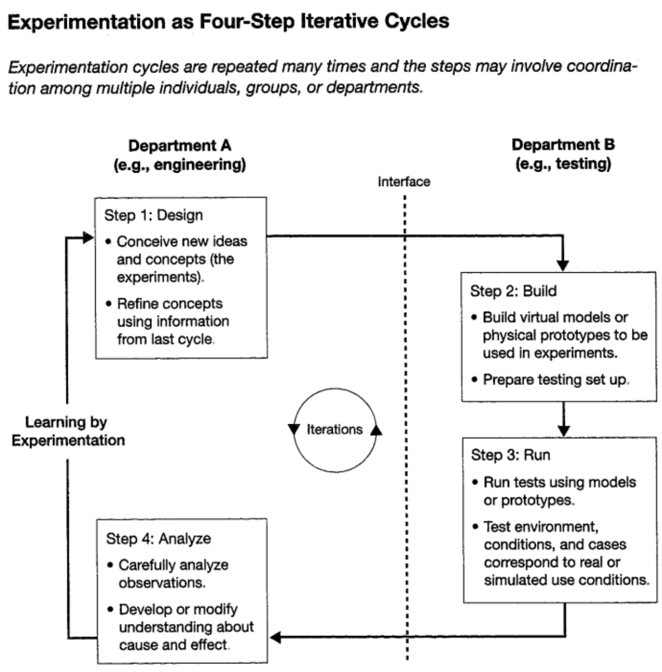

Figure 1 : Thomke's experimentation model (Thomke 2003)

2.1 Principle 1 : Anticipate and exploit early information

With this first principle, the author reminds the importance of doing tests early in the innovation process. Instead of correcting costly and late-stage development problems, early experimentations permit to know what works and what doesn't work in advance (Thomke & Fujimoto, 2000). In particular, he demonstrated how numerical simulation allows to identify

[1] The sixth principle has been pruposely removed from our analysis because it is not direectly related to experimentation projects. This last principle suggests that experiments can also be conducted in other levels of the firms such as the communciation with the customers.

problems early in a project then reducing the cost and accelerating the program. Identifying problems upstream permits to “reduce unwelcome surprises” (Browning & Ramasesh, 2015) such as the allocation of significant resources for impossible solutions or the incompatibilities of solutions. This principle is in line with an important body of research regarding the concept of “front-loading” (Thomke & Fujimoto, 2000) or the “frond end of innovation” (Khurana & Rosenthal, 1998 ; Koen et al., 2001 ; Reid & de Brentani, 2004) in new product development. Transferring knowledge from past projects is one technique to anticipate critical problems (Tuna & Windisch, 2014). However, for very radical projects, the information is not easy, or even impossible, to anticipate. In order to uncover known unknowns (ie. “things that we now know we don't know”), some authors propose to decompose the different systems of projects in several parts (Feduzi & Runde, 2014 ; Loch, Solt, & Bailey, 2007 ; Ramasesh & Browning, 2014).

2.2 Principle 2 : Experiment frequently but do not overload your organization

This second principle tempers the first one. If early experimentations are crucial, Thomke admits that too many experiments may also provoke important issues for the firms. Thus, the experimentations must not exceed the firms’ learning capacities. Such capacity depends on several factors such as the firms’ capacity in term of resources, the cost of experiments or the iteration time between experiments. Interestingly, Thomke proposed a rule of thumb solution, in which the number of experiments is related to the square root of their cost (see (Thomke & Bell, 2001) for further details). This principle refers to different theories such as the queuing theory, which stated that, for a given capacity, one has to carefully manage the number of experiment in order to avoid costly delays. Furthermore, with this principle, Thomke recalled the usual problems of organizational barriers, he emphasized on setting up projects teams that could control the entire experimental process.

2.3. Principle 3 : Combine new and traditional technologies

Even if new technologies such as 3D computer simulations are very efficient technologies to lead experimentations, the third principle reminded us that conducting experiments with new technologies is not always the best choice. Rather, his recommendation is to use traditional and new technologies in concert. If new technologies may provide quicker results than traditional ones, the fidelity of experiments, it means the degree to which the model under test is representative to the real conditions, can sometimes be worst than traditional technologies. Furthermore, a recommendation given is to constantly keep a close watch on the next technology generations.

2.4. Principle 4 : Organize for rapid experimentation

The fourth principle insists on the necessity of doing quick experimentations. The rational behind the principle is that rapid experimentation permits to gain fast feedback, which permit to redirect the projects. This principle is consistent with the large body of literature regarding design thinking that insist on the importance of iteration in innovation process (Brown, 2008 ; Seidel & Fixson, 2013). Especially, rapid prototyping is a particularly relevant method to obtain information between trials. Moreover, the author recommends composing small development groups of people with different background in order to quickly analyze the results of experimentation. Finally, a last recommendation is to experiment several alternatives in parallel rather than sequentially (Dahan & Mendelson, 2001 ; Fredberg, 2007 ; Lenfle & Loch, 2010 ; Loch, Terwiesch, & Thomke, 2001).

2.5. Principle 5 : Fail early, and often, but avoid mistakes

Doing experiments often imply failures. Learning from failures permit to reorient the project. Thomke claimed that some errors are simply caused to a lack of preparation in the experiments. These mistakes can be avoided. He argued that any experimentation should respect the basic of experimentations : the objectives (ie. what do you anticipate learning ?) and the hypothesis (what do you expect to happen ?) must be clearly defined. The control variables must be carefully designed in order to increase the ability to learn from the experiments. In the current literature, the culture of experimentation, the tolerance to risk-tasking is often highlighted (Cannon & Edmondson, 2006 ; Lee, Edmondson, Thomke, & Worline, 2004 ; Maidique & Zirger, 1985). Furthermore, the culture of experimentation is proved to be improved by flexible goals and low-supervision (Lee et al., 2004 ; McGrath, 2001).

Table 1 : Synthesis of Thomke's principles

Principles	Description (adapted from Thomke (2003))
Anticipate and exploit early information.	- Identifying problems upstream, where they are easier and cheaper to solve. - Use low fidelity experiments first and then high-fidelity experiments
Experiment frequently but do not overload your organization	- Plan experimentations in function of your firms ' learning capacities. - The number of experiments is related to the square root of their cost
Combine new and traditional technologies	- Use new and traditional technologies in concert.
Organize for rapid experimentation.	- Examine and, if necessary, revamp entrenched routines, organizational boundaries, and incentives to encourage rapid experimentation. - Use small development groups that contain people with all the knowledge required to iterate rapidly. - Favor parallel experiments than sequential
Fail early and often, but avoid mistakes.	- Embrace early failures and advance knowledge significantly. - Don't forget the basics of experimentation : well-designed tests have clear objectives (what do you anticipate learning ?), hypotheses (what do you expect to happen ?) and variable controls.

III. RESEARCH METHOD : THE HISTORICAL ANALYSIS OF THE MANATHAN PROJECT

3.1. Study of product innovation and scientific discovery : the legitimacy of historical studies

What we are looking for is a better understanding of the management of experimentation in situation of breakthrough innovation. The natural methodology here is that of the longitudinal single case study (Yin, 2002). Finding appropriate cases, however, proves difficult. Most of the time, R&D projects with a high degree of innovativeness, and thus of uncertainty, are considered confidential, and are therefore closed to both quantitative and qualitative outside assessment. One way to overcome this problem is to go back to history. Although such approach is widely adopted by historians and sociologists of technology (Hughes, 1983) or economists (Freeman & Soete, 1997), it is not the case of innovation scholars (a notable exception is (Van-de -Ven, Polley ; Garud, & Venkataraman, 1999). History, however, constitutes a powerful way to test the relevance of existing theory or to generate insights on contemporary questions (Kieser, 1994 ; Siggelkow, 2007). Comparing different research methods to study scientific discovery, concluded that there are not one best way to study innovation. According to authors, studying scientific discovery with history-based research method permit to have high face validity, long and coarse-grained resolution of the data and a high understanding of the social and motivational factors. In our case, there is no doubt that the Manhattan Project's members managed the experimentation processes toward radical innovation.

3.2. Choice of the case: an emblematic case of experimentation in the unknown

The Manhattan Project is an exemplary case of experimentation in the unknown. Indeed the main characteristics of the project are the lack of knowledge on the phenomena's at stakes. As explains by H. Smyth in his famous report of 1945, even if there has been important theoretical development in nuclear physics during the 20's and 30's « *there were many gaps and many inaccuracies. The techniques were difficult and the quantities of materials available were often submicroscopic. Although the fundamental principles were clear, the theory was full of unverified assumptions, and calculations were hard to make. (...) The subject was in all too many respects an art, rather than a science* » (Smyth, 1945 : 365). This has major consequences for the unfolding of the project, the greater being that people were "*proceeding in the dark*" as stated by L. Groves, the Manhattan Project Director (Groves, 1983 : 40). Regarding several aspects of the project, there was no available theory. Experimentation was the only solution, but experimentation without theory. Rhodes presented the project as "*two years and a half of trial and error*" (Rhodes, 2012 : 598). For Hewlett & Anderson : "*Experiments, not theory, had been the keynote at Berkeley. The magnetic shims, sources, and collectors that gave the best results were used, although no one could explain their superiority"* (Hewlett & Anderson, 1990 : 142).

3.3. Data collection

Fortunately, the Manhattan Project has been extensively studied, mainly by historians, and its relevance no longer needs to be proved. We may therefore draw on a large amount of historical material that has so far not been used to study innovation management. Our objective is not to provide a comprehensive account of the Manhattan Project, but to focus on a specific set of events that, we believe, reveal the problems raised by the management of experimentation in projects with unforeseeable uncertainty (we therefore do what Langley (1999) refers to as bracketing events for theoretical purposes). Our data relied on the "official" history of the project (Groves, 1983 ; Hewlett & Anderson, 1990 ; Jones, 1985 ; Rhodes, 2012 ; Smyth, 1945) which provides the background of the case we are studying here. However since we focus here on the design of the implosion bomb, most of our data comes from the history of the Los Alamos Laboratory which was in charge of the design of the bomb (Hawkins, 1961 ; Hoddeson, Henriksen, Meade, & Westfall, 1993 ; MacKenzie & Spinardi, 1995 ; Thorpe & Shapin, 2000). In particular *Critical assembly*, the landmark study of Hoddeson et al. (1993) was an invaluable source of information on the organization and unfolding of the experiments on the implosion bomb.

IV. THE MANHATTAN PROJECT: EXPERIMENTING THE IMPLOSION BOMB

4.1. Overview of the problem

The Implosion bomb is one of the two solutions explored by Los Alamos scientist to design an atomic bomb. First considered as a back-up, it became a priority late in the project (April 1944) once it became clear that the other solution (called a gun design) would not work with plutonium[2], the most promising fissionable material at this date. In its principle the implosion bomb constitutes a complete breakthrough innovation in weapon design. Indeed, conventional explosives are placed around a plutonium core. When they detonate, they blow inward, the core collapse and thus become critical leading to an explosive chain reaction (Figure 2). This design was used in the "Fat man" bomb dropped on Nagasaki on August 9, 1945.

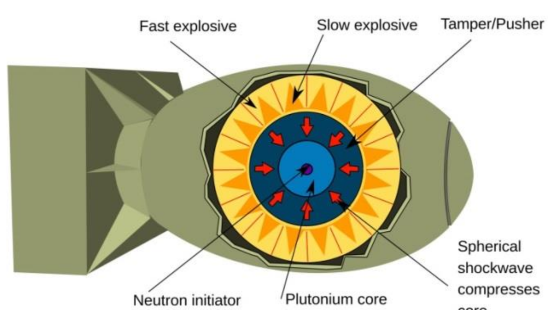

Figure 2. Implosion type fission bomb

[2] This episode is known as the "spontaneous fission crisis" see (Hoddeson, Henriksen, Meade, & Westfall, 1993 : chap.12).

The technical and scientific challenge was absolutely colossal. As explained by Hawkins, "*There was no established art that could be applied even to part of the mechanical problem. In this respect the implosion research differed from the gun research, where many mechanical and engineering features and methods of proof were at least relatively standard*". (Hawkins, 1961, p. 138). Indeed « *implosion moved the Los Alamos scientists onto new terrain. In part, the move was into areas of physics with which they were less familiar: implosion is a problem in hydrodynamics rather than just in nuclear physics* » (MacKenzie & Spinardi, 1995 : 56). Nobody never envisioned this type of ignition mechanism before. This explains why some were doubtful throughout the project on the feasibility of the implosion bomb.

Technically speaking the most difficult problem was the symmetry : to ensure the start of the chain reaction, the inward collapse of the plutonium core should be absolutely symmetric. This had never been done before and explosives were not dedicated to this purpose. As explained by Hoddeson, "*imploding a metal shell symmetrically required precision in the use of high explosives, an almost unexplored concept in 1943*". (p. 88). Generally, since this was a breakthrough innovation, the available knowledge was almost inexistent. Los Alamos had thus to explore simultaneously the hydrodynamics of implosion, the design of explosives 'lens' around the core, the design of the initiator that would release the neutrons necessary to start the chain reaction (see figure 2), the electronics to coordinate the detonators around the bomb, and so on… and to keep in mind that they must design a practical weapons[3]. Here again the lack of theory made experimentation the only available strategy.

4.2. Exploring implosion: first tests

It is impossible to describe here the entire implosion program[4]. We will focus on the exploration of the functioning of implosion itself i.e. what happens when one tries to blow a solid material inward with explosives. This was the topic of the first experiments organized by Seth Neddermeyer, the first leader of the implosion program. The problem was awfully complex. As explained by Hawkins, "*the difficulties lay principally in the necessity for recording events inside an explosive and for timing these events within an uncertainty of the order of 1 microsecond*". (Hawkins, 1961 : 140). So they started with "*remarkably crude*" experiments in july 1943 consisting of "*using tamped TNT surrounding hollow steel cylinders*" (Hoddeson et al., 1993 : 88) and analyzing the bashed-in pipes with different configurations of the parameters (figure 3). This approach, known as the "termination method", allows them to get a first very crude understanding of the processes at stakes.

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[3] In this perspective one of the first design decision was to define the size and weight of the bomb so that it could fit in a B29.

[4] A detailed account of the entire implosion program is found in Hoddeson & al. 1993, mainly in chapters 8, 9, 14, 15 et 16.

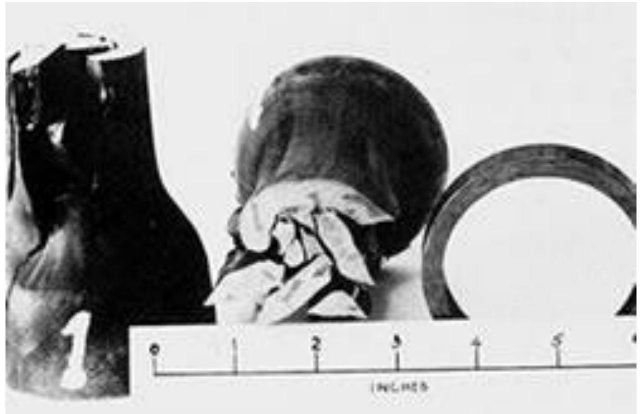

Figure 3 : first implosion experiments at Los Alamos, august 1943 (from Hoddeson, p. 89)

These first tests allows them to "*develop an approximate one-dimensional theoretical model for the implosion process*" (p. 89) but also revealed the complexity of the processes at stakes and exposed "*many of the difficulties that would plague the Los Alamos Laboratory over the next two years*" (ibid). In particular, the experimentations showed that "*simple implosion shots were too asymmetrical to release the nuclear energy required for a usable weapons*" (ibid, p. 130).

The first breakthrough in the implosion program occurs in September 1943 with a visit of the famous mathematician John Von Neumann. He had previously worked on shock waves and fluid dynamics on high explosives. Learning about the early experiments, he suggest to "*initiate implosion by arranging shaped charges in spherical configuration around the active material (…) and to achieve a faster kind of implosion assembly based on increasing the amount of high explosives*" (Hoddeson et al., 1993 : 131). This result in a considerable expansion of the implosion program. Given the lack of knowledge on the processes at stakes, theoretical and experimental work proceeds simultaneously.

4.3. Exploring the implosion : the intertwining of theory and experiments

On the theoretical side a new group dedicated to theoretical study of implosion was set up in early January 1944 under the direction of E. Teller. They focused immediately on the formal analysis of the hydrodynamics of implosion for example to calculate "*the time of assembly for large amount of explosives*" (p. 158). However they were soon confronted to a major difficulty i.e. the limit of their computing power given the complexity of the phenomena at stakes. Indeed "*a major problem was that the partial-differential hydrodynamic equations employing realistic equations of state applicable to high temperatures and pressures were insoluble by hand computations*" (p. 159). This explains why Los Alamos, to overcome this bottleneck, pioneered the use of computers in the form of 10 IBM punch cards machines received in april 1944 which, with their associated operators (mainly women), allowed them to realize extremely complex calculations (Metropolis & Nelson, 1982). These theoretical studies would have a major importance in the implosion program even if they did not provide complete explanations. As explained by Hoddeson, "*in theoretical study numerical analysis and iteration often replace full analytical solutions because the basics of shock hydrodynamics were not yet known. Nor were enough data available to develop detailed quantitative treatments. In 1944 there existed analytic solutions for shocks in perfect gases, but few* *understood shocks in other materials, and there were no data for nuclear materials, particularly in the temperature-pressure-energy regions of interest surrounding implosion*" (p.294). Nevertheless theoretical studies provided immediately important insights to organize the experiments. The calculations "*reveal new quantitative and qualitative details of how implosion would occur*" (p. 161) For example, as soon as the new theoretical group was set up, E. Teller "*sent ten detailed suggestions for cylindrical test shots : (1) use long enough cylinders so as to make end effects negligible in the interior ; (2)* [etc]…" (Hoddeson et al., 1993 : 158). And in return, "*the initial experimental work on the compressibility of various fissionable materials, supplied the data needed to calculate theoretical equations of state*", p.158-159. This explains that the theoretical and experimental groups worked in close interactions : from the beginning of the project Hans Bethe[5], a leading theoreticians at Los Alamos, "*had insisted that members of the T-Division work with experimental groups. In January 1945, Bethe formalized the policy by assigning theorists to specific experimental groups*" (p. 308).

This explains why experimental work was of the utmost importance. Following Von Neuman visit, two new divisions were created that borrowed people from the previous divisions[6] :

- The Gadget (G) Division, lead by R. Bacher, was to investigate implosion experimentally and eventually design a bomb.
- The Explosives (X) Division lead by G. Kistiakowsky, specially hired to lead the implosion program, was devoted to design the high explosives components of the implosion bomb and develop methods of detonating them.

"*The primary concern during the first half of 1944 was to devise diagnostics to measure implosion parameters such as symmetry, the time of collapse and the degree of compression*" (p. 130). Even if experiments were conducted on all the components of the implosion bomb (ex : shaped charges, detonators, initiator, etc), we will focus our attention on the experimental techniques developed to study the crucial parameters described above. As explained previously the team was confronted to the unheard of problem of recording the extremely fast phenomena that happens during the implosion of a cylinder (or, latter, a sphere) with explosives. Given the lack of knowledge on this question the lab used multiples and overlapping approaches to enrich their understanding of the phenomena at work, increase the likelihood of success and save time. More precisely the team use 7 different approaches to study implosion (see fig 4 below).

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[5] 1967 Noel Prize in physics.
[6] Details are available in Hawkins, 1961 chap. 9 and Hoddeson & al. 1993, chap. 14.

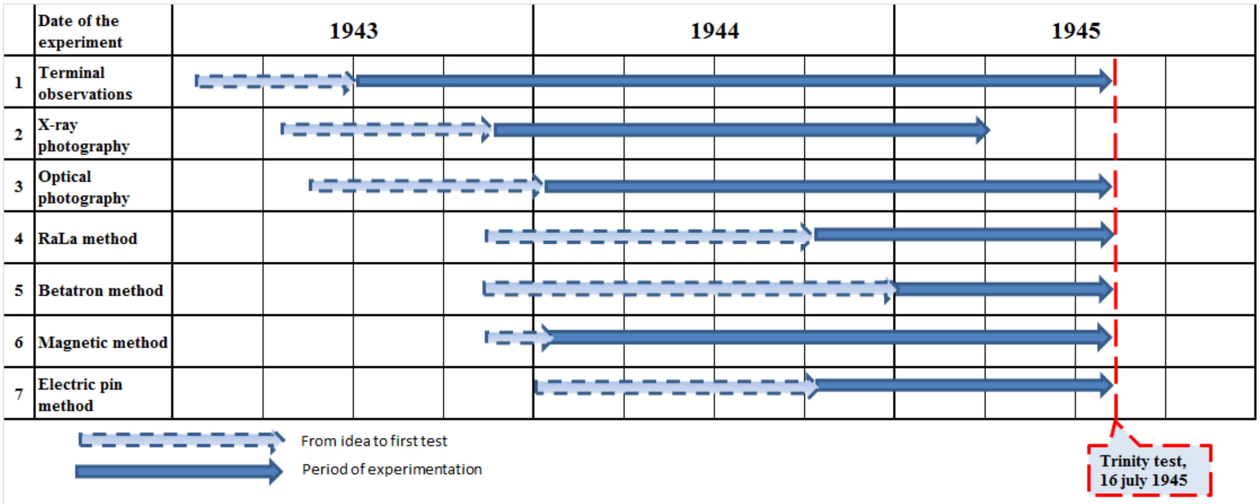

Figure 4 : the 7 diagnostic methods of the implosion program (from Hoddeson & al, 1993)

The first three experiments were extension of known techniques. For example, in the X-ray method "*single X-ray photographs of a sequence of identical shots, each on taken at a later moment in the shot, produced a "movie" of the implosion, which indicated both the symmetry and compression*" (p. 141 and figure 5 below). However X-ray test proved at the same time extremely valuable to the program, by revealing fundamental phenomena (i.e. "jets" formation during implosion), and a very complex endeavour. Indeed, implosion raised huge and completely new constraints. The main problem was to correlate as precisely as possible the Y-ray flashes with the shots. This explains why, as elsewhere in the program, the X-ray experiments, were continuously changing to improve this timing problem: new machines were added, new analytical devices were invented and new people hired to solve the unanticipated problems. Moreover the difficulties of organizing the tests were compounded by constant "*difficult shop and procurement situation*" and the "*haste of the program*" (p. 143).

Figure 5. Flash X-ray images of the shock waves

The growing complexity of the problem and the overwhelming difficulty to understand the processes at stakes in implosion leads the experimental group to constantly add and invent new experiments. Therefore, in the second half of 1944, they invent new experiments (4 to 7 in figure

4). To understand the problems raised by experimentation on implosion we will describe here the RaLa method and its contribution[7].

4.3. Experimenting in the unknown: the RaLa method

4.3.1. Principle

The RaLa method was invented in November 1943 by Robert Serber. This novel procedure for diagnosing implosion was "*based on placing a γ-ray source at the center of a spherical assembly. The emitted γ-ray would travel outward radially through both the collapsing shell and high explosives. Because increasing compression of the metal caused the γ-rays to be increasingly absorbed, the emerging γ-rays, monitored by detectors set around the high explosives, would provide information on density changes in the collapsing sphere of metal. The data would indicate the time of collapse, the degree of compression, and the symmetry by comparing γ intensity in different directions*" (p. 148 and figure 6 below). Compared to the other diagnostic methods, the strength of the RaLa approach was to "*report on the implosion parameters throughout the assembly"* and therefore to "*yield a continuous report of the progression of the implosion*" (ibid.).

Figure 6 : RaLa experiment at Los Alamos

A RaLa experiment at Los Alamos in 1947 (left from https://en.wikipedia.org/wiki/RaLa_Experiment) and remote handling of RadioLanthanum at Los Alamos during the war (right from Hoddeson & al. p. 270)

4.3.2. Designing and building the experiment

But this was mainly theoretical in November 1943 since the RaLa method "*meant the development of unprecedented performance with ionization chambers as well as unheard of sources of gamma radiation*" (Hawkins, 1961 : 234). The first problem was to find the better source of γ-rays and to produce it in sufficient quantity for the experiments. RadioLantanum-140 proved to be a good candidate (thus the "RaLa" method) but its procurement from the new Oak Ridge plant in Tennessee would remain an important problem throughout the program. To have an idea of the magnitude of the effort one has to understand that, "*to fulfill the Los Alamos request for radiolanthanum, Oak Ridge would have to construct a special extraction laboratory and a second plant for dissolving the radioactive irradiated uranium slugs and recovering barium from the solution. The*

[7] The reader interested by the other methods could refer to Hawkins (1946) and Hoddeson & al (1993) chapters 8, 9 & 14.

material would have to be shielded in lead during its transport across 1200 miles to Los Alamos, in special trucks driven 24 hours a day" (p. 150). Moreover, the demand of Los Alamos of a powerful emission source was unprecedented. "*No one ever worked with radiation levels like these before, ever, anywhere in the world*" said Rod Spence, a chemist involved in the program (ibid.).

Then comes the design of improved ionization chambers with adequate time resolution to analyze the emission of *γ-rays,* the design of the recording systems, the preparation of the test site, shelters to handle radioactive materials, etc. This explains why the RaLa experimental program was "*a prime example of multidisciplinary research at Los Alamos*"(Hoddeson, p. 151). It involves chemist, metallurgists, machinists, theorists, electronics engineers, explosive specialists and logisticians. To manage this complex endeavor Luis Alvarez[8] was named head of the RaLa program. By late spring 1944 the design of the experiments becomes more precise and most of the instrumentation was completed.

To understand the problem at stakes it is useful to describe the experiment. Indeed, the team "*planned to surround high explosives with several set of ionization chambers, typically four ; just before being destroyed by the explosion, the chambers would register the signals and send them through amplifiers to oscilloscopes set in a bomb-proof shelter. Two calibration circuits, one used before firing and the other after, would be placed in an underground "firing pit", located 15 feet from the firing point. The chambers were simple enough in design to be produced quickly and in sufficient quantities, even by the already overburdened machine shops*". (p. 151-152).

4.3.3. Running the RaLa experiments

However the difficulties were so complex (e.g. impurities in the first shipment of radiolanthanum in june) that the first RaLa test was fired only of 22 september 1944. It "*confirmed that the ionization chambers were performing well and also provided time of collapse. But the source was too weak to provide conclusive information on the symmetry and compression*" (Hoddeson, p. 269). This "*midly encouraging results*" (ibid.) leads to recommandations of theorist on the design of the test. A second shot was fired on octobre 4. It was "*similar to the first but used a much stronger source* [of *γ-rays*]". (…) The third shot was fired on 14 october with *"a smaller quantity of RaLa. Neither shot showed evidence of compression, but in making these shots the group learned how to carry out the experiment"* (ibid.) These first results lead the RaLa team to change the different components of the experiments i.e. "*the nature of high-explosives, (...) the mode of initiation, the design of the sphere containing the RaLa [which change from a hollow to a solid core], the circuits for the investigation of the simultaneity of the detonation, the detonators*", etc. In early December the results of the first shot using a solid sphere "*were erratic*" (p. 271). But the next shot fired on December 14 "*was a milestone, for it showed definite evidence of compression"* (ibid, p. 271). Therefore "*slowly but systematically, the RaLa program contributed valuable information about implosion as the strengths of radiation, materials, and nature of explosives were varied*" (p. 269) This also stimulates theoretical studies on the processes at stakes and leads Hans Bethe to suggest modifications to the experiments. In February 1945 the first shots using electric detonators were fired. "*They showed a great improvement in quality and caused a definite if modest turn in the implosion program. By providing the first observations of sizable compression, they confirmed significant improvement was possible when*

[8] Later Noble Prize in physics, 1968. He was later replaced by the Italian physicist Bruno Rossi.

electric detonators were used. After these shots, electric detonators were used in RaLa tests and the results essentially settled the design for the Trinity gadget[9]*"* (p. 271). However this does not mean that the RaLa test becomes routine. Indeed "*the month of march 1945 saw little RaLa testing because of an insufficient supply of radiolanthanum*" (p. 326) Some test (march 3) were inconclusive because for example the weakness of the *γ-rays* source. Other, with explosive "lenses" looks promising but the data were destroyed by "*electromagnetic disturbances*" (april 4 test in Hoddeson, p. 326). However "*subsequent shots show definite compression, a density increase and reasonable acceleration*" (p. 326). Finally "*although the accuracy of RaLa work was still improving (...), by june the main pre-Trinity task of the RaLa experiment – to aid in settling the design for the Trinity gadget – had been accomplished, and the prominent members of the group had turned their attention to Trinity work"*(p. 326).

4.4. Concluding remarks

This summary of the RaLa experiment demonstrates the complexity of experimenting to understand unknown processes with new to the world experimental devices. Indeed, as explained by Hoddeson "*the history of the seven-pronged experimental program to study implosion (...) is one of painstaking progress with few highlights or definitive measurements, many ambiguous steps, and numerous failures*" (p. 268). This is due to the lack of knowledge on implosion and on the performance of the experiment. She continues by stating that "*informed trial and error was the approach most frequently taken in these simultaneous lines of experimental inquiry, since theoretical understanding was still incomplete. While each program offered its particular advantages, many efforts overlapped, adding modest confirmation to the amassing body of understanding. Despite lingering uncertainties about the feasibility of an implosion weapon, the 6 month following the august 1944 reorganization saw the central research question of the lab shift from "can implosion be built ?" to "how can implosion weapon be made*?" (p. 267).

5. RESULTS: COMPARING THE THOMKE'S PRINCIPLES AND THE MANHATTAN PROJECT

The case of the implosion weapon helps us to understand the specificity of experimenting in the unknown. As we will demonstrate in this section it draws a picture of the experimentation process that is quite different from Thomke's model. In this section we propose to underline its mains features and discuss how they differ from Thomke's principles (see Table 2).

The first, and probably most important, characteristic of the case is the almost complete lack of knowledge on the processes at stakes. Nobody ever envisioned an implosion weapon. Therefore there was no theory on how it works. As explained below, "*no established art that could be applied even to part of the mechanical problem"* and the scientist and engineers were confronted to completely new question, typically the hydrodynamics of implosion. In this situation the role of experimentation is simply to test things and see what happens. This was exactly the goal of the crude tests organized by Neddermeyer in 1943 : blow cylinders with explosives and look at the result to see what happens, if the inward collapse is symmetrical or not… and try again. These tests were not designed to discriminate between hypothesis, but to get a first understanding of the

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[9] The name given to the first implosion bomb test in the new mexico desert on july 16 1945.

basic processes at stakes. What is fascinating here is the complete intertwining of theory and experiments. The situation is not one in which theory generate hypothesis that have to be tested. There is simply no theory to generate hypothesis and test them. The process is completely simultaneous : looking at the "results" of the first crude experiments generates some hypothesis that leads simultaneously to modify the test, to the formulation of theoretical recommendations (e.g. use of large quantities of high explosives) and to create new experiments. This is the constant interaction of theory and experiments that progressively leads to a better understanding of the processes at stakes and to a bomb design. But it is almost impossible to identify which one comes first. The contrast is striking with Thomke's first principle that emphasizes the need to "anticipate and exploit early information". Indeed in situation of exploration there is nothing to anticipate. The problem is just to (try to) understand what happens. Therefore the notion of "late or early experiments", perfectly understandable in the context of a development project, has literally no meaning here. The only solution is to experiment. Note that this is coherent with theory of innovation. Van de Ven & al (1999) in particular explains that experiments are the only solution when there is no a priori knowledge on a field. This process of "learning by discovery" allows to progressively understanding the nature of the problem and the potential solutions.

The second prominent characteristic of the case is the absence of a pre-existing organization. The Los Alamos laboratory was built on a desert New Mexico mesa acquired by the Army in February 1943. It literally started from scratch and becomes a huge scientific-industrial complex in less than three years. As explained by Thorpe & Shapin (2000) : « *Construction on the project began in january 1943 with approximately 1500 workers. Scientific personnel began to arrive on a permanent basis in march 1943 and, by the end of the year, the population has reached an estimated 3500. This rose to 5765 by december 1944, and by june 1945 the total population has reached a wartime peak of 8750. These figures stand in marked contrast with early projections, by Oppenheimer and his fellow scientists, about the likely scale of the project. Oppenheimer original guess was that perhaps as few as six scientists (or, with support personnel, several hundreds) might do the job. (…) Both the laboratory and the post expanded beyond all expectations as new configurations of scientific knowledge, technological activity and organizational form were thrown up as emergent properties of the work* ». (p. 556). The last sentence is of particular significance for our research. It demonstrates that the question was not to avoid overload, as suggested by Thomke's second principle, but rather to manage the exponential growth of a program that change as new problems appears, generally following (new) experiments. The implosion program was at the center of this evolution since, as we have seen in the case, it leads to the creation of two new division ("G" and "X") to tackle the challenge. Moreover we can see that each new experiment leads to the creation of a new multidisciplinary group. This is evident in the organization of the G-Division in august1944 (figure 7). This does not means that there was no overload at Los Almos, quite the contrary. However, given the unlimited resources, this was not the central problem. Much more difficult was the constant evolution of a "*structure*" that was "*by nature ephemeral*" (Thorpe & Shapin, 2000). The central challenge here was to manage the portfolio of experiments and the amazingly rapid evolution of theory and engineering. Making sense of the situation is exceedingly difficult here. We will come back on this point later.

G-1	Critical Assemblies	O. R. Frisch
G-2	The X-Ray Method	L. W. Parratt
G-3	The Magnetic Method	E. W. McMillan
G-4	Electronics	W. A. Higginbotham
G-5	The Betatron Method	S. H. Neddermeyer
G-6	The RaLa Method	B. Rossi
G-7	Electric Detonators	L. W. Alvarez
G-8	The Electric Method	D. K. Froman
G-9	(Absorbed in Group G-1)	
G-10	Initiator Group	C. L. Critchfield
G-11	Optics	J. E. Mack

Figure 7 : organization of the G-division at Los Alamos, august 1944

The third interesting feature of the case is the lack of technology to experiment. As illustrated in the previous section, the experimentations on implosion that occurred at Los Alamos mobilized several technologies. However, the most important challenges for the experimenters were not to scan the available experimental instruments and to make a choice between traditional or new technologies as suggested by Thomke's third principle. Indeed, a large number of the instruments simply did not exist. Both the instruments to test and to analyze the results were unavailable. The design of implosion bomb induces to make experimentations on the scientific instruments themselves, either to modify existing technologies (typically on X-ary and optical photography) or to create entirely new ones (eg. RaLa method). Again, our case study shows that experimenting in the unknown is not a question of choice but of co-creation of the experiments and the scientific instruments. This has obviously major consequences since it introduces new uncertainties in the experimentation process. Experimenting unknown process with new to the world experiment means that uncertainties are everywhere, in the phenomena at stakes, in the instruments to test and in the instruments to analyze the results. This considerably amplifies the challenge for the experimenters since it multiply the parameters, the source of "error", etc. This explains their frequent perplexity with the results of the test. As seen below in the RaLa experiment then first shot were "*midly encouraging"* and "*neither shot showed evidence of compression, but in making these shots the group learned how to carry out the experiment"* (ibid.). Learning here relates to the phenomena, the instruments and how to carry the experiments.

This leads us to the fourth prominent characteristics of the case that directly questions Thomke's fifth principle, "fail early and often but avoid mistakes". This is probably where Thomke's theory of experimentation is most influenced by the context in which it has been developed. His insistence on the need to "avoid mistakes" is symptomatic of this. He wrote : *"failures should not be confused with mistakes. Mistakes refer to the wrong actions that result from poor judgment or inattention ; they should be avoided because they produce little new or useful information and are therefore without value. A poorly planned or badly conducted experiment that result in ambiguous data, forcing researcher to repeat the experiment, is a mistake. Another common mistake is repeating a prior failure or learning nothing from the experience"(p. 213*). Our hypothesis is that the level of prior knowledge must be very high to avoid mistakes. It implicitly implies a tight control of the experimentation process. Indeed, what happens if this prior knowledge is weak or non-existent ? The case of the implosion weapon is revealing here. The problem was to analyze unknown processes with new, unproven, experimental technology (typically the RaLa method). Therefore controlling the conditions of experiments to avoid "mistakes" seems almost impossible. Was it a mistakes to test with a low source of energy (first shot, September 1944) ? Or to have the data destroyed by unanticipated magnetic disturbances (april 1945) ? Who knows ? They were in a situation in which they have to design / test / analyze as the same time as they were learning how to do it, the problem it raises, etc. Therefore the frontier between "failure" and "mistakes" becomes very fuzzy. It was not unusual on the Manhattan project to be confronted to situations where people were unable to understand what happens. "*Learning nothing from the experience*", a problem for Thomke, is classic in exploration. The archetype of this happens in the production of plutonium where the different groups working on a critical problem[10] "*seemed to be learning more and more about less and less. They were amassing data but not developing a process*" (Hewlett & Anderson, 1962, p. 224). Indeed the process was excessively complex, comprising a huge number of steps and parameters each of which susceptible to cause failures. In exploration understanding what is a failure of a well-designed test and what is a mistake represent two faces of the same coin.

Table 2 : Thomke's principles comparisons

Thomke's Principles	**Experimenting in the unknown : the implosion case**
Anticipate and exploit early information.	Lack of knowledge on the phenomena at stakes. Experimenting to create new knowledge Intertwining of theory and experiment Anticipation is not the problem
Experiment frequently but do not overload your organization	No pre-existing organization Competences not available Exponential growth of the lab Coevolution of experiments and organization Managing an ephemeral structure
Combine new and traditional technologies	No available technology to experiment Creation of new instruments to run the experiment and analyze the results
Organize for rapid experimentation.	This was the essence of Los Alamos. Multidisciplinary teams were created throughout the project to handle the different experiments
Fail early and often, but avoid mistakes.	Given the lack of knowledge and the novelty of the experiments it is excessively difficult to discriminate between failures and mistakes.

6. CONTRIBUTION AND PERSPECTIVE OF RESEARCH

6.1. Clarifying the boundary conditions of Thomke's principles : 'completeness'

The analysis of the case study leads us to unveil the boundary conditions of Thomke's model and his principles. According to our analysis, Thomke's experimentation principles suit very well the experimentations that hold in conditions of 'completeness' (Garud et al., 2008). Garud et al. explained that "*Completeness allows for the pre-specification of a problem, the identification of pre-*

[10] On this "slug crisis" see Hewlett & Anderson, 1962.

existing alternatives and the choice of the most optimal solution" (p.351). In Thomke's works, this condition of completeness is, for instance, observable during the first design step "*during which individuals and teams define what they expect from the experiment based on their prior knowledge*. (p. 95). It is also observable when Thomke emphasized on the importance to use numerical simulation techniques such as crash test in automotive industry. Indeed, doing car crash tests require both a stable product architecture and high prior knowledge. Theory should exist to make prediction such as "if we use steel A and form B in design C then X should occurs"[11]. This condition of completeness is also visible in his third principle concerning the recommendations that the experimentations should not exceed the organizational capacities. Here again, this statement holds in situation of completeness, the organization is assumed to already exist and structured by departments. Conceptually, Thomke falls in the scope of a positivist conception of experimentation in which theory comes first and the main role of experimentation is to discriminate between true or false hypothesis.

On the contrary, this research shows that the experimentations organized during the Manhattan Project is much better symbolized by a condition of "incompleteness" (Garud et al., 2008). As stated by Van de Ven & al. (1999), the experimentations were much more a process of discovery rather than test. The experimentations occurred while the problems were not pre-identified, the alternatives were unknown and the organizations did not pre-exist.

6.2. Making sense of the experiments

This incompleteness is well illustrated by the overlapping of the different experiments. The lack of knowledge leads to the impossibility to associate one experience to the study of a particular phenomenon. This involves a fundamental shift in the management of experiments. The problem here is not to control the unfolding of carefully designed experiments, but rather to make sense of the "*amassing body of understanding*" (Hoddeson & al, p. 267). Managing in real-time the portfolio of experiments constitutes a huge challenge. What is striking in the implosion case is that no single experiments provide THE solution to the problem. Rather it's the multiplication of partially overlapping, "in-progress" experiments and the theoretical work that progressively helps to understand the phenomena at stakes, identify problems and solutions. At Los Alamos this sensemaking process was managed through the strong leadership of Oppenheimer (Thorpe & Shapin, 2000) and the continuous interaction between people in charge of the different experiments and experimenters and theoreticians (see section 4 above). For example the colloquium, a weekly plenary meeting of all the main actors constitutes a powerful means to help laboratory members understand the meaning of their work, and set the direction and pace of action. As explained by Thorpe and Shapin (16, p. 570-572), "*Los Alamos scientists were, almost without exception, highly concerned that each should have an overall sense of how their specialized work fitted into the specialized work being done by others, and into the instrumental goals of the laboratory as a whole. Information, they reckoned, should circulate throughout the laboratory as efficiently as practicable. (…) The solution was simply to provide for more face-to-face and free interaction, to encourage meetings and discussions at as many levels as possible and among as many specialized work groups as possible. This is how and why the weekly Colloquium for all staff members assumed such importance. The Colloquium was considered important as a means of disseminating information, and also*

[11] We use here the case of crash simulation but we find a similar logic in the other cases studied by Thomke.

as a way of creating solidarity and face-to-face accountability". This kind of organizational device helps them to manage the portfolio of experiments and to constantly change them according to the available "results". This is well illustrated by the decision to combine the different experiments of the implosion program. Thus, in in mid-july 1944, G. Kistiakwoski "*pointed out that the magnetic method was admirably suited to be used in conjunction with other tests, which (as X-rays) provide good information on the symmetry at the particular instant of the process, but are very laborious when used to determine all stages of collapse.*[therefore] *they planned to install magnetic method equipment in large-scale X-ray and RaLa tests*" (Hoddeson, p. 156).

6.3. Research avenues: from reducing the unknown to design the unknown

Interestingly, this research invites us to fundamentally change our point of view on the notion of 'unknown' in management. Since the last decade, an important amount of research in project management has been done to integrate the unknown in risk analysis. In particular, several frameworks have been developed to enable managers to be better aware of possible occurences of high uncertainties (Feduzi & Runde, 2014 ; Loch, DeMeyer, & Pich, 2006 ; Ramasesh & Browning, 2014). In such literature, the unknown is often symbolized by exogenous and unforeseen events. Such external events are said to be of negative consequences on the course of the projects. In this perspective, it is not surprising that the unknown is seen as highly undesirable. To come back to our questions of experimentations, Thomke's principles provide brilliant examples of how management can permit to reduce the unknown as much as possible. Interestingly, our case study provides another conception of the unknown: the actors deliberatively elaborate the unknown. Indeed, they proposed new 'things' that did not exist yet. For instance, they desired to invent 'the most powerful weapon that was never designed in the world'. Literally, this proposition was an 'unknown' in itself: the actors did not know how to design such object, and they even learnt things that *"they did not know they did not know"* (Rumsfeld, February 2002). Here, the unknown is no more view as an "unwelcome surprise" to avoid (Browning & Ramasesh, 2015). On the contrary, the experimenters are actively engaged in the design of the unknown (Garud et al., 2008 ; Le Masson, Weil, & Hatchuel, 2010).

Despite its explorative nature, this research offers some promising research directions in order to change our point of view on the notion of 'unknown' in management science. In particular, further research should be done to better the roles played by the different actors when exploring the unknown. One important avenue of research is about how actors can "share" the unknown to design. Indeed, our case study shows an exemplary case of successful collaboration. The Manhattan Project was neither a basic research program nor an applied R&D project. It was a collaborative project between scientists and engineers. On this aspect, further research should be done to understand differences and complementarities between science and design. As claimed by Hatchuel et al. (2013), our analysis reveals that the 'unknown' attracts the scientists and the designers differently. For the scientists, the unknown is considered as an enigma. The implosion type fission bomb was not interpretable with their current theoretical knowledge. Regarding the engineers, the unknown seems to be much more evoked than observed. They imagined and described artifacts that did not exist yet. Further research should be done to better understand how these different logics could be employed in same projects. Our case seems to offer first evidences that the "unknown" mobilized by the scientists and the engineers can have an entraining mechanic. On the one hand, the formulations of unknown 'objects' by engineers motivate the scientists to clarify the validity and the critical elements of their current theories and knowledge. On the other hand, this helps engineers in their design process of the atomic bomb. Finally, further research should be done to better understand what are the boundary objects that are manipulated by the actors when exploring the unknown (Star & Griesemer, 1989). Here, and in other context of radical innovation[12], the scientific instrumentations seem to play a critical role. Exploring the unknown seems to challenge the available instruments of measurement and test.

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[12] Another intriguing example comes form the creation of the first weather balloon. The first empirical observation of the stratosphere in 1893 was first considered as an error of scientific instrument. The discovery was only made official in 1902 by Teisserenc de Bort (Rochas, 2002).

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